THE VIENNA CIRCLE'S DEBATE ABOUT PROTOCOL SENTENCES
REVISITED:
TOWARDS THE RECONSTRUCTION OF OTTO NEURATH'S
EPISTEMOLOGY

by

THOMAS ERNST UEBEL

B.A. Philosophy, University College, London
(1979)

SUBMITTED TO THE DEPARTMENT OF
LINGUISTICS AND PHILOSOPHY
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

at the
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
April, 1989

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Signature of Author

Department of Linguistics and Philosophy
April, 1989

Certified by

Sylvain Bromberger
Professor of Philosophy
Thesis Supervisor

Accepted by

George Boolos, Chair
Professor of Philosophy
Committee on Graduate Students in Philosophy
Department of Linguistics and Philosophy
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Submitted to the Department of Linguistics and Philosophy on April 12, 1989 in partial fulfillment of the requirements for the Degree of Doctor of Philosophy in Philosophy

ABSTRACT

This dissertation concerns the Vienna Circle's philosophies of empirical science. I distinguish and reconstruct the different theories of empirical justification endorsed in the Circle's so-called "protocol sentence debate". My focus lies on Otto Neurath's contribution to this debate, my aim is to lay the foundation for a comprehensive interpretation of his unduly neglected epistemology. I put forward a reading of the debate and Neurath's contribution which radically challenges the Received View—a composite picture of traditional opinion of the Vienna Circle—about the debate in general and Neurath's contribution in particular.

My first main point is that the Received View misrepresents the scope (issues), the structure (players), and the timing of the debate. According to the Received View, the debate concerned the status of the evidential statements of science and whether they referred to physical objects (physicalism) or sense experiences (phenomenalism) and was conducted mainly between Neurath and Carnap as "physicalists" and Schlick as the "phenomenalist" for perhaps two years up to 1934. Close attention to their writings shows, however, that, first of all, each of Neurath, Carnap, and Schlick, argued against both of the others, secondly, that the debate began in immediate response to the publication of the Aufbau in 1928 and continued long into the 1930's in four stages, and, thirdly, that it is false that the disputants agreed on everything but the form, content and status of these protocol sentences at any point. The debate concerned at bottom the conception of the very nature and method of epistemology. There were in play three different conceptions of what a "scientific philosophy", and thus epistemology, would look like. Schlick pursued a broadly foundationalist program, Neurath proposed an anti-foundationalist alternative, and Carnap sought a position in between. All three positions were, as I show in my reconstruction and analysis, well beyond the pre-Kantian naivété sometimes attributed to the Vienna Circle, and show thematic preoccupations still familiar from contemporary discussion.

My second main point is to establish the importance and nature of Neurath's contribution to the debate. On this point the Received View virtually completely overlooks the foundational debate in the physicalist camp: Neurath did not propose what Carnap made precise. Neurath's proposals aimed for different things than Carnap's, and Neurath and Carnap never wholly agreed on whether their programs were compatible. I present a detailed demonstration that and how Neurath argued for what has come to be known as a "naturalised epistemology" (put forth—under this name—by Quine, more or less with
Neurath's boat metaphor as its motto). Neurath argued against Carnap's "methodological solipsism" from 1928 onwards; in 1931 he first presented what amounts to a "private language argument". But Neurath's argument was not decisive historically, for his naturalised epistemology met with prejudicial preconceptions: Carnap and Schlick had their own agendas. With reference to the puzzle that results when Neurath's theory is simply pressed into the grid of common epistemological taxonomies and to the misunderstandings which Neurath's proposals were subjected to already in the Circle, I isolate two related bodies of doctrines which define Neurath's project: a theory of protocol sentences and a meta-epistemology. My concluding sketch highlights the distinctive practical-critical nature of Neurath's still non-standard conception of naturalised epistemology: its attention to the social dimension of knowledge production and transmission. Neurath's theory was not only, as a matter of historical fact, a precursor of, but also constitutes, as a matter of doctrine, an alternative to Quine's.

The result of this dissertation is a twofold one. By investigating the protocol sentence debate in much greater detail than has so far been done I support the case for the revision of the record of Vienna Circle history. I show the emergent new perspective on Carnap and Schlick as "analytical" neo-Kantians to be applicable to their distinct theorising about empirical science, and demonstrate that Neurath's epistemology requires an equally non-traditional, yet again completely different, reading, reconstruct it in outline, and show some of its contemporary relevance.

Thesis Committee: Sylvain Bromberger
Title: Professor of Philosophy

James Higginbotham
Title: Professor of Philosophy
ACKNOWLEDGEMENTS

I have many people to thank for their criticism, help, and encouragement, but I cannot blame the unclarities and mistakes that remain on them. I hope those mentioned can find at least some of their standards of a philosophy worth doing met here.

Being German I hope to be excused if I begin at the beginning, repeatedly, and with an analytic truth. This dissertation simply would not exist were it not for the members of my thesis committee, Professors Sylvain Bromberger and Jim Higginbotham, to whom I am deeply indebted. Fortunately, this is a distinctly empirical claim. As someone who prefers a good question to many answers, Sylvain gave me the confidence to put the questions in this dissertation before the reader, indeed, identified Neurath as a topic of interest when, amongst ramblings about Quine and the challenge to belief-desire psychology, I reported my first Neurath reading. His steady encouragement since that I could give the content I perceived a suitable form and put the matter in at least halfway plain English after all has been invaluable. Similarly, Jim Higginbotham again and again identified the kernels in unwieldy drafts from which I could go on in my reconstruction. I also wish to thank Professor Paul Horwich for many meetings and his efforts to hold me to standards of clarity compatible with what I call, in my conclusion, “controllable rationality”. His comments greatly clarified the tasks I faced and shaped my project as it approached its final form.

I thank the faculty of the Department of the Philosophy and Linguistics at MIT, where I have been a graduate student since 1981 for their teaching, help and kindness: if they ever read this, they should know they are meant--to name them all would be to prejudice the reader: surely, in this case I should have learnt better. Indeed, in particular since I also was an undergraduate and postgraduate student at University College London from 1976 to 1981. But I must mention Professors Ned Block, Joshua Cohen, W.D. Hart, Hide Ishiguro, Richard Larson and, at Kingsway-Princeton CFE Max Johnson.

Why this German had to go to America to end up with a dissertation about the Vienna Circle is a question, of course, not without its paradoxical charm. Nor is it without trace of cunning reason, for I had the great good fortune of Professor Robert S. Cohen’s interest in my work. I am very grateful for his advice and encouragement as my project matured, and particularly for his detailed comments on the penultimate version of this dissertation. I also wish to thank Professor Paul Neurath for his help and warm reception of my questions about Viennese Neurathiana, Professor W.V.O. Quine for a highly instructive interview, and Professor Rudolf Haller for discussing my dissertation project with me and giving valuable advice and orientation.

Over the years I have become indebted for peer instruction to Hugh Aitken, Ken Albert, Michael Antony, Alex Cambridge, Susan Dwyer, Paul Hoyningen-Huene, Jay Lebed, Eric Lormand, Charles Langley, Dan Leary, Ron McClamrock, Jeff McConnel, Kenneth McKenzie, Jim Page, Gabriel Segal and Steve Simon, particularly to Paul Pietroski on the characterisation of naturalism, and Margaret Reimer for help with the English; particular thanks to Marcia Lind and Sarah Patterson, Phil Schulte and Jo Simpson. For American ground support I thank Raffael Altmann, Kevin Clayton, Magnus Johnstone, Gary and Ntsiki Langford, Maurice Linton, Jim Roberge, and particularly Gretchen Kalonji. Longterm debts to Leandro Mazzoni, Margo Perrin, Louann Brizendine, and to Jenny Garland are gratefully acknowledged.

Last but not least, there is my family. I wish to thank my wife Phyllis for putting up with as much as she could: without her I definitely would not have made it as I did. I also wish to thank Camille Jordan and the Jordans at large. Very special thanks go to Renate and Oskar Bley for their support, and, particularly, to my mother, Doris Uebel.
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PREFACE:

ON NEURATH'S BOAT

Everybody familiar with contemporary analytical philosophy is likely to have come across the metaphor known as "Neurath's Boat":

We are like sailors who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and to reconstruct it there out of the best materials.¹

For Quine, to whose adoption its wide coinage is owed, this metaphor eloquently expresses that it is impossible for philosophers to succeed in basing scientific knowledge on indubitable foundations, and the moral that flows from this: epistemology might as well be naturalised! Very broadly, this means that the theorist of knowledge should forego a priori standards and presuppose the available results of scientific investigations in order to understand scientific knowledge. Science must be made to "explain itself". The boat metaphor has come to encapsulate this approach to epistemology. Despite Quine's frequent remarks on the Neurathian provenance of the "parable of the mariners"², however, few have thought it worth the effort to look beyond Quine to the meaning of the original. Yet this is just what I propose to do. In this preface I explain why I wish to stir up these muddy waters and sketch my method of inquiry.

¹Neurath 1932d:92.
1.1 The Puzzle of Neurath's Boat

Many things are puzzling about Neurath's metaphor (hereafter "Neurath's Boat" or "the Boat"). Perhaps first among them is what it meant for Neurath. Did it mean for him what it meant for Quine? If it did, we'd have another puzzle before us right away: was it not Quine who liberated analytical philosophy from the restrictive a priori conceptions of the Vienna Circle, and was Neurath not a founding member of this very group of philosophers? Another puzzle is, of course, why we are puzzled at all: how come Neurath's intended meaning is not better known--after all, the metaphor bears his name? I will not be concerned with this latter question here, though what I have to say does throw some light on it. My interest rather lies in the first puzzle, in the reconstruction of the philosophical arguments which Neurath meant to represent in the Boat. As it happens, this means that I must also be interested in the second puzzle, for I shall argue that Neurath did propose to naturalise epistemology.

Much contemporary philosophical interest in the Vienna Circle concerns its role in the formation of the views of Carnap and Quine. When such interest is focussed upon the Circle's philosophies of empirical science, as opposed to its philosophies of logic and mathematics, contemporary discussion typically sees "the" Vienna Circle opposing Quine on the topic of naturalism, for it rightly views Quine to be in conscious opposition to Carnap. One wants to know whether Quine is justified in his particularly radical conception of naturalism: need naturalised epistemology, for example, renounce appeal to meanings and propositional attitudes, all social science? Here, I believe, it is of not inconsiderable significance to note that Quine's radical opposition was not the first naturalistic response which Carnap had to face. Already in the Vienna Circle Neurath argued that epistemology be naturalised. What is more, Neurath's radical challenge to Carnap was not only, as a matter of historical fact, a precursor of, but also, as a matter of
doctrine, an alternative to Quine's. Neurath's epistemology was more than a neglected alternative at the time, it still is an unduly neglected alternative. This dual claim may surprise many, for even readers sympathetic to Neurath's epistemological project are often baffled by his writings and ask "What does Neurath want?" My answer is that Neurath wanted something quite simple in conception yet very hard to realise. Neurath wanted to develop a conception of knowledge as a tool for empowerment, an instrument of emancipation. What makes Neurath interesting is therefore not just his de facto anticipation of naturalistic epistemology, but the wider objectives behind his naturalism.

To fix ideas, let's ask what makes for naturalised (or naturalistic) epistemology. What does it mean to "explain science by science"? Given the ongoing developments in naturalistic epistemology, it is best to characterise this program not in terms of any particular doctrines which would prejudice its outcome, but in terms of its methodology. What makes epistemological inquiries naturalistic is the decision to employ only such concepts as can be explicated, in turn, by concepts which have proven themselves in the expansion of processes antecedently understood to be "natural" ones. Only ultimately scientifically legitimate concepts may thus figure in the justification of knowledge claims. Thus Quine replaced talk of "epistemic priority" by talk of "causal proximity to sense receptors"\(^3\), talk of "subjective similarity" by talk of "innate quality spaces"\(^4\), and talk of "the given" by talk of "observation sentences"\(^5\). For Quine, neurophysiology, evolutionary biology and behavioristic psychology furnished the explication of the epistemological concepts required by naturalism. Both Quine and Neurath required a

\(^3\)Quine 1969a:84/5.
\(^5\)Quine 1975:73. Observation sentences are defined as "occasion"--as opposed to standing or eternal--sentences "whose occasion is not only intersubjectively observable, but generally adequate, moreover, to elicit assent to the sentence from any present witness conversant with the language. (Ibid..)
scientific explication of epistemological concepts. They differed on what kind of sciences could be employed in this explication.

To get a sense of what is involved in Neurath's non-standard naturalism, consider that there are different ways of reading Neurath's Boat. There are what I shall call the "austere" reading and the "rich" reading of what is involved in rebuilding the metaphorical ship, understood to represent scientific knowledge. Both readings agree that it is impossible to repair any deficiencies in the body of scientific knowledge by stepping beyond it and appealing to insights that could not be challenged. The two readings disagree on the standards which the "internal repair" of science has to meet. According to the austere reading, whatever cannot meet the individuation conditions of physical kinds has to be consigned to the scrap-heap of mythology and cannot be used in reconstruction. The result is a restriction of the concept of science. Given the austere reading, Neurath's Boat means that natural science explains (knowledge of) natural science. According to the rich reading, all human cognitive endeavours could serve as replacement planks and beams to keep the ship of science afloat, provided one condition is met. This condition is that the assertions characteristic of these various enterprises be empirically testable (and thus, presumably, less than a reduction of all explanatory types to physical types is required). Given the rich reading, Neurath's Boat means that natural and social science explains (knowledge of) natural and social science. It is clear that the austere reading is Quine's, but which one was Neurath's? My claim is that the rich reading was what Neurath intended. This claim is related to my earlier ones. To make good the claim that Neurath's naturalised epistemology not only constituted a precursor, but also an alternative to Quine's means that my long-term task is to show that the rich reading of Neurath's Boat was the one intended.

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6 For two readings in furtherance of the comparison between the theories of Quine and Davidson see Hookway 1988.
by Neurath. Only the rich reading supports a conception of scientific knowledge compatible with Neurath's idea of knowledge as an instrument of emancipation.

1.2 Recovering the Cognitive Cargo of Neurath's Boat

I called this a long-term task for it demands a long account. Telling the story of Neurath's rich reading of his Boat is not just a matter of getting to understand one voice in the Vienna Circle. One must also understand the early Neurath and the late Neurath (neither of which has been much studied). All of them have their own Boats and each requires careful consideration: each one testifies to the originality of the thought of Neurath. Moreover, Neurath was a very contrapuntal writer: each metaphor comments or "signifies" on specific writers and positions in ways which are not immediately obvious. Each Boat, in other words, requires a deep background reading. The full story of the rich reading of Neurath's Boat is far too long to be fully told here, therefore. Here I will only make a start: I will undertake a re-reading of the Vienna Circle's protocol sentence debate, in the course of which Neurath's Boat made its most celebrated appearance, in order to distinguish Neurath's voice clearly from the other voices in the Vienna Circle and bring out his underlying epistemological perspective. I shall delineate Neurath's neglected variant of naturalism and will indicate how further research might proceed.

My investigation is of interest not only to students of Otto Neurath, but is intended as a contribution to general Vienna Circle scholarship. The reason for this is that, as I said, Otto Neurath was a contrapuntal writer: to understand his position it is necessary to take into account the positions of the writers he responded to. This characteristic of Neurath's work considerably influences how I must proceed in my reconstruction of its content. My inquiry thus cannot begin with Neurath's own theorising: first those positions against

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7Other uses of the simile are Neurath 1913a:457, 1921:199, 1937a:181 and 1944:47.
which he reacted must be presented. Only against their background do interpretations of
Neurath's pronouncements lose the appearance of being arbitrary. For the Neurath of
interest here--the Neurath of the Vienna Circle--this means that his remarks have to be read
against the background of the theories of Rudolf Carnap and Moritz Schlick. But
construing Schlick and Carnap is not as simple as one might think. Of late, a revision of
the picture of Vienna Circle philosophy has been in progress which challenges the
traditional view that its members were naive believers in scientism, the self-sufficiency of
formal and natural science. The revision focusses particularly on Carnap's philosophical
positions. For my reconstruction of Neurath's views this means that his views must be set
against not only the traditional but also this new picture of Vienna Circle theorising! This
consideration and evaluation of the new reading of Carnap and its bearings on the
interpretation of Schlick and Neurath--in which I have to engage in order to get at Neurath--
makes my inquiry relevant to Vienna Circle scholarship in general.

This then is my project in this dissertation. I propose to begin the project of
reconstructing Neurath's non-standard naturalistic epistemology by providing a detailed
reconstruction and reevaluation a re-reading of the historical context in which Neurath's
Boat made its most famous appearance and from which I shall, in due course, derive
pointers for its correct interpretation. Neurath's theory lurks behind his apparently throw-
away remarks in a confusing debate in which the Vienna Circle's underlying conflict about
the nature of epistemology surfaced obliquely. Neurath's neglected theory of knowledge is
greatly at variance not only with the conceptions of knowledge and epistemology
commonly associated with the Vienna Circle, but also with the other conceptions in fact put
forward there. My review of the debate will provide detailed demonstration not only of
Neurath's opposition to the epistemological projects commonly associated with the Vienna
Circle, but also of his opposition to the epistemological projects actually pursued by
Schlick and Carnap. To demonstrate this properly—in full cognisance of recent advances in Vienna Circle scholarship, of historical fact, and in aid of my own thesis—I will review the debate as a clash of three different epistemological perspectives undergoing several distinct stages. (To show what Neurath argued against whom and how, thus requires even that the Carnap-Schlick debate be highlighted, for it provides valuable insight into the thinking of Neurath's opponents not available in their exchanges with Neurath himself. More on all this in chapter 2, the "Introduction" proper.)

The outline of Neurath's epistemology which emerges is one which is not only consistent with, but also supportive of the view of Neurath's non-standard naturalism put forward here. In the conclusion, chapter 12, I have tried to make the foundation I have laid for this view "call out" for its complementation by one rich reading of the Boat. The following chapter 2, "Introduction", begins the descent into the trenches, chapters 3 to 8, which reconstruct Vienna Circle's protocol sentence debate in detail. Properly informed, I return to the large-scale questions, raised in the introduction (ch.2), in my analytical chapters 9 to 11. It is in these chapters that Neurath's naturalism crystallises into sufficiently concrete doctrines which are partly developed in chapter 11, and which provide the foundation for the non-standard naturalism advertised in this preface and further developed in the conclusion, chapter 12.

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8 Quine, it must be said, already turns up towards the end of chapters 3 and 6, and figures heavily in 11.
9 Some further advice for the hurried reader. For a further overview of the contents of this dissertation, but presupposing some of the discussion in the sections preceding it in ch. 2, Introduction, see sect. 2.6. For further detail, short of reading the thing, see the detailed Table of Contents and compare the little abstracts preceding each chapter. (In general I have also sought to make clear in my footnoting which particular parts of my reconstruction any particular part of my analytic chapters builds on.)
Chapter 2

INTRODUCTION:

OTTO NEURATH. THE VIENNA CIRCLE.
AND THE PROTOCOL SENTENCE DEBATE

Abstract: Sketch of the epistemological problem of scepticism to which Neurath's Boat applies (2.1). Presentation of the basic tenets of the scientific philosophy which constitute the common denominator for the views held by the members of the Vienna Circle to which Neurath belonged (2.2). Two important mistakes in the traditional picture of the aim and method of the Vienna Circle; discussion of related literature (2.3) Schematic overview of the themes and structure of the Circle's protocol sentence debate (2.4). Preview of Schlick's, Carnap's and Neurath's distinct conceptions of scientific philosophy (2.5). Overview of the course of my inquiry (2.6). Appendix: Defense of my dating of the stages of the debate.

In this chapter I set the stage for my subsequent review of the protocol sentence debate as an entry into Neurath's theory of knowledge. I introduce the Vienna Circle, consider how the Vienna Circle philosophy in general is to be interpreted, introduce the themes of the protocol sentence debate, sketch the orientation of the main disputants, and give an overview of the chapters that follow. I begin with the epistemological problem to which the metaphor of Neurath's Boat applies.

2.1 Neurath's Boat and the Challenge of Scepticism

The problems to which the theory of knowledge or epistemology addresses itself are most directly raised by sceptical questions.¹ Sceptical questions challenge the claim that

¹For a more careful delineation of the territory see chapter 9: the subtleties developed there will not come prominently into play until then.
we do possess knowledge. They ask that knowledge claims be justified. Sceptical arguments purport to show that the knowledge claims at issue are not justified.

There are many kinds of scepticism. When sceptical questions ask that knowledge claims be justified, they focus upon a statement or proposition which can be doubted, and ask that this doubt be stilled. Sceptical questions can thus be raised about all kinds of knowledge claims, and within each kind, about all areas. There is scepticism about perceptual knowledge, about induction and memory, about other minds, about moral and a priori knowledge. Clearly, the more areas or kinds of knowledge the sceptic calls into question, the harder it becomes to answer the challenge. Furthermore, over the course of history, sceptical challenges changed and became more radical. Whereas the sceptic of antiquity merely doubted our knowledge of the true nature of the natural and moral world, the modern sceptic doubts our knowledge of the very existence of the purported object of knowledge. Cartesian scepticism doubts the existence of the physical world; since Nietzsche, scepticism even doubts the existence of an "inner" world, in particular, the correct grasp of the content of one's own mental states.²

The sceptical challenge proceeds by means of a regress argument. Grant, as seems plausible, that having knowledge involves having justification for holding true the belief claimed to express knowledge. (Consider, in other words, knowledge to be, roughly, justified true belief.)³ Ask: how do beliefs accrue their justification? They can have it either intrinsically or extrinsically, in virtue of their relation to other beliefs. The sceptic

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²On the absence of idealism in Greek philosophy see Burnyeat 1982, for the anticipation of contemporary radical scepticism (pithily summarised in Heil 1988) in Nietzsche see e.g. his 1883-88:#476: "Our inner world, too, 'appearance'!" For a Vienna Circle appreciation of Nietzsche see Frank 1917, for observations on the Vienna Circle Nietzsche reception see Fischer 1982a.

³Gettier-type arguments against the traditional concept of knowledge as justified true belief are most often easily rephrased into arguments for a more precisely delimited concept of justification; they do not reject the analysis tout court, but seek to provide a further fourth condition which qualifies and clarifies the justification condition. For the foundation and the fruits of the Gettier industry see Griffiths 1967, Gallis & Roth 1970/79, Pappas & Swain 1978, Pappas 1979, Nozick 1981, Shope 1983, Dancy 1985, Pollock 1987.
holds that, unless we come up with intrinsically justified beliefs, none of our beliefs are ever justified: with only extrinsic justification at hand, justification would remain incomplete. Extrinsic justification would either lead to an infinite regress, to a circular chain, or to a brute breaking-off of justification. Neither is acceptable as justification for knowledge claims, but we do not have intrinsically justified beliefs either. So the sceptic argues with the argument from error. (The argument from error goes as follows. No matter how justified I may appear to be for making a particular claim, I could be in a, from my point of view, identical type of situation, in which I would be mistaken in my claim, and I would not know that I would be in that situation. Since I could not distinguish the situations, I am not justified in believing that I am not deceived, say, by an evil demon or a neuro-surgeon and computer hack.) With no beliefs justified, the sceptic concludes that we have no knowledge.

The argument from error is sometimes rejected flat out, but this does not necessarily diminish its worth. The value of sceptical arguments lies not only in raising the question of fact, namely, do we have knowledge, but also in raising a more conceptual question, namely what is knowledge? It does so because it raises the question of right, namely, are we justified in our knowledge claims, and so raises the question of what kind of justification is appropriate for knowledge claims. The sceptical argument thus switches from possession of knowledge to claims to know. Against this sceptical challenge, which holds that we cannot produce the justification needed to redeem knowledge claims, the defense may either accept the sceptic's premises and show him mistaken in his reasoning, or accept his reasoning and challenge his premises. In common terminology, the defense may either be foundationalist or anti-foundationalist. Foundationalists hold that all empirical knowledge consists of either basic or non-basic beliefs, and that basic, self-justifying beliefs can be found. Anti-foundationalists, by contrast accept that there are no
self-justifying beliefs that could help the justification of empirical knowledge claims; consequently, they must provide an alternative account of justification.

It is at this point in the sceptical argument that the metaphor of Neurath's Boat comes into play.

We are like sailors who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and to reconstruct it there out of the best materials.

Neurath's Boat addressed itself (primarily) to the problem of justifying empirical knowledge claims against the sceptical challenge. In this context of the anti-sceptical defense, the Boat expresses an anti-foundationalist position. Whereas the foundationalist owes an account of the nature of basic beliefs and of the relation of "epistemic ascent" from the basic to the non-basic beliefs, the anti-foundationalist owes an argument establishing that no self-justifying propositions can be found and an account of the nature of justification without foundations. My reconstruction of the cognitive cargo of Neurath's Boat thus requires, first, an argument for the impossibility of epistemic foundations, and, second, an account of non-foundationalist justification. (What takes the place of intrinsic justification according to Neurath's Boat?) The metaphor suggests that, for Neurath, extrinsic justificaton was all the justification there was. Neurath disputed the sceptic's premmiss that knowledge requires the kind of certainty which self-justifying beliefs are said to provide. But how was he to respond to the claim that mere rejection of the demand for certainty did not make an infinite regress, a circle, or a breaking-off of justification any more epistemically respectable, and so did not allow knowledge by default? Why, in other words, is Neurath's position not a sceptical one--or is it? Here we may expect his naturalism to tell the story we need to hear.

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4Referred to, as I announced in the preface, by "Neurath's Boat " or "the Boat".
5Neurath 1932d:92.
6See below, section 8.2.2.
So Neurath requires, first, an argument against foundations, second, an argument for why a non-foundationalist justification must be a naturalistic, "scientific" one, and, third, an account of how that naturalistic justification would go. Any of these accounts and arguments are still outstanding. Though the word is out that Neurath darkly anticipated the naturalistic turn of epistemology, and so "saved" knowledge, it is not at all clear, however, how he did so. The claim that Neurath naturalised epistemology still lacks proper justification. (It cannot be said to be a piece of knowledge.) The source of this claim lies in the fact that Quine adopted Neurath's Boat as the motto for his program of naturalistic epistemology, yet Neurath's own theory of knowledge has remained virtually unknown. In fact, there is much puzzlement over how Neurath could have anticipated naturalistic epistemology, for the Vienna Circle is commonly held to have pursued a radically different strategy of opposition to the sceptic, namely, the traditional route of foundationalism. It is this matter that this dissertation seeks to set straight. It turns out, furthermore, that in the course of demonstrating that and how Neurath proposed the naturalisation of epistemology, I must also challenge the common conception of the Vienna Circle as traditionally foundationalist in the case of Neurath's opponents.

2.2 The Scientific Philosophy of the Vienna Circle: Basic Doctrines and Aim

The present section has three tasks: to briefly identify the Vienna Circle\textsuperscript{1}, to set out its common doctrines, and to introduce its overall anti-sceptical strategy.

2.2.1 The Vienna Circle

The Vienna Circle was a group of thinkers who met for weekly discussions of questions in the philosophy of formal and natural science from 1923 to 1936/7. Its

\footnote{For an overview of the wide literature on the Vienna Circle see below sections 2.3.1, 2.3.3, 2.3.4, 2.4.3, 2.5.3, the appendix to ch. 2, and the Bibliographical Remarks 13.1.}
principal members were Moritz Schlick, Hans Hahn, Otto Neurath, Philip Frank (since 1912 in Prague), Rudolf Carnap (since 1931 in Prague); others include Herbert Feigl (since 1930 in the US), Friedrich Waismann, Hugo Bergmann, Viktor Kraft, Bela von Juhos and Heinrich Neider. Viennese associates of the Circle include Kurt Goedel, Karl Menger, and Edgar Zilsel.8 (It's "official opposition", in Neurath's phrase, is Karl Popper.)

The central task undertaken by the Vienna Circle was the philosophical understanding of scientific knowledge.9 Unusually so for philosophers of their time, all of the Vienna Circle's members and most of its associates were trained in the formal or empirical sciences. (The Circle's common philosophical heroes were Poincare, Russell, Frege, Wittgenstein, Mach and Duhem.10) Apart from the philosophical pre-history of its older members (Schlick, Hahn, Neurath, Frank and, much shorter, Carnap), the Circle's existence fell into two periods. During the formative period from 1923 to 1928/9, their activity was mostly confined to their weekly discussions. In 1928/9, the Circle entered its public period with the formation of the Verein Ernst Mach, the publication of its "manifesto" Wissenschaftliche Weltschauung: Der Wiener Kreis by some of its leading members, the first public conference of "Scientific Philosophy" in Prague, and the first publications of a series of philosophical monographs Schriften zur wissenschaftlichen Weltschauung edited by Frank and Schlick.

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8I do not claim completeness here; compare Rutte 1977.
9Neurath, in fact, rejected the term "philosophy" as a label for the object of his efforts and preferred the more neutral term "scientific world conception" ("wissenschaftliche Weltschauung"). I shall continue to class him amongst the scientific philosophers nonetheless. See Rutte 1982a for a discussion of the justification of calling Neurath a philosopher despite his own disclaimers.
10The individual emphases are: Poincare for Carnap, Schlick and Neurath, Russell for Carnap and Schlick, Frege for Carnap, Wittgenstein for Schlick, Mach for Carnap and Neurath, Duhem for Neurath; important oppositions are Neurath to Wittgensteinian "correspondentism" (to use Coffa's phrase) and Schlick to Machian pragmatism.
The Circle soon attracted international attention and collaboration, first with Hans Reichenbach and the Berlin "Society of Empirical Philosophy", in the acquisition of the editorship of a philosophical journal renamed Erkenntnis, and soon with further colloquia and international congresses in Koenigsberg, Prague, Paris, Copenhagen, Cambridge, England and Cambridge, Mass.. International visitors and associates included, from Berlin: Carl Gustav Hempel; from Poland: Alfred Tarski; from England: A. J. Ayer; from the USA: Albert Blumberg, Ernest Nagel and W. V. O. Quine; from Scandinavia: Arne Naess, Eino Kaila, Ake Petzaell and Joergen Joergensen. Partly due to these wide contacts, the death and dispersion of key members from 1934 onwards (Hahn † 1934, Neurath exiled to Holland 1934, Carnap to US 1935, Schlick † 1936) did not mean the extinction of the Vienna Circle's philosophy. 11 Through the subsequent work of earlier visitors (Ayer, Naess, Nagel, Quine) and particularly through the work of their members and collaborators who had emigrated to the USA (Carnap, Feigl, Frank, Hempel, Reichenbach), their philosophical explorations proved extremely influential on the development of Anglo-American analytic philosophy.

2.2.2 The Task To Comprehend Scientific Knowledge: The Historical Setting of Vienna Circle Philosophy

Here, briefly, is the historical situation in which the Vienna Circle saw its task located. Kant's answer to the sceptical claim that objective knowledge of nature was impossible involved what he called the synthetic a priori: regulative principles of reason that are imposed upon the deliverances of sensibility, just as our faculties of sensibility impose upon stimulation the intuitions of space and time. These synthetic a priori principles provided, according to Kant, an intersubjectively intelligible picture, not only of the phenomena of contingent experience, but also of the necessities of nature. All objective

11 For a careful study the dispersion of the Vienna Circle see Dahms 1985c.
knowledge was attained, however, only against the background of our continuing confrontation with "noumena", unknowable "things-in-themselves".

In the course of the 19th century, science and philosophy parted ways in most of the German speaking world. As philosophy became more extravagant in its justifications of knowledge claims, practitioners of science themselves became more modest in what they claimed scientific knowledge to be. The positivistic program of reducing the knowledge claim of science and then providing legitimisation to what's left, found wide favour. Description, not the explanation, of natural phenomena became the professed task of science. Theorists of science no longer looked at philosophy for metaphysical foundations, but rather found them in the utility of their preferred empirical procedures. Hypotheses and all other generalisations and abstractions were viewed as legitimated only by the results of whatever were the preferred empirical methods. For the positivist precursors of the Vienna Circle, like Mach and Avenarius, the reference to "noumena" placed the Kantian grounding of science in the same idealist bag as the philosophies of Fichte, Schelling and Hegel who followed him. Along with the positivists, the Vienna Circle thought the Kantian answer to the sceptical challenge uneconomical. Secondly, the Vienna Circle and its conventionalist precursors, like Poincare and Duhem, saw it contradicted by formal science, logic and mathematics. Riemann's geometries showed that questions about the geometry of physical space were open to more than one answer: was physical space Euclidean or non-Euclidean? It fell to the Vienna Circle itself to see and argue that physical science, namely relativity theory, showed the untenability of

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13Such an empiricist view left non-empirical, philosophical justifications of science out in the cold; at best, as with Helmholtz--whose writings Schlick edited with P. Hertz--even Kant's speculations were viewed as standing in need of empirical support.

14In this all agreed, whether they followed Mill's inductivism (as did Helmholtz) or Whewell's deductivism (as did Hertz).
Kant's conception of space and time as forever fixed a synthertic priori forms of intuition.\textsuperscript{15}

The obvious answer to these problems of Kant's grounding of science might seem to be a radical empiricism like Mill's, but Frege's anti-psychologistic critique had shown empiricism unable to account for the knowledge of arithmetic. How then could the Vienna Circle defend the claim that science provides knowledge, given the insufficiency of purely a posteriori knowledge to account for knowledge of logic and mathematics, and the impossibility of synthetic a priori knowledge to account for empirical knowledge?

\subsection*{2.2.3 The Viennese Solution: the Marriage of Empiricism and Logicism}

The Vienna Circle saved empiricism by following the positivist strategy of freeing epistemology from impossible tasks; their strategy absolved empiricism from having to explain the possibility of formal knowledge. They defended the basis tenet of empiricism, that all factual knowledge comes from experience, by the reconceptualisation of the relation between a priori and a posteriori inquiries. This reconceptualisation allowed the proper understanding of both formal and physical science by means of their own meaning theorem.

Let me note four steps in this reasoning. First, the Vienna Circle embraced the mathematical logic of Frege and Russell and, in particular, their doctrine of logicism. Arithmetic was considered a part of logic and thus treated as entirely analytical, without any empirical content. Formal truth was exhausted by what could be proved. The Vienna Circle proposed a broadly constructivist epistemology for formal science: the synthetic a

\textsuperscript{15}This is extremely rough: unorthodox Kantians like Simmel and orthodox Neo-Kantians like Cassirer in effect argued that formal and physical reality could not contradict transcendental reasoning. For Schlick's important role in the refutation of this view (Schlick 1915, 1917, 1921, 1922) see also sect. 4.1 below.
priori was not needed for the legitimisation of formal science because all of its results were non-synthetic.

Second, the Vienna Circle adopted the view that only those statements were meaningful which could in some sense be tested for their truth. The Vienna Circle's verificationism was in fact an attempt to arrive at a notion like empirical content. Loosely speaking, the fact designated by a statement—which constituted its meaning—had to be empirically discernible: no such discernment, no factual meaning. Empirical science needed no synthetic a priori either; all of its statements or propositions could be tested. Concepts like causation whose applicability was empirically indiscernible were to be banned from science. (Whether an empirical claim needed to be actually verified or only be potentially verifiable, or only be potentially testable, and whether so by current or only by future means, in order to be meaningful was a matter of discussion in the Circle. \(^{16}\)

Next, note how the doctrines of logicism and verificationism interact. Third, the Vienna Circle relieved empiricism of the task of accounting for logical or mathematical knowledge. Given their constructivist reading of logicism, the verificationist criterion of meaningfulness categorised all statements as either meaningful or meaningless without jettisoning formal science. It became possible to distinguish between acceptable and unacceptable non-empirical statements: the statements of logic and mathematics could be retained due to their tautologous nature, as they could be proved true without reference to matters of empirical fact.

Fourth, the Vienna Circle held that the meaning of terms for the unobservables of empirical science had to be reconstructed by logical operations from the meaning of observational terms. Only if such reconstructions were provided did the more theoretical parts of science retain their empirical character. Here the Circle's distinction between the

\(^{16}\)See e.g. Carnap's retrospective comment in 1934/7:321/2 and Schlick 1936.
formal and the empirical sciences, so that the former furnished purely analytical tools for
the pursuit of the latter, proved helpful. Just what kind of reduction should and could be
aimed for was not always clear. However, it is the reductionist fervour with which the
Vienna Circle implemented its basically reconstructivist requirement on "theoretical" terms,
that the Vienna Circle is perhaps best known for.

With the marriage of empiricism and logicism, the demarcation criterion of
meaningful discourse became the demarcation criterion of scientific discourse. The
meaningful was identified with the scientific. The Vienna Circle defended scientific
knowledge claims and at the same time ruled out of court all contenders to the title of
knowledge except formal and empirical science. In particular, the claims of metaphysical
philosophers were thus dismissed. One and the same conceptual tool, the verificationist
criterion of meaningfulness, was of prime importance for both the defense of science and
the dethronement of metaphysics. (Whatever meaning did not survive this identification
with the scientific was deemed purely emotive, explained as merely a psychological by-
product, and consigned to poetry. The Circle's position was while that such meaning may
well exist, it was irrelevant to the justification of knowledge claims.)

Part of the reason that the verificationist theory of meaning became so extremely
important, was that the Vienna Circle also took what is known as the linguistic turn. Of
the different meanings this phrase has taken on I focus on these two. First, contents of
mind were considered amenable to philosophical analysis only once they were linguistically
articulated. This may be called the "anti-intuitional" aspect of the linguistic turn. Secondly
it consisted, in the words of Bergmann (who coined the phrase), in the turn from simply
talking about the world to "talk about the world by means of talking about a suitable

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17 The linguistic turn is sometimes held to be first taken in Wittgenstein's *Tractatus* (Schnaedelbach
1977:47) which the Circle studied intensively in its early years.
language" for talking about the world. This may be called the "meta-linguistic" aspect of the linguistic turn. The linguistic turn, as understood here, thus consisted in the presuppositions that, first, all knowledge required linguistic representation and that, second, all philosophy concerned ways of representing, rather than the nature of the represented.

Given the close connection between meaningfulness and knowledge, verificationism after the linguistic turn meant that there simply remained no place to run to for the defense of non-empirical scientific or non-meta-scientific philosophical knowledge. The claim that science abides by the verifiability criterion of meaningfulness thus amounted to the adoption of the unity of science thesis in one form or another. The unity of science thesis comes in different versions.

According to the methodological version of that thesis, all of science abides by the same criteria: no basic methodological differences separate the natural from the social or cultural sciences ("Geistewissenschafeten") as claimed by those who distinguish between 'explanation' and 'understanding'. According to the latter, explanation consists in the subsumption of a given phenomenon under a general law, whereas understanding is accomplished through a process called 'empathy'. Given the impossibility of verifying empathetic knowledge, statements claiming to represent such knowledge violated the criterion of meaningfulness as they could not be backed by empirical, intersubjectively available evidence. Any claim based on empathetic knowledge that could also be supported empirically would of course constitute regular scientific knowledge without needing empathetic evidence. With the synthetic a priori rejected, all factual knowledge had to be empirical knowledge and had to stand the tests of natural science. All members of the Circle endorsed this methodological version of the unity of science thesis.

18Bergmann 1964:177.
19E.g. Carnap 1928a:82/3.
Another, yet stronger, version of the unity of science thesis was a linguistic thesis. This thesis held that all objects of scientific knowledge could in principle be comprehended by the same basic "universal" language. Physicalism asserts that this language is the language which speaks of physical objects. While everybody in the Circle endorsed physicalism in this sense, the understanding of its importance varied. There was disagreement on whether the thesis of physicalism was (Neurath) or was not (Carnap) of ontological import, and on whether the presumed truth of this thesis was merely contingent and philosophically irrelevant (Schlick) or, while not logically necessary, nevertheless philosophically decisive (Neurath). The discussion of the protocol sentence debate ultimately turned on the question whether the physicalistic language in fact sufficed also for expressing the claims of scientific philosophy, which will occupy us much below. (Note also that this linguistic version of the unity of science thesis was only later clearly distinguished from the nomological version: whether all scientific laws could be reduced to those of physics was yet another matter.20)

2.2.4 What is "Scientific" about Scientific Philosophy?

So much for the basic doctrines of the Vienna Circle. As we saw, the central task undertaken by the Vienna Circle was the philosophical understanding of scientific knowledge. The accomplishment of this task involved its members in the following theme: to understand science better than previous philosophies, the Vienna Circle found that they had to refashion philosophy itself. The Vienna Circle argued that philosophy itself had to become "scientific". It is this theme which makes the Vienna Circle's theory of knowledge particularly interesting.

The distinguishing trait of Viennese scientific philosophy is its desideratum that philosophical discourse has to meet the same standards of intelligibility as inquiries in the

established empirical and formal sciences. Science, having long forsaken explanatory appeals to authorities whose workings were unfathomable, and having stressed instead the testability of its theories by any qualified individual, here served as the ideal exemplar of rationality. Its rationality consisted in the requirement that scientists follow two principles: first, that they make only testable claims and, second, that they adjust their beliefs to accord with intersubjectively available evidence. The claim that philosophers should follow science in this respect set the philosophy of the Vienna Circle and its sympathisers apart from the academic philosophy of their time. All participants of Schlick’s Circle bemoaned the kind of philosophy they saw done and the inconclusive state of conflicting philosophical knowledge claims.\textsuperscript{21} The results of two thousand years of philosophical research compared badly with the progress of natural science in only a few hundred years. There did not even exist agreement as to what would constitute a solution to a philosophical problem. How different matters were in science: only those pronouncements were accepted into its body of theory which withstood experimental tests. Philosophy too, the Vienna Circle concluded, stood in need of something like the scientific method.

But that was not all. Philosophy not only failed to reach the standards of scientific inquiry in terms of agreement between its practitioners about what would constitute solutions to its problems, it also failed to understand science itself. If philosophy had to emulate the rationality of science to retain its claim to represent knowledge, as the Vienna Circle held, it was also required that natural science itself be philosophically attended to. According to much academic philosophy, the knowledge claim of natural science was itself in jeopardy.\textsuperscript{22} A new specifically anti-scientific scepticism had arisen: The inability of philosophy to account in particular for the "new physics" of relativity theory was widely taken to challenge the authority of scientific rationality itself. Science itself, so it turned

\textsuperscript{21}See the Circle’s manifesto’s declaration of intent to remove “the metaphysical and theological debris of millenia” (Neurath et al 1929:317).

\textsuperscript{22}Compare Frank 1941:4, 1949:1–4 and Menger 1933b.
out, rested on the employment of concepts and principles which were not univocally supported by empirical evidence. Beyond the longstanding problem of the justification of basic principles of scientific explanation like induction, there now also existed the problem of how basic physical concepts like space and time were to be understood. Proponents of especially those intellectual and cultural positions which had been undermined by the advances of natural science in the 19th century, fastened on this crisis in scientific metatheory and sought to resurrect their dogmas on the alleged ruins of their adversary. In order to allow philosophy to firmly "take its stand on the ground of empirical science"23, the Vienna Circle had to establish the claim of science to provide "authentic"24 knowledge. If philosophy is to be rational, it must resemble science in the right respects; hence it must come to understand what these respects are.

The first Viennese step towards the scientific philosophy thus consisted in a defense of the new physical theories. Viennese scientific philosophy took scientific knowledge as a basic fact to be explained. It abandoned metaphysical knowledge claims and counter-claimed all metaphysical claims as meaningless. This abandonment of metaphysics amounted to the abandonment of some philosophical conceptions of knowledge, for instance, of the conception of knowledge of ultimate reality! In this respect, all in the Vienna Circle were "anti-metaphysical": no member left the traditional picture of knowledge undisturbed—philosophical preconceptions could be, and were in fact, challenged. The Vienna Circle did not, then, affirm a traditional "first" philosophy over and above science. Instead, it attempted to derive conclusions from philosophical experience, namely, the history of philosophy. The project of inquiry itself underwent change in response to checking its theories. Philosophers proceeded like scientists. (Neurath, who, incidentally,

24Carnap 1928b:308.
is responsible for the names "Vienna Circle" and "Logical Empiricism", sometimes referred to scientific philosophy in this sense as "Empirical Rationalism".\footnote{See Frank 1949a for the recollection of Neurath's intention to recall happy things Viennese with the label "Vienna Circle". See Neurath 1928b and 1929 for his previous use of the label "Wiener Schule um Moritz Schlick". See Neurath 1931b:52 for his first use of "Logical Empiricism" (and Neurath 1945/6:234 for a reminder of this origination); see Neurath 1928b:295 and 1937c:191 for uses of "Empirical Rationalism"; Neurath credited the term to the Russian logician Gregorius Itelson, not to Abel Rey, the French conventionalist philosopher of science, read in the "first" Vienna Circle (see below), who also used the term.}

2.3 Different Interpretations of Viennese Scientific Philosophy

I said that the interest of the Vienna Circle's theory of knowledge lies in its desire to become scientific by its own standards. This desire, in particular, its refusal to reaffirm the primacy of philosophy, makes it relevant to modern epistemology. Many problems which contemporary theorists of knowledge face first made their appearance then. What is important here to our correct understanding of the positions adopted and the reasoning put forward in the Vienna Circle is that the scientific turn of epistemology has been construed in different ways. First, it has been construed differently by different schools of historians of the Vienna Circle. Second, it was construed differently by different members of the Vienna Circle itself. In this section, I shall first review and criticise the traditional view of scientific philosophy and briefly introduce two non-traditional schools of Vienna Circle scholarship and their perspectives on Viennese scientific philosophy. (In the final section of this chapter I shall sketch in broad outlines the different perspectives adopted in the Vienna Circle itself by the three protagonists of the protocol sentence debate.)

2.3.1 The Traditional View of Scientific Philosophy Presented and Criticised

The traditional view of the scientific philosophy of the Vienna Circle, which bases itself on the common Viennese assumptions and theses reviewed above, is the following.
The scientific philosophy of the Vienna Circle tried to comprehend increasingly complex scientific theories and their counter-intuitive results by combining logicism and phenomenalist empiricism and rejecting Kant's transcendental philosophy.\(^{26}\) Its neopositivism consisted in the resolute worldliness of the conception of knowledge it advocated and its refusal to invoke trans-empirical presuppositions such as the synthetic a priori. Scientific knowledge was a body of statements whose claim to embody knowledge could be established by logical means alone. As for philosophy itself, its methodology and knowledge claims were construed as paralleling those of the formal sciences; its point consisted in the logical clarification of scientific concepts and justification procedures.

The traditionalist picture of Vienna Circle philosophy is not all wrong, of course, but it definitely stands in need of qualification. First, it gives a false sense of unity of the philosophies pursued in the Vienna Circle. Neurath never endorsed phenomenalism. Second, it misrepresents the point of agreement in the Vienna Circle. All of them were aware that the very possibility of knowledge was called into question. The traditional picture suggests a false sense of unity in the view of the Vienna Circle because the question of exactly how the program for a scientific philosophy was to be executed constituted the central topic of disagreement in the Circle itself. The traditional picture misrepresents their agreement because their common aim of a scientific philosophy was not arrived at in "pre-critical" ignorance.

Three distinct sets of positions on numerous issues must be distinguished within the Vienna Circle. (I document their interaction in the body of this essay.) The different sets of answers given by Schlick, Carnap, and Neurath pertained not only to questions of detail. This makes it all the more unfortunate that the traditional picture misrepresents what all three perspectives on scientific philosophy agreed on. Put bluntly, to defend science,

\(^{26}\) Representative here are, for example, Kraft 1950, 1974, Joergensen 1951, Ayer 1959b, Passmore 1957/68--but note the non-standard acuity in his 1967 (see fn. 57 below)--and Hanfling 1981.
the Vienna Circle accepted established scientific theories and subsequently proved their knowledge claim. To its detractors (who mostly base themselves on the traditional view) this move seems peculiarly unsophisticated. The Viennese aversion to metaphysics and its apparently rabid reductionism seemed to hide the failure to confront scepticism. Given the traditional picture, it might be tempting to agree with Popper's early judgement that the Vienna Circle's philosophy was pursued more or less in ignorance of Kant\textsuperscript{27}: it stated the facts and limits of knowledge, its actuality, as if its very possibility had never been in doubt--its foundationalism was pre-critical in a Kantian sense.\textsuperscript{28} What invites Popper's early judgement and the Critical Theorists' reaction is what appears as the Circle's seemingly indiscriminate use of verificationism as a theory of meaning. It encourages questions like: Didn't they know Kant's critique of empiricism? How could they simply presuppose what is highly problematic, namely, the objectivity of experience? Amplifying Humean habit through modern logic hardly seemed to complete the very task Hume had left undone: to show how norms could be derived from an investigation of fact. The facts of the case, however, suggest otherwise. The theorists of the Vienna Circle were not pre-critically naive as alleged by these "philosophical" criticisms.

What is overlooked in the judgement that the Vienna Circle was guilty of naive scientism--a judgement encouraged but not necessitated by the traditional view--is precisely what Schlick, Carnap and Neurath actually agreed about. They agreed that the defense against the sceptical challenge to science required a new theory of meaning that was critical of the empiricists' theory of meaning of old, namely an associationist picture meaning. They were concerned with the forms of representation for the very reason of developing a theory of meaning which faced the Kantian conundra and solved them. The linguistic turn

\textsuperscript{27}Popper 1974:82/3. 
\textsuperscript{28}Given the traditional picture, it also tempting to agree with the judgement of Critical Theorists that the Vienna Circle's "scientism" was pre-critical in a Hegelian-Marxian sense (Horkheimer 1937, Habermas 1968). I cannot deal with this question in this dissertation, but see below ch. 12.
sought to answer Kant, not avoid him. Even the most traditional and the most radical thinkers of the Vienna Circle (Schlick, Neurath) struggled with the Kantian question of the possibility of objective knowledge, and the Viennese middle way (Carnap) made this its central theme. All three sought to develop an account of justification that addressed the normative force which talk of objectivity possesses. It is simply false to attribute ignorance of the doubt that knowledge be even possible to Schlick or Neurath or Carnap. (That, in fact, was one of the few things they did agree about.)

2.3.2 An Alternative View of Vienna Circle Philosophy

The traditional view overlooks the fact that the anti-intuitional aspect of the linguistic turn meant that the Vienna Circle needed and pursued an entirely new theory of meaning. They pursued the idea that objectivity, and thus content or meaning, rested in the form of representation. As I shall show, a debate about the very nature of epistemology after Kant constituted a mostly unspoken core of the Circle's discussions in the protocol sentence debate. In this debate, the reach of the sceptical claim that knowledge is impossible was itself at issue. This problematic is reflected in Schlick's, Carnap's and Neurath's different theories of meaning. Despite their differences, however, they agreed on what made their theories of meaning "critical".

The label "verificationism" cannot stand on its own as characterising the Vienna Circle's theory of meaning. True, they held that whatever is relevant in the verification of empirical statements constitutes, in some sense, the meaning of these statements; but this did not exhaust the Viennese theory of meaning. Talk of verification supposes that the meaning of empirical statements ultimately involves experience of the fact referred to by the statement in question, or involves epistemic access to a necessary and sufficient criterion of its truth. (More modest talk of confirmation would be content with rough-and-ready criteria.) Call this the "empirical meaning" of statements. But this empirical meaning was
not all the meaning the Vienna Circle knew. There was also what could be called "analytic or structural meaning" or "combinatorial content". The Vienna Circle found in modern logic and in logicism the tool for a new promising conception of meaning.

Logic provided all the meaning which the statements of formal science needed in order to be meaningful. The formal sciences were without empirical content but not meaningless: they distinguished between well-formed and non-well-formed formulae and correct and incorrect operations. The combinatorial value of the symbols used in the expression of their theories lay in the totality of combinatorial operations they could be subjected to in a given system. Being tautologous does not mean being meaningless! Frege, Russell, Wittgenstein and Hilbert had not only opened what were, for the Vienna Circle, promising epistemic paths for the knowledge claims of formal science, namely logicism and formalism, but they also furnished the means to defend empirical science. They did so because the conception of analytic or combinatorial meaning, which they had pioneered, provided the tool to rework verificationism just as science moved further from intuitive experience. (By means of logic, we can generate, from a small set of basic sentences which are epistemically privileged, a structure of sentences all of whose elements bear a relation to what the basic sentences bear a relation to, namely, some grounding which makes them privileged.) The realm of the meaningful was systematically enlarged by this method. Theories as wholes gained empirical support. The new theory of meaning by means of which the Vienna Circle supported empiricism opened 'pure' theoretical concepts to control by experience. What opened this realm was a conception of meaning as arising at least partly from form or structure.

It is against the common background of the deeply anti-intuitional aspect of the linguistic turn that the differences between Schlick, Carnap and Neurath must be viewed. They all agreed that meaning was at least partly a matter of form, but they differed on how
much so and understood the key concept form differently. Their differences showed up mostly with respect to how the meta-linguistic aspect of the linguistic turn was implemented. What did it mean to concern oneself not with the world, but representations of the world? Schlick, Carnap and Neurath had different conceptions of epistemology. While Carnap's may be happily described as an "alternative neo-Kantian" one (in the sense explained below), and Schlick's is already less so, Neurath's conception was most pronouncedly anti-Kantian. Already Carnap and Schlick disagreed on the value of purely formal philosophy. Neurath, by contrast, further challenged the residual Cartesian assumptions of both of their conceptions of knowledge. Their disagreement over now the form is content view was to be understood, gave rise to their conflicting conceptions of scientific philosophy.

The alternative picture of the Vienna Circle drawn here (and substantiated in the body of this essay) rejects the traditional picture of its naive scientism as a caricature. If we can express the traditional picture of the Vienna Circle as "logistically reconstructed Cartesian foundationalists", then the traditional picture fails to represent any of the positions held in the Circle. The Vienna Circle's "scientistic" decision to abandon "first" philosophy--at least as far as the presuppositions of the traditional conception of knowledge is concerned--does not support the contention that they considered the knowledge claim of science to be unproblematic. On the contrary. To account for the objectivity of scientific knowledge in the face of the failure of traditional empiricism and of Kant, was the central problem to which their considerable efforts were addressed.

2.3.3 The New Canon of Vienna Circle Scholarship

That the traditional view stands in need of repair is the view of what I shall call the "new canon" of Vienna Circle scholarship. Its holds that the traditional view fails to acknowledge the Kantian dimension. It holds further that the Vienna Circle's linguistic
turn sought to build on the Kantian idea that objectivity resides in the form of human
cognition by exploring the form of symbolic representational systems.²⁹ Reading Carnap
as an alternative neo-Kantian builds on long recognised elements of Vienna Circle
philosophy, but places them in a wider and philosophically more sophisticated context.³⁰
Reading Carnap as an alternative neo-Kantian means to identify as "neo-Kantian" Carnap's
concern with the a priori conditions of knowledge; what made him "alternative" was his
rejection of the synthetic a priori.

What the new canon says about Carnap and Schlick is indispensable for an
understanding of the background of Neurath's theories. I have freely followed the new
canon in my criticism of the traditional view and added my own, to wit, that there were
more traditional and more radical interpretations of the idea of scientific philosophy even in
its reconstituted Kantian dimension. This means that Schlick, Carnap and Neurath had
different conceptions of the idea that the required theory meaning would be a theory of the
form of representation, and, in turn, that were different estimations of the value of
Carnap's alternative neo-Kantianism. I believe that we must distinguish three positions in
the Vienna Circle. Carnap's epistemological project found critical reaction not only from
Schlick, but also from Neurath. The new canon concentrates on Carnap's thought and
also illuminates Schlick's; it says little about Neurath's third view. Taken on its own, the
new canonical picture might suggest a wider agreement on just how concern with the form
of scientific representation contributed to answering the sceptical challenge than actually

Rungaldier 1984:ch.8 and Moulines 1985. Not particularly committed to reading Carnap as an
alternative neo-Kantian, but of great importance in the wider New Canon of Analytical Vienna Circle
scholarship on which I draw here, are Goldfarb 1979 and Ricketts 1982. For a different perspective,
equally centered around the Circle's philosophy of formal science, but sharply critical of its worth see

³⁰The new canonical view is not without its precursors: already Petzaell 1935:24/5, 49, Cohen 1963:110,
141 fn.95, and Bergmann 1950:13 noted the Kantian element in Carnap's thought.
obtained in the Circle, and thus misrepresent where the actual agreement amongst Schlick, Carnap, and Neurath lay. For Neurath, neo-Kantianism of any sort was a non-starter.

2.3.4 The Continental Neurath Rediscovery

That the traditional view of the Vienna Circle stands in need of repair, is also the view of what I shall call the "Austrian School" of Neurath criticism\textsuperscript{31}, a number of German and Italian philosophers.\textsuperscript{32} (This Neurath revival broadly, European analytical philosophy owes considerable impetus to the editor of Neurath in English, R. S. Cohen.\textsuperscript{33}) The Continental Neurath criticism focuses on the neglect of Neurath's non-standard views in the traditional picture of Vienna Circle philosophy.\textsuperscript{34} By contrast, its proponents point out Neurath's anticipation of the criticism of standard doctrines of Logical Empiricism by such philosophers as Quine, Kuhn, and Feyerabend. This overdue re-evaluation of Neurath's role in the Vienna Circle is complemented by suggestive discussions and explorations of Neurath's general approach to philosophy and of his distinct perspective on how to ground the knowledge claims of science.

What I add to the new canonical criticism of the traditional view of Vienna Circle philosophy stands broadly speaking in the tradition of this European Neurath renaissance. More precisely, I seek to extend the insights provided by Haller's Austrian school and, amongst the German writers, Heidelberger and Koppelberg.\textsuperscript{35} I seek to do so by reading

Neurath, like Carnap and Schlick, as a theorist of "critical scientism", whose distinct radicality was at least in part a function of his rejection of Carnap's alternative neo-Kantianism and Schlick's residual foundationalism.\(^{36}\) (Carnap's view already was different from the one traditionally attributed to the members of the Vienna Circle; it is thus important to clarify the relation between Carnap's and Neurath's views. But Carnap was not the only alternative neo-Kantian in the Circle; we must also differentiate the background to Neurath represented by Carnap and Schlick, that is, we must see Schlick's "foundationalism" in the new context.) I shall argue that Neurath's proposed naturalisation of epistemology constitutes his attempt to come to terms with the problem of epistemic justification by grounding theoretical knowledge claims in the practical contingencies of inquiry. What distinguishes Neurath here from pragmatism in general, and renders his naturalism non-standard still by the measures of Dewey and Mead, is his recognition and exploration of the social dimension of knowledge production and knowledge transmission within the framework of historical materialism.\(^{37}\)

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\(^{36}\)I say "at least in part" here because much of Neurath's anti-foundationalism was fully developed long before Neurath joined Schlick's circle: his 1932d is, after all, the third use of the metaphor by Neurath.

\(^{37}\)This extremely interesting aspect of Neurath's philosophy is only touched upon here in the appendix to ch. 10 and in the conclusion. For avowals of historical materialism by Neurath see, e.g., 1928a, 1930e, 1931c, 1931d, 1932a, 1932b. It seems to me that, least in intention, Neurath's epistemology was "critical" not only in the Kantian but also in the Marxian sense of the term (but see Freudenthal 1989).
2.4 Approaching the Protocol Sentence Debate

I will now introduce the arena in which the differences of philosophical orientation between Neurath, Schlick, and Carnap were played out, and from which their positions must be reconstructed. This arena was the protocol sentence debate. I begin by highlighting the background for the Vienna Circle’s linguistic turn somewhat further and then locate its critical concern with objectivity within it. I then place the protocol sentence debate in the context of the struggle for scientific philosophy.

2.4.1 The Setting of the Protocol Sentence Debate

I noted that the Viennese response to scepticism required a new theory of meaning or representation. An early Viennese answer is presented by Carnap’s *Aufbau*. The conception of language and meaning developed in Carnap’s *Aufbau* provided the background and starting point for the Vienna Circle’s protocol sentence debate. Already in his 1883 work on the history of mechanics, Mach had conceived of a special, universal language of science which would bear the content of its statements on its sleeve, that is, show it in its compositional form: "to read [it] would be to understand [it]."\(^{38}\) Mach’s brief hints towards this universal language of science were fully realised, however, only by the *Aufbau* of 1928. Between Mach and Carnap’s *Aufbau* lies an important shift of meta-scientific method, the "linguistic turn". This was the turn away from psychologistic attention to the act of scientific judgement and its grounds towards the linguistic attention to the representational forms which scientific judgement takes (and to the relations of conceptual support obtaining between several or all of them). The linguistic turn did not occur overnight, of course. It required the development of special analytical tools with which the forms of scientific representation could be investigated.

\(^{38}\)Mach 1883:481/2.
Already the members of Hans Hahn's discussion group from 1907 to 1912, which included Neurath, Frank, and v. Mises and which is sometimes called the "first" Vienna Circle\(^{39}\), were familiar with the initial steps in this logico-linguistic direction: Mach's idea for a universal science of language, Hilbert's axiomatisation of geometry and the extension of his structuralist or formalist view of theories from mathematical disciplines to physics by the French Conventionalists. The decisive step in the logico-linguistic development towards Carnap's Aufbau, however, was the appearance of what Carnap in 1930 was to call "the new logic" in its full logicist splendour. Whereas the first Vienna Circle remained, for all its logical sophistication, essentially rooted in Schroeder's *Algebra of Logic*\(^{40}\), the Vienna Circle proper was most decisively influenced by the logic of Frege and Russell. During the academic year 1924/5, Hahn gave a seminar on Russell's and Whitehead's *Principia Mathematica* and, following a prior groundbreaking presentation by the mathematician Kurt Reidemeister, the Circle began its detailed readings of Wittgenstein's *Tractatus*, previously considered a mere philosophical oddity.\(^{41}\)

The new logic provided the tool for the investigation of representation at its most general. The logical structures erected by the formation and transformation rules of symbolic systems provided the abstract conceptual space within which the phenomenon of representation and meaning could be explicaded. The new logic opened the dimension of logical form. Logical form is that aspect of statements on which deductive logic turns, but also that which concerns the categorical frameworks which, in some sense, underly the

\(^{39}\)Haller 1985. The perhaps earliest though oblique mention of this group can be found in the final paragraph of the section giving the historical background to the circle around Schlick in the collaboratively composed manifesto of 1929 *Der Wiener Kreis*: it notes that "especially since 1900 [...] there was in Vienna a sizable number of people who frequently and assiduously discussed Poincare, Duhem, the foundations of mathematics and the like" (Neurath et al 1929:303). Following Neurath's own recollections of the history of the Vienna Circle (Neurath 1936a, 1936b) Frank twice expanded his earlier comments in his eulogy on Hahn (Frank 1934) in his forewords to two collections of his essays (Frank 1941, 1949). See also Lindemann 1944 and Feigl 1969a:58/9 for further mentions of this group.

\(^{40}\)On Hahn see Menger 1980, on Neurath see Koehler 1982, on the importance of Schroeder see Goldfarb 1979.

\(^{41}\)McGuiness 1967, Feigl 1969a:60/1; Menger 1980:x, xii.
interpretation of languages. (The logical form of a statement is often distinct from its grammatical surface structure.) With Wittgenstein's Tractatus, the question arose whether the logical form of representations could itself be represented. The question whether to adhere to or dissent from Wittgenstein on his position of the unrepresentability of logical form, provided for a major division amongst the members of the Circle. (The pros and cons are discussed in chapter 3.) It is here that the new reading of Carnap in particular comes into its own. What traditionally is viewed as a somewhat technical (though consequential) disagreement in the logicist tradition, now emerges as absolutely central to the project of validating scientific knowledge. If the very possibility of objectivity rests in the form of cognition, then the specifiability of that form becomes the central issue for a philosophy that claims itself to be scientific: science does not speak of the unspeakable! One expression of (the then as yet only implicit) dissent from Wittgenstein's position was Carnap's Aufbau: with mathematical logic available to explicate the concept of logical form, all propositional knowledge became amenable to explication as representation in virtue of its structural properties.

While the Aufbau represents the culmination of the turning away from pure intuition and experiential quality as a source of scientific knowledge, it also raises in a most acute fashion two broad problems which had not yet been answered—even were only raised by--this trend towards the structuralisation of knowledge. These questions concerned, on the one hand, the nature of empirical knowledge and, on the other, the subject matter of logic.

Due to ongoing developments in meta-mathematics, the answer to the question of the subject matter of logic underwent significant change. All too briefly, this development was most broadly one from the Russelian conception of logic as concerned with the

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42 Compare Hahn 1933 and Menger 1933a for detailed expositions of the diminishing importance of "intuition" in the formal and physical sciences; see also Friedman 1983 and 1987 for related observations in the cases of Schlick's and Carnap's philosophies.
specification of the relation between objects in general to a conception of logic as concerned with the specification of arbitrary logico-linguistic frameworks. This development away from the idea of "the true" logic ultimately led to Carnap's principle of tolerance characterising his Logische Syntax of 1934. (If you like, the path led from "the true form of things in general" to the tolerance of "logical possibilities", even the embrace of "logical plurality".) This aspect of the development of the Vienna Circle's views I shall touch upon only when needed to provide relevant background (namely, when discussing how it influenced the stance towards Wittgenstein's position on logical form).

Central to my inquiry is the development of answers to the first question, namely about the nature of empirical knowledge. This is the topic of the protocol sentence debate. In the Vienna Circle, we find broadly foundationalist and radically anti-foundationalist positions, and positions in between. All of them, however, seek to ground the objectivity of scientific knowledge in the medium of its representation. For all of the Circlists, meaning was at least partly a matter of logical form. But just how logical form safeguarded the objectivity of knowledge was the issue on which they disagreed.

Neurath was long familiar with the pre-Principia Mathematica state of the linguistic turn of attention to the forms of scientific representation. Neurath's anti-foundationalist holistic attitude towards knowledge was a result of his confrontation of the state of the latest positivist and conventionalist philosophies of science which the "first" Vienna Circle had studied. His anti-foundationalist holistic attitude (though not perhaps the details of his positive theory) were thus formed long before the time of the Vienna Circle: his Boat had already made two appearances in his writings, the first in 1913, the year after the Hahn's "first" Vienna Circle ended. The protocol sentence debate accordingly presents a

Goldfarb 1979. For Neurath's early endorsement of the Russelian conception, in the form of "Itelson's definition as the theory of things in general", see Neurath & Hahn 1909:5.

For the details of this development see Menger 1979b.

Neurath 1913a:457, 1921:199.
chronicle of the challenge which Neurath's views on the matter of knowledge faced in the Circle, and of his responses to this challenge. The locus classicus of Neurath's challenge to Carnap is the Vienna Circle's protocol sentence debate.

2.4.2 The Themes of the Protocol Sentence Debate

Tradition characterises the Viennese conception of philosophy as consisting in the logical clarification of scientific concepts and justification procedures. I shall argue that it remained an open issue amongst participants of the Circle what the Viennese concern with logical clarification amounted to. Was philosophy turned into the determination of experiential meaning (Schlick), reduced to a purely formal inquiry (Carnap) or rendered obsolete (Neurath)? This was not merely a terminological matter. As I shall show, the protocol sentence debate reveals a deeper disagreement about the the status of philosophy, the nature of epistemology, and the proper conception of the nature of knowledge.

The protocol sentence debate concerned the sentences or statements reporting the evidence for scientific theories, typically the conditions, procedures, and results of experiments, and so secured the objectivity of science. (These Protokolle or reports I shall variously call "evidence statements" or "protocol sentences" or "protocol statements"). On the face of it, the protocol sentence debate simply concerned the logical form and the epistemological status of these statements. At bottom, the debate concerned the nature of epistemology itself. I will show this by showing that the answers given by Schlick, Carnap and Neurath to the questions of the form (and content) and the status of protocol

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46 In my review of the debate I do not, of course, presuppose acquaintance with Neurath's pre-Vienna Circle views but rather seek to prepare the ground for drawing out the views Neurath held during the time of the Vienna Circle from his sometimes rather oblique arguments in the protocol sentence debate itself. (The place of the protocol sentence debate in Neurath's own philosophical development is briefly considered in the concluding section of chapter 9.) Nonetheless, it must at least in passing be noted that the sophistication attributed to Neurath's thought here—an attribution still more controversial than the view of the Carnap here endorsed as the "new canon"—is simply a function of the length and breadth of his involvement with the question of scientific philosophy (or what Neurath had called "Universalwissenschaft" ("universal science") in 1910)
statements reflect their answers to three underlying problems. The discussions of and answers to these questions are progressively more obscure.

First there was the basis problem. (This question concerned the availability of epistemological foundations.) What are the grounds for empirical knowledge claims? Its answer followed from the answers to the concrete questions about protocol statements. The basis problem concerned first the question of the basis upon which the structure of empirical knowledge was to be erected: was it permissible to proceed from the phenomenal given, however abstractly conceived? Schlick and Carnap thought it was, whereas Neurath thought it was not. Second, the basis problem concerned the question whether there were or were not foundations for scientific knowledge. Schlick thought so, Neurath disagreed, and Carnap held various opinions on this matter.

Second, there was the already more obscure problem of the viability of the project of the thorough formal treatment of epistemological questions. This problem divided into two. First, there was the structuralisation problem. This concerned the question whether the formal treatment was at all possible. Second, there was the adequacy problem. It concerned the question whether the formal treatment did or did not exhaust what is of epistemological interest. Combining their answers to the two subproblems, the Viennese answers were these. Carnap thought that the experiential aspect of the cognitive process could be ignored, Schlick thought it could not\textsuperscript{47}; neither did Neurath, but for different reasons. He thought that the practical context of use needed to be examined as well\textsuperscript{48}.

Behind the second question yet another question loomed. What were the guiding conceptions of knowledge which dictated the epistemological perspectives and the concrete positions on the questions of the content, form, and status of protocol sentences which the

\textsuperscript{47}Friedman 1983 argued that Schlick continued to be torn about whether this was the right answer; that noted, I shall concentrate my exposition on his tendency to provide a negative answer.

\textsuperscript{48}Neurath et al 1929: last sentence.
disputants adopted? What was the kind of scepticism thought to be at issue, and how was it to be avoided in answering the previous two problems in the ways they did? The third question, then, concerned the disputants' underlying assumptions about the nature of knowledge itself. The disputants' answers to this third question are only indirectly revealed by their particular way of responding to the basis, the structuralisation and the adequacy problem.

The three problems represent different levels on which I investigate the protocol sentence debate. There is the level of first-order epistemology, the theories of scientific knowledge put forward. Then there is the level of meta-epistemology which concerns the conceptions about the proper pursuit of epistemological inquiry itself. Finally, there is the level of broad guiding ideas which inform the meta-epistemological positions. I shall conclude that it was concern with this third problem which animated the protocol sentence debate. The lasting interest of the debate—and the reason for which I shall engage in re-reading it—lies in these meta-epistemological matters.

2.4.3 The Structure of the Debate

In my interpretation of the protocol sentence debate, I categorise the contributors somewhat coarsely as either foundationalists (Schlick) or naturalists (Neurath), and place them against Carnap's changing intermediary positions. (That Schlick's foundationalism was not a naive one will be noted repeatedly.) My interpretation of the protocol sentence debate differs in content from others both in overall picture and in details. I do not portray the debate as taking place within one monolithic common conception of scientific philosophy, the one traditionally ascribed, but portray it as the clash of three different conceptions of scientific philosophy.
My interpretation of the protocol sentence debate differs from existent accounts also in dating. My dating of the debate as reflected in the Circle's publications is as follows. The starting point and background of the debate is provided by Carnap's Aufbau, published in 1928. The debate which followed came in four stages. This division reflects the different answers given by Carnap to the basis problem.

**Stage one** started with the dissent from the epistemological position of Carnap's Aufbau: it featured both foundationalist and naturalistic criticisms of this position and extended until 1930. That year Schlick published his anti-formalist "Wende", and Carnap in effect conceded Neider's and part of Neurath's criticism. These stage one criticisms challenged the way Carnap made use of "the given" in his epistemological reconstruction, though they did not address the scientific protocols themselves.

**Stage two** squarely focussed on the question of the linguistic form, content, and status of scientific protocols. It featured (with the exception of Zilsel's contribution) only exchanges between Neurath and Carnap. At issue between them was the proper understanding of the position of physicalism originated by Neurath and adopted by Carnap in a modified form as his response to Neurath's criticism. This stage two extended from (at the latest) early 1931 until the very end of 1932 when Carnap conceded yet more, but still not all, to Neurath.

**Stage three** marks Schlick's foundationalist response to the physicalist view now broadly shared by Neurath, Carnap, and Hempel. This stage three lasted from 1933 until 1935. The Carnapian version of physicalism operative here was that arrived at the end of

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49My reconstruction does not rely on protocols of the Circle's Thursday evening discussions. Comparison of the debate as recorded in publications with these discussions, though highly interesting at certain junctures (e.g. 1930-1931), would not be able to corroborate important claims made here due to Carnap's absence from Vienna (except for short visits) since the autumn of 1931. That said, I must add that I fully expect these protocols and the correspondence between the members to corroborate my interpretation of the debate, what is known of it in the literature corroborates my account. (See e.g., Haller 1982d: xxi and Hegselmann 1985.)

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stage two. Neurath's and Carnap's differences were not publicly discussed. Stage three extended until 1935 when Carnap adopted Tarski's semantic theory of truth.

Stage four began with Carnap's attempt to reconcile Schlick and the physicalists at the First International Congress for Scientific Philosophy in Paris in 1935 and lasted until it petered out around 1940/1. Carnap's semantic position rapidly found wide adherence and became the basis for the "received view" of scientific theories which dominated Anglo-American philosophy of science until Quine's internal critique of Logical Empiricism and Kuhn and Feyerabend's external critique of it. (Both these criticisms were anticipated by Neurath.) Carnap's position did not, however, find agreement with the parties he sought to reconcile, Schlick and Neurath. In so far as it remains true to speak of a "protocol sentence debate" here, this stage four mostly featured ongoing criticism of the by-now abandoned broadly physicalist position; what makes it interesting are Neurath's engagements. (Neurath's underlying dispute with Carnap was continued to any extent only in their correspondence until his death in 1945.)

How does this dating relate to the existing accounts of the protocol sentence debate? Though often remembered as a debate between the physicalist Neurath (and Carnap and Hempel) and the foundationalist Schlick (what I call 'stage three'), it is known that this engagement was preceded by a debate between Neurath and Carnap (what I call 'stage two'), though opinions differ on quite when the debate began. I argue, first, for an

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52 Such impressionistic memories may well take their start from Hempel 1945:n.50 who is concerned only with the logical relation of confirmation between statements and less their content and status, from Scheffler 1967:ch.5 who for thematic reasons of his own focussed his review of the protocol sentence debate exclusively on Neurath and Schlick, from Belke 1967:24 who is concerned only with Schlick, or from Hilpinen 1982:n.1.
earlier than usual dating of the beginning of stage two and, second, for the recognition of a yet prior stage of discussion (what I call 'stage one'). Recognition of the earliest manifestation of stage two is necessary to appreciate the ongoing nature of Neurath's dispute with Carnap from his earliest highly oblique published criticism of 1928 to his progressively more explicit criticisms of 1932 and later asides. Recognition of stage one is necessary to see the continuity of Schlick's stage three criticism with his earlier views and is necessary to see the unbroken continuity of Neurath's already recognised stage two criticisms with his earlier pre-Vienna Circle position. The correct dating of the various stages of the debate is important because it provides the first step for an appreciation of the continuity of the positions of all the participants. (For a defense of my dating of the stages of the debate see the Appendix to Chapter 1.)

Speaking in terms of the content of the debate again, I argue that none of the stages of the debate resulted in agreement. It follows that my interpretation differs in content from existing ones not only in the sharp differentiation of the three perspectives on scientific philosophy, but also in the recognition of additional facts about the conflict between them. First, already during stage 1 the disputants disagreed about the matters raised and remained divided on them. Second, stage two did not result in agreement in all relevant aspects (even apart, that is, from the issue of the preferred form of protocols). The continuity in the positions held by the participants in the debate, even despite some spectacular changes,

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54 Only Heidelberger 1985:174 so far took note of one of Neurath's earliest criticisms and Joergensen 1951:69 vaguely implies that Neurath never agreed with the Aufbau conception.

55 Nobody, to my knowledge, has so far commented on the conflict between the positions of Carnap and Schlick during this stage one.

56 Koppelberg 1987 is in agreement with this point but needs to be supplemented with a detailed diagnosis of the remaining disagreement (as he must be quite generally on the point of the significant differences between Neurath's and Quine's naturalised epistemologies, despite the former's wide ranging anticipation of the latter.)
strongly suggests that a deeper background issue moved them. This background issue was, of course, that of the conflict of the three perspectives on scientific philosophy and epistemology I outlined earlier. Ultimately, the protocol sentence debate can be understood only against the background of these differences over the kind of theoretical approach to be taken to the project of validating empirical knowledge: the issue of the form, content, and status of protocols was but the focal point of their larger-scale disagreement.

All that said, let me note two simplifications I make for the sake of perspicuity in my reconstruction of the debate. First, I have consigned Schlick’s responses to stages one and three: strictly speaking, Schlick’s "stage one" criticism, often implicit, extended over stages one and two of the debate. Second, there is Popper’s role in the debate. From what I can see it is smaller than is often assumed, but it is in any case not easily fitted into my schema. I shall include his positive suggestion to Carnap in stage two and deal with what may have been Popper’s arguments against Carnap in an extra section following the review of stage three. There I shall also give reasons for my judgement of their importance.

I must also enter this caveat. My review of the debate does not aim for completeness; what it aims for is to provide the basis for an improved understanding of Neurath. With the exception of Carnap’s semantic position from 1935, the fourth stage of the debate will for present purposes be mostly disregarded even though this means that I must disregard, for the main part, Neurath’s stage-four contributions to the debate. For thematic and organisational reasons these later writings of Neurath are best considered only

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57 Passmore 1967:56 correctly notes that the protocol sentence debate was a debate between three positions, and that Neurath never agreed with Carnap, but does so at a considerable level of abstraction from the details of the debate. In order to support this position I found that one must add to Passmore’s sketch (i) the recognition of the pre-1932 debates, (ii) the recognition of the underlying differing philosophical orientations for each of the positions of Schlick, Carnap, and Neurath and their interrelations and conflicts (broadly along the lines of the new canon and the Austro-German scholarship, and (iii) present a philosophical argument in detailed explication of Neurath’s persistent opposition to Carnap.
after his earlier contributions to the debate are properly reconstructed from their context: they are best dealt with on another occasion when Neurath's positive theory of knowledge will be more fully explicated.58

2.5 Preview: Three Viennese Conceptions of Scientific Philosophy

My review of the protocol sentence debate will be long and detailed, but here is, by way of preview, a sketch of the three different conceptions of scientific philosophy espoused by Carnap, Schlick, and Neurath. The traditional picture of Vienna Circle philosophy stands in two-fold need of correction: once to incorporate Carnap's neo-Kantianism (and Schlick's tendencies in this direction), and once to take account of Neurath's naturalism. I build on the new canonical reading of Carnap for the first amendment and on the Austrian school of Neurath criticism for the second.

2.5.1 The Applicability of the New Canonical Picture: Carnap's Alternative Neo-Kantianism

I support the new canon of Vienna Circle scholarship in my characterisation of Carnap's philosophical orientation. Carnap's overriding philosophical concern was to show the possibility of objective knowledge, not to demonstrate its actuality. His main concern lay in the investigation of the a priori conditions on knowledge, and thus possessed a greater affinity with the Kantian project than recognised by the traditional view of Vienna Circle philosophy. I interpret Carnap as an alternative neo-Kantian. This means to identify as "neo-Kantian" Carnap's concern with the a priori conditions of knowledge; what made him "alternative" was his rejection of the synthetic a priori.

58 See section 8.1 for an overview of stage four and 8.4 for Carnap's stage four position. Note the qualification: the detailed reconstruction of Neurath's positive theory would indeed require extensive reference to his stage four contributions.
For the new canon it is no coincidence that, in his doctoral dissertation of 1921, Carnap still endorsed the Kantian view of space as a synthetic a priori form of intuition. By the time he joined the Vienna Circle, of course, Carnap no longer endorsed the synthetic a priori, but still sought to explicate the a priori conditions of knowledge in purely formal, that is, analytical terms. Carnap's formalism was a development of a Kantian idea, it was not merely a logically refined retread of Hume's associationist theory of ideas. Carnap's concern with the logical form of languages sprang from his epistemological concern to show the very possibility of knowledge. Over the course of his long career, Carnap changed his mind about the nature of the calculi appropriate to the representation of scientific theories, but he retained the strategy of language construction as an approach to philosophical problems throughout. Carnap's philosophical work as a language constructor has to be understood as his pursuit of the analytical a priori of cognition.

2.5.2 The Applicability of the Foundationalist Picture: Schlick's Concern with the Actual Justification of Knowledge Claims

The new canon correctly recognises that the alternative neo-Kantian conception of objectivity as form of cognition was in fact pioneered by Schlick. The question in what sense he could still be called a "foundationalist" arises therefore not only in the case of Carnap but also in the case of Schlick. The traditional picture of the Vienna Circle as concerned with the facts and limits of knowledge, not just the mere possibility of knowledge, is not entirely wrong, of course. That, in a way, is just how Schlick is presented in this essay. The difference from the traditionalist picture is that Schlick was a foundationalist against the background of his confrontation of Kantian questions. This is extremely important. Schlick, in fact, was one of the pioneers of the conception of combinatorial or structural meaning. Schlick's early struggles in his pre-Vienna Circle

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59 Carnap 1922:63.
Allgemeine Erkenntnistheorie, to discern what makes for objective knowledge, speak against the suggestion that his was a pre-critical epistemology.60

The development of Schlick's philosophy of natural science exemplifies a concern that is very prominently displayed in the traditional picture of Vienna Circle philosophy. That is the justification of appeal to apparently merely hypothetical entities in science. Schlick sought to show not only how such appeals could be in principle justified but how they were in fact justified. The difference here is that he was concerned not only with what makes knowledge possible, but also with showing that we do actually have knowledge. The overriding concern of Schlick's during his pre-Vienna Circle theorising was to show that the special and general theory of relativity was indeed an item of genuine scientific knowledge.61 Both neo-Kantians and reductionist positivists denied (for different reasons) what Schlick sought to establish as a fact, namely, that relativity theory constituted the only genuine knowledge about space and time there was. In his papers on relativity theory, Schlick moved from the view that empirically equivalent theories are equally true62, to one which ascribes true reality claims only to theories whose concepts were grounded in observation by deductive relations (no matter how complicated) to observation statements, and thus denied theories relying on notions like absolute space and time their claim to knowledge.63

Schlick's effort to exhibit relativity theory as a bona fide example of scientific knowledge thus fully coincides with the traditionally recognised aim of the Vienna Circle: to comprehend the increasing complexity of scientific theories and show that, despite their

60See below section 4.1.
61Schlick 1915, 1917, 1921, 1922.
62Schlick 1915:172, 1917:266. My reading here contradicts Friedman 1983:503: as far as I can see he overlooked Schlick's qualification of the claim (which led to Friedman's assertion here contradicted) that the aether hypothesis is senseless and “without physical meaning”, namely for scientists who accept relativity theory (Schlick 1915:185).
63Schlick 1922:345/6.
apparently counter-intuitive results, they remained indeed sufficiently grounded in empirical fact. That Schlick's concern lay not with the mere possibility but with the demonstration of the actuality of knowledge makes his the more traditional of the two views of the alternative neo-Kantian wing of the Circle.

2.5.3 The Applicability of the Austrian Perspective: Otto Neurath's Conception of Scientific Philosophy

Neurath's views on the task and nature of epistemology are, if anything, even less known than Carnap's alternative neo-Kantianism and Schlick's more traditional views. (Even Neurath's oeuvre itself is still being discovered and is only gradually becoming more accessible.) Neurath's interest in epistemology was not purely theoretical or academic. I must defer to the conclusion my placement of his epistemological theory in the wider context of his emancipatory interests; here I can only indicate how his practical orientation found expression in his epistemological perspective.

That Neurath sought to ground the knowledge claim of science in practical considerations and thus reorient the theory of knowledge, is a point made by the Austro-German critics. I shall argue in support of this thesis and seek to determine what

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64 Neurath's written output is voluminous by any standards. A first bibliography lists 278 original articles and books (Neurath 1973; my counting includes two short selections first published there), to which 61 more original articles listed in a supplementary bibliography (Neurath 1983) must be added (as well as some in Stadler 1982a). Yet more additions are forthcoming. Koppelberg 1987 noted Neurath 1946a, an important find. I suggest the following additions. There are a handful of abstracts of lectures given in 1931/2 and published in Erkenntnis of which, I would urge, Neurath's authorship is highly likely (my numbering, see bibliography): Neurath 1932e, 1932f, 1932g, 1932h. (For their importance see Appendix to Chapter 1.) I also suggest adding Neurath 1906, 1930d, 1939/46, Neurath & Schumann 1919 and Neurath & Frank 1930.

65 Apart from his contributions (Neurath 1938 and Neurath 1944) to the International Encyclopedia of Unified Science series, which he founded (see Morris 1973), there are four collections of his writings currently in print (Neurath 1973; Neurath 1979; Neurath 1981 and Neurath 1983) and several reportedly in preparation: Hegselmann's German edition of the Carnap-Neurath correspondence, Haller's German edition and Cohen's English edition of Neurath's economic and a German edition of his picture-theoretical writings are all in various stages of pre-publication.

66 Rutte 1982b, see also Nemeth 1981. Heidelberger 1985 comes closest to perceiving this aspect of the Neurathian third perspective on scientific philosophy, and in so doing is alone amongst the more general
Neurath's practical orientation amounted to. First of all, note that the Austrian point about Neurath coheres with the new canonical view of Carnap and Schlick. Neurath's practical turn was also a "critical" position, already in its Kantian sense. To see this, consider this striking but little-noticed fact: Already in 1913, on the occasion of his first Boat, Neurath presented the anti-foundationalist holistic conception of knowledge, which he endorsed with his simile, explicitly as an alternative to the views of Descartes and Kant.67

What then did Neurath's practical orientation amount to in comparison with Carnap and Schlick? Like his colleagues, Neurath took his cue from science: scientific theorising presented a picture of rational thought whose rationality was intelligible to every participant; ideally, no one had to acquiesce in its findings by faith in a higher authority. It was in his understanding of scientific rationality that Neurath differed from his colleagues in important respects. For Neurath, it was imperative, as it was for Carnap, that the knowledge claims of science be shown to be in principle justified; it also was imperative, as it was for Schlick, that they be shown to be in fact justified. Above all, however, it was imperative that the rationality of science be shown practically intelligible in its actual pursuit. Neurath asked wherein lay the possibility and actuality of intersubjective control of scientific assertions. Neurath demanded that the justification of scientific knowledge claims reflect the practical requirements of rational cognitive conduct. Neurath stressed that the account of how epistemic agents came to be justified in their claims had to make these grounds

Vienna Circle studies. Heidelberger juxtaposes the traditional reading with one that attributes to the Vienna Circle the desire to establish the unity of science and life (ibid.:158). The fragmentation of cognition into common sense and scientific kinds here to be overcome is of course another aspect of the already traditionally noted "crisis of science", challenging its bearing on the world of everyday experience. Heidelberger associates the concern to re-unify science and life with the Circle in general; by contrast I am inclined to associate it with Neurath in particular. Koppelberg 1987:22/3 designates Neurath's insistence that the actual practice of science be investigated (without noting the need to re-unify "life" and science) as Neurath's anticipation of naturalistic epistemology (whereas Heidelberger does not mention Neurath's naturalism in this connection). This characterisation of Neurath's naturalism's important but too narrow. It fails to stress the acquiescence into the circularity of explaining science by science and Neurath's distinctive conception of how to do so.

67Neurath 1913a:455ff.
intersubjectively available. That meant that any knowledge claims had to be justified on
grounds that are intelligible to others besides the claimant. The objectivity of scientific
knowledge had to be derived, in one way or another, from the intersubjective controllability
of scientific statements, the practical condition of intersubjective understanding.\(^6\)

Neurath's Boat expresses what Neurath believed was the only realistic conception
of the justification of knowledge claims, one which did not make indefensible idealisations.
Carnap's and Schlick's alternatives ran roughshod over the constraints under which human
knowledge was acquired and defended. In a somewhat hackneyed double pun: Their
reconstructed justifications "missed the Boat".

2.6 Overview of the Course of Inquiry

Here, briefly, are the points my chapters seek to establish. Chapter 3 introduces
and investigates the specific background against which the debate took its start; I argue
against the traditional and for the new canonical reading of Carnap's Aufbau. Chapters 4
and 5 deal with the first stage of the debate. Chapter 4 argues that Schlick disagreed with
Carnap's ideas about how to justify knowledge claims and specifies their two main points
of disagreement. Chapter 5 argues that Neurath too disagreed with Carnap, determines the
nature of his dissent, and specifies Carnap's first concessions. Chapter 6 provides the
somewhat changed background against which the second and third stages of the debate
proceeded and argues that, despite superficial appearances, Carnap's "syntactic" theorising
and Neurath's adoption of it did allow for theorising about meaning. Chapter 7 deals with
stage two. It shows how Neurath argued against Carnap, specifies Carnap's further
concessions and their remaining disagreements. Chapter 8 deals with stage three. It
determines the nature of Schlick's objections to the physicalist positions, his own

\(^6\)If this sounds vaguely Davidsonian, it cannot be helped. Important aspects of the conclusions which
Davidson (1982) was led to by reflection on the protocol sentence debate were, it could be argued,
anticipated in the debate by Neurath; they are, in any case, congenial to Neurath's perspective.
alternative and the physicalist responses, describes Carnap's semantic turn which marked the beginning of stage four of the debate, and evaluates Popper's contribution to the debate.

With the review of the protocol sentence debate from stage one to the beginning of stage four concluded, I then proceed to prepare the ground for the reconstruction of Neurath's positive proposals. In this essay I only offer a sketch of Neurath's concept of a naturalistic epistemology and the formulation of some concrete problems his theory must overcome. I must postpone for another time a full account of Neurath's positive theory. How such a full reconstruction, which I do not actually undertake, would build on the result of my re-reading is what I show in the third of my three concluding chapters 9 through 12. In chapter 9, I provide a systematic analysis of Schlick's Carnap's and Neurath's epistemologies in terms of their answers to the three concrete questions about protocols and the solutions to the three underlying problems, noted above. With the results of my investigation in hand, I shall then fill out the sketch of the distinguishing characteristics of the three perspectives on scientific philosophy which I provided in this introduction. In chapter 10, I place the Viennese concepts of empirical justification—so far considered historically in chronological (chs. 3-9) and systematic (ch. 9) order—on the ahistorical grid of contemporary epistemological taxonomy. So informed I begin the evaluation of the Viennese answers and establish in what respects they remained underdescribed and where more research is required before judgement can be passed on the adequacy of the solutions proposed. With Neurath's intention to provide an alternative conception of epistemology and knowledge and its ill fit in common epistemological taxonomies firmly established, I undertake, in chapter 11, an additional review of the puzzled and puzzling reactions to which Neurath's naturalism was subjected already in the Vienna Circle by opponents and fellow physicalists alike. These questions raised thereby concern aspects of Neurath's naturalism which are essential to the success of his project. Already without presenting Neurath's considered answer in full detail, however, and just
on the basis of the reconstructive and analytical results presented here, I can provide, in the brief concluding chapter 12, a sketch of Neurath's conception of naturalistic epistemology which throws into relief his distinctive reading of this program.

Though even my limited project here is still a lengthy one it is not, I believe, an exercise of excessive thoroughness. The philosophers of the Vienna Circle were not as unsophisticated as common presumption would have it. A refutation of common presumption cannot be accomplished in a summary fashion but requires a detailed re-reading of primary sources. My re-reading will bring out not only the original positions and intentions of the Vienna Circle, but also, along the way, one explanation for the wide currency of the mistaken presumption. Despite apparent surface agreements with contemporary usage, the Vienna Circle's philosophical language and terminology is at times highly misleading. The need to get behind these appearances explains the detailed attention I give to some matters which may appear to be marginal to the protocol sentence debate at first. Only after the original meaning of Vienna Circle terminology has been recovered does it become possible to show that it is not "safe to conclude that the positivists have little to offer in the direction of a viable coherence theory" of justification, and to argue, as I do, that Neurath presented a promising anti-foundationalistic response.⁶⁹

⁶⁹Bonjour 1985:214. It must of course be admitted that calling Neurath's a "coherence" theory of justification is less than totally correct description of his highly complex proposal. (See sect. 11.3.) Yet note that on account of his own so-called "coherence" theory of justification, BonJour is hardly in a position to require that all coherence theories be, as it were, "pure" coherence theories.
Appendix to Chapter 2: On the Dating of the Stages of the Debate

My contribution to the historiography of the Vienna Circle in this essay lies in the excavation of its content, not its dating. The proof of it must lie in the sense which it allows me to make of the debate. Nevertheless, to justify my novel way of dating the debate, I consider briefly the objection that the existence of "stages" one and two is twice contradicted by Neurath himself, and I give the reasons for my dating of stage two.

First, the doubts about the existence of stage two. There is Neurath's reference, at the end of "Protokollsätze", to "[t]he discussion begun here". This phrase is not to be read as referring to the whole debate over the empirical ground of empirical theories, but instead only to a particular part of the debate, namely, that about the specific form of the non-phenomenalist protocol sentences: previous contributions of Neurath's did not address this point in great detail. Such a reading is supported by the opening sentence of Carnap's reply: "Otto Neurath has re-opened ["von neuem aufgeworfen"] the question of the protocol sentences in the preceding article." The next question concerns when the debate was "opened". This question splits into two: when did the stage begin to which "Protokollsätze" belonged, and was it preceded by a still earlier one?

To take the latter question first. The existence of this first stage may be challenged by reference to the 1929 manifesto's claim that all knowledge "rests on what is immediately given". This phrase suggests agreement between the signers of the manifesto (Neurath, Hahn, Carnap) about the appropriateness of a phenomenalist conception of the empirical base of science, that is, agreement about just what was disputed in the protocol sentence debate. Contrary to this, I suggest that the manifesto represented an unhappy compromise

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70Neurath 1932d:99.
71Carnap 1932e:E215, my italics; compare also his 1961:vii.
72Neurath et al 1929:309.
with respect to both the topic of political partisanship of the "scientific world conception" and the preferred epistemological strategy. I read Neurath's acceptance of the phrase quoted as his contribution to the Circle's unified front against "school-philosophy". It is often noted that the thinly veiled political tone of the manifesto could not have been avoided without affronting Neurath; what is overlooked is the possibility that Neurath made significant doctrinal concessions in turn.\footnote{Menger 1982:92 obliquely suggests as much.} Both the question of the political partisanship of scientific philosophy and the question of wherein the empirical ground of science consisted were (as yet) only matters for internal discussion.

Recognition of stage one recommends itself once the fully formed anti-foundationalist holistic pre-Vienna Circle position of Neurath's is taken into account. How reasonable would it be to think of Neurath as shelving his central conviction and agreeing with Carnap's (partly) foundationalist \textit{Aufbau}-conception, not only in public for the sake of the Vienna Circle's united front against the "school philosophies"--as he did in its founding manifesto--but also in their less public discussions? That it would not be reasonable to do so is suggested by three documents, Neurath's 1928 review of the \textit{Aufbau}, his 1929 address to the Circle's first conference in Prague, and Neider's recollections of discussions with Carnap and Neurath.\footnote{See chapter 5 below. Pertinent to my dating of the first stage of the protocol sentence debate is also a later footnote of Neurath's which stressed that the 1929 Prague addresses by Hahn, Frank and himself all bore on Carnap's \textit{Aufbau} "without identifying with it in detail" (Neurath 1937b:fn.1). As we shall see Neurath was critical of the epistemological approach taken by Carnap there.} Parallel considerations apply in Schlick's case. The position he defended against the physicalists in 1934 and 1935 was continuous with that in his 1930 "Wende", but differs in important respects from that taken by Carnap in his \textit{Aufbau}.\footnote{See chapter 4 below.}

What distinguishes stage two from stage one is, as I have already noted, the explicit focus on protocol sentences during stage two. Recognition of the beginning of stage two as dated here to 1930, at the latest early 1931, recommends itself for the following three
reasons.\textsuperscript{76} The first reason for thinking that the debate between Carnap and Neurath about the proper understanding of physicalism was already in full swing before 1932 is a remark of Carnap's in a paper in an issue of \textit{Erkenntnis} published early in 1932 (and yet prior to that containing Neurath's "Soziologie im Physikalismus"): 

In the theory of knowledge it is customary to say that the primary sentences refer to "the given"; but there is no unanimity on the question what it is that is given. At times the position is taken that sentences about the given speak of the simplest qualities of sense and feeling (e.g. "warm", "blue", "joy" and so forth); others incline to the view that basic sentences refer to the total experiences and similarities between them; a still different view has it that even the basic sentences speak of things.\textsuperscript{77}

Carnap did not say who held those views, but it is not difficult to determine this. The first view was of course Mach's and Russell's and had recently been revived by Kaila in his criticism of Carnap\textsuperscript{78}; the second view was Carnap's own as put forth in the \textit{Aufbau}, and the third view was Neurath's.

The second reason for holding that the debate between Carnap and Neurath was in full swing by 1931 is that Neurath's paper "Physikalismus" was published in the November 1931 issue of \textit{Scientia}. In this paper Neurath first presented his argument against Carnap's (methodological) phenomenalism which was repeated in various publications over the next years.\textsuperscript{79} But even the publication of this paper was not the beginning of the second stage of the debate. Hints towards this conclusion are contained in this and in another paper of 1931, "Weltanschauung und Marxismus" (which makes the anti-phenomenalist argument more obliquely), Neurath makes implicit references to earlier

\textsuperscript{76}Importantly, one reason commonly given for dating the beginning of the protocol sentence debate to 1931, when it is so dated (Schnittler 1980:17n., Barone 1982:337, Laeener 1983:114, Koppelberg 1987:20) is not a good reason: Neurath's "Soziologie im Physikalismus" was not published until 1932, despite the inscription on the cover of vol.2 of \textit{Erkenntnis} to the contrary. It was published in issue 5/6 of volume 2 which like still earlier issue 4 of the same volume was not published until 1932. See the bibliographical remarks in Benson 1963:1023 (Carnap had papers in either issues).

\textsuperscript{77}Carnap 1932a:63. This paper in fact was a revised version of a lecture delivered in November 1930 in Warsaw.

\textsuperscript{78}Kaila 1930; see also Carnap 1931a:77.

\textsuperscript{79}See chapters 7, 9 and 11 below.
lectures by Carnap given (partially) under the auspices of the Verein Ernst Mach. The series to which these lectures belonged constituted an early forum for the debate between Carnap and Neurath.

The existence of this lecture series provides the third reason for my dating of stage two of the protocol sentence debate. It was organised jointly by the Verein Ernst Mach and the Wiener Volksbildungsverein (Vienna Adult Education Institute) and entitled "Probleme der Einheitswissenschaft" ("Problems Unified Science"). What is important about this lecture series is that three of the lectures given here turned up as papers in Erkenntnis—in which capacity they are recognised to constitute central documents of the protocol sentence debate. They were: Carnap's "Die physikalische Sprache als Universalsprache der Wissenschaft" (translated as "The Unity of Science"), Carnap's "Psychologie in physikalischer Sprache" (translated as "Psychology in Physicalistic Language"), and Neurath's "Soziologie im Physikalismus" (translated as "Sociology in Physicalism"). The abstracts of Carnap's lectures clearly express the positions of the later published papers and so does the abstract of Neurath's lecture: it contains a clear attack on Carnap's position of protocol sentences "needing no justification".

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80 The first hint is this. At both Neurath 1931b:[G420] and 1931e:408 Neurath's preferred language form is described as " intersubjective" and " intersensual" — that is, with full quotation marks around them. I take this pair of unattributed quotations to indicate a reference to Carnap's lecture "Die Sprache der Physik" — an "early version" of Carnap 1932b (Benson 1963:1023) — given March 1, 1931 whose published abstract featured this very pair of (nominalised) terms (Carnap 1932f). The second hint is Neurath's reference (at his 1931b:[G420]) to "Carnap's remarks" — without citation of place — to the effect that introspective feeling statements could not be used as scientific statements and his suggestion that they are, though more vague, of equal acceptability as other statements. I take this to constitute a reference to Carnap's lecture "Psychologie in physikalischer Sprache" — "based on" Carnap 1932c (Benson 1963:1023) — given March 8, 1931 whose published abstract contained remarks to this effect (Carnap 1932g).

81 See Erkenntnis 2:82 for the program and ibid.:310-312 for the abstracts of the lectures. Stadler 1982b reports on this lecture series in the course of his review of the activities of the Verein Ernst Mach, but fails to note its connection to the Circle's protocol sentence debate.

82 These are the pairs: Carnap 1932f and 1932b, Carnap 1932g and 1932c, Neurath 1932f and 1932a.

83 Carnap 1932b:42.
Every new statement must be compared with the system of previous statements and laws. Either it will be integrated or rejected as incorrect, unless the whole system will be changed.\textsuperscript{84}

There is no evidence that Neurath drew the Carnapian distinction between scientific system statements in the physicalistic language of science and protocol statements outside of the system language, though involved in the production of scientific system statements.\textsuperscript{85}

Thus one is forced to conclude that already the lecture series of February-March 1931 provided an arena for the debate between Carnap and Neurath about physicalism, and thus for stage two of the protocol statement debate. Given, furthermore, that already in 1930, Carnap acknowledged the need of science (not epistemology) for physicalistic protocols—his response to Neurath’s and Neider’s criticism during the preceeding stage 1 of the debate—stage 2 of the protocol sentence debate started "about 1930", the date given by Carnap for the inception of "physicalism".\textsuperscript{86}

\textsuperscript{84}Neurath 1932f; compare 1931b:[G419] and 1931d:408.
\textsuperscript{85}See sections 7.3.2 and 7.6 below.
\textsuperscript{86}For further specification of the end of stage one and the changed framework of stage two, see section 5.5 and chapter 6 below.
Chapter 3

THE BACKGROUND OF
THE PROTOCOL SENTENCE DEBATE:
CARNAP'S AUFBAU

Abstract: Overview of the main points of Carnap's Der logische Aufbau der Welt (3.1). Carnap's decisions concerning the form of his reconstruction of empirical knowledge (3.2). Consideration of the interpretative question of what the main point of Carnap's project was: against the traditional and for the new canonical reading (3.3). Some tensions in and one fatal difficulty with Carnap's project (3.4). Summary of my interpretation of the Aufbau and synopsis of the kind of criticism made of it in the protocol sentence debate (3.5).

In this chapter I begin the review of the Vienna Circle's protocol sentence debate by focussing upon Carnap's Der logische Aufbau der Welt, the first major work commonly associated with the Vienna Circle.¹ My concern with the Aufbau is only that of providing the kind of familiarity with its doctrines which is essential to the understanding of the protocol sentence debate which took its start from it.² (Schlick's and Neurath's early dissent from the position taken there, will be discussed in the two subsequent chapters.)

¹Strictly speaking, it belongs to the non-public phase of the Vienna Circle for it was published in 1928, still one year prior to the the Circle's official inception. It nevertheless bears the clear mark of Vienna Circle theorising. In 1925 Carnap had presented drafts of what was to become the Aufbau to Schlick's circle and, presumably on the strength of this was offered an appointment at Vienna university and invited to join the discussion group (Feigl 1969:61.)
²For general descriptions of the Aufbau and criticisms of it see Kraft 1950, Joergensen 1951, Goodman 1951 and Rungaldier 1984.
3.1 Carnap as Language Constructor: Overview of the Aufbau

Carnap pursued the aim traditionally ascribed to the Vienna Circle—furnishing an account of the nature of scientific knowledge adequate to the then latest advances—and his own, more recently recognised aim—accounting for the possibility of objective knowledge—by developing constructed languages for scientific disciplines. Carnap did not, then, defend the knowledge claim of science by analysing the languages which science actually used. Over the course of his long career, Carnap changed his mind about the nature of the languages appropriate to the representation of scientific theories, but not about the philosophical strategy of approaching problems by the method of language construction.

The point of this method was to provide "rational reconstructions" of the logico-linguistic basis of scientific theories. Carnap did not primarily construct these languages for actual use either, but merely to exhibit in a perspicuous way the meaning of scientific propositions and how they could be justified. Carnap was interested in the frameworks which made knowledge possible.

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3See above sects. 2.3 and 2.5.1.
4Carnap's work as a language constructor fell into three distinct periods. Each one is marked by somewhat different interests The first period extended until 1930: during this period he was specifically concerned with the foundations of empirical science, particularly physics. Starting in his dissertation with the analysis of the concept of space, and soon thereafter including analyses of causality and time, Carnap published his first comprehensive model of the construction of the concepts of physics in 1926 (Physikalische Begriffsbildung) and followed with the Logischer Aufbau in 1928, aiming for yet greater constructional detail and scope of explication: now he meant to comprehend all of empirical science. The models of scientific knowledge developed in these works proceeded from the phenomenal given. (In the latter Carnap expressed the view that a similarly comprehensive construction could also be achieved with the physical language as a basis. More on this claim in this and later chapters below.) This first period could thus be very broadly called his "foundationalist" period. Carnap's second period took its start from Goedel's report of his method of formalising meta-mathematics by correlating numbers with signs and expressions and culminated with the publication of Die logische Syntax der Sprache in 1934. The main thesis of this period was that a complete description of the logical features of a language could be given entirely in terms of formal properties: no adversion to the meaning of its expressions was required. This thesis applied first and foremost to the languages of formal science, where all truth was logical truth, but Carnap extended it also to the languages of empirical science. (More on this in chapter 6.) This second period of Carnap's is best called his "syntactic" period. Carnap's third period began with his acceptance of Tarski's theory of truth in 1935. Much of Carnap's efforts during this period went to developing inductive logics and semantical systems, first extensional and later modal systems. In this "semantic" period Carnap no longer defined analyticity in (supposedly) syntactic formal terms (as he had done in 1934) but recognised it as a semantic notion. (For observations on the beginning of this period see chapter 7.)
The title of Carnap's *Der logische Aufbau der Welt*, literally "The Logical Construction of the World", suggested to him by Schlick, is thus somewhat misleading. It would have been more correct to announce its aim as Carnap did in the text, namely as *the logical reconstruction of knowledge claims about the world*. Call it then "Der logische Wiederaufbau der Welterkenntnis", "The Logical Reconstruction of Theories of the World". First, the aim of Carnap's reconstruction was the potential justification of knowledge claims, it was not to give an accurate picture of the actual cognitive processes of knowledge acquisition, nor to provide tools for the practice of science. Carnap expressly stressed that his project aimed merely to provide a *rational reconstruction* of the processes of cognition: to lay a "rational foundation" for the exercise of scientific concepts was to be distinguished from investigations of how these concepts and the scientific theses wherein they figure have actually been arrived at.

Alongside this claim to methodological modesty, Catnap made a substantial material claim: the scientific picture of the world could be generated by purely logical constructions out of the elements of immediate experience. What Carnap thus reconstructed in what he called his "constitution system" ("Konstitutionssystem") was not so much the world as independent objects of experience but rather the *concepts with which experience is comprehended*. Carnap's constitution system took the form of a special "constructional language" in terms of which empirical concepts were reconstructed.

For Carnap, showing that a proposition was verified or verifiable, i.e. satisfies "verification conditions", consisted in showing that it stood in the proper logical relation to propositions expressing first person knowledge. In short, Carnap sought to erect a logical,

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5On Carnap's own doubts about this title see Coffa 1985:147n.
6Carnap 1928a:89
7Ibid.:xvii.
8In the text itself this remained somewhat unclear, partly because Carnap's own decision to use the terms "concept" and "object" interchangeably (ibid.:10).
set-theoretic, structural system of concepts which was, by definition, rooted in experience, and which validated scientific knowledge claims, if they could be translated into it. The plot of Carnap's rational reconstruction was as follows. Grant an incorrigible first-person knowledge base and on this base construct the verification conditions for all empirical claims. In this construction, however, proceed entirely structurally, that is, the description of these verification conditions must abstract from their experiential quality, and use descriptions only of the general kind used in science, namely, relational, not intrinsic descriptions. (More on these below.)

The "philosophical" ground plan of the Aufbau then must be distinguished from the "scientific" strategy by which the reconstruction of empirical knowledge was to be effected. Carnap's ground plan consisted in providing a "genealogy of concepts" which related them to a base in the phenomenal "given": all empirical concepts were to be shown to be reconstructible on the basis of an individual's stream of experience. As soon as this phenomenalist setting of the epistemological problem was granted, however, knowledge became itself the object of scientific analysis: the given itself was to receive scientific descriptions of its relational structure, not its intrinsic experiential quality. Carnap's strategy for building this "genealogy of concepts" was to assign each concept a definite place in the hierarchy of the system wherein they were constituted. Its genealogy was to make for the identity of the concept in question, concepts were to be extensionally defined. It is a not always properly understood measure of the complexity of Carnap's project that he sought to do with only one primitive predicate and that he considered even sense-data as objects to be constructed from unanalysable whole experiences with the help of logic and the one basic predicate. (More on the method and reason for this below.)
3.2 Carnap's (Re)Constructional Choices: The Mechanics of the Aufbau

With this overview of the Aufbau in hand, I now review four important choices highlighted above, which Carnap made in deciding on the form of his constitution system, that is, his constructional or artificial language. First, there is the choice of the object domain of his theory, the physical or the psychological; second, there is the choice of the category of predicates to be used, whether these should be relational structural predicates or predicates expressing intrinsic properties; third, there is the choice of basic predicates within the chosen category; fourth, there is the choice of the criterion of adequacy that the reconstruction of empirical knowledge must meet. In what follows, I will discuss the considerations that guided Carnap’s decisions on these choices. In line with the distinction just drawn between Carnap’s phenomenalist ground plan of reconstruction and his scientific method of pursuit I discuss choice one under the heading of "phenomenalist choices" and choices two through four under the heading of "structuralist choices".

3.2.1 The Phenomenalist Choice of the Object Domain

I already mentioned the basic object domain of Carnap’s reconstruction: the basic relation holds between experiences of an individual subject. This means that the ultimate basis of scientific theories consists of the experiences of individual investigators. Carnap opted for an "auto-psychological", i.e., phenomenalist basis.9

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9Ibid.:101. The basis was further delimited within the phenomenal domain. In general, Carnap counted into this realm both "acts of consciousness: perceptions, representations, feelings, thoughts, acts of will, and so on [...] also unconscious processes to the extent to which they can be considered analogous to acts of consciousness, for example, unconscious representations." (Ibid.:32/3.) As a basis for the constructional system, however, no unconscious occurrences but only "conscious appearances (in the widest sense)" (Ibid.:102) were permitted to serve; these Carnap called "the given". A potential difficulty arises here. Since "all experiences belong to [the basis of the constitution system] no matter whether or not we presently or afterward reflect upon them" (Ibid.) it is not entirely clear whether Carnap's distinction between conscious and unconscious psychic occurrences can be preserved if these terms are meant purely descriptively: the pre-conscious is, after all, also unconscious in the descriptive sense of the word, but it, unlike the unconscious proper, can relatively easily be made available for reflection after the fact. It seems then that Carnap meant to exclude from "the given" only the unconscious in Freud's non-descriptive topographical sense of the term.
What was the precise status of this option? Most important to Carnap's decision was the desire not only to reflect "the logico-constructual order of the objects [of science] but also their epistemic order."\textsuperscript{10} Carnap spoke explicitly of a choice: nothing in the "logico-constructual order of objects" forced him to adopt the phenomenalist basis. (Thus he explicitly stated that a constitution system with a physicalist basis could be devised.) Carnap called the phenomenalist base the "natural starting point in the epistemic order of objects."\textsuperscript{11} What suggested the phenomenalist basis as preferable to Carnap was the belief that our empirical knowledge of the physical world could only be justified by reduction to the given. His phenomenalism was epistemologically motivated. This then was the position of "methodological solipsism"\textsuperscript{12}: the immediate phenomenal given was basic to cognition of the world in an evidential sense. The position of methodological solipsism did not, in Carnap's view, carry any metaphysical import. Carnap held that the domains of psychological and of physical objects were mutually logically reducible, and that either could serve as the basis of the constitution system.\textsuperscript{13} That is why his solipsism was merely "methodological" in his own estimation.

I shall call Carnap's reason the "intertranslatability thesis". It consisted in this. The reduction of the physical to the psychological followed directly from the adoption of the verification theory of meaning and methodological solipsism. On pain of the meaninglessness of all physical object discourse, it followed that all physical objects had to be reducible to phenomenal ones, for only if statements about physical objects can be reduced to statements about phenomenal objects can they be verified.\textsuperscript{14} Reduction in the opposite direction, of the psychological to the physical domain, could be in principle

\textsuperscript{10}Ibid.:101. The fact that this choice was also more economical in terms of the number of basic concepts needed was said to be only a secondary reason at ibid.:88.

\textsuperscript{11}Ibid.:107.

\textsuperscript{12}The phrase was adopted from Driesch; see ibid.:102.

\textsuperscript{13}Ibid.:100/1.

\textsuperscript{14}Ibid.:92.
achieved along two routes: once by considering the phenomenon of psycho-physical correlation, and once by considering the "expression relation" between bodily motions and psychological processes. The first of these routes went as follows. Accepting psycho-physical parallelism as the working hypothesis of the empirical science of psychology\textsuperscript{15}, every statement about a psychological object becomes translatable into statements about physical objects—if only in principle because at present it cannot be stated to just which physical objects psychological objects are to be reduced.\textsuperscript{16} But Carnap clearly did not dare to rest his case with this presumption, since his verificationism at this stage required more than reducibility in principle. (The \textit{Aufbau} required complete verification.) Accordingly, his intertranslatability thesis relied crucially on this second way of reducing the psychological to the physical. Every psychological state of another was held to be recognisable only on the basis of his expressive motions or reports which served as "indicators" of that psychological state. Every statement about other minds could thus be translated into statements about their indicators, that is, behaviour, and thus all hetero-psychological objects became reducible to to physical ones.\textsuperscript{17} (Note for future reference that Carnap did not provide for the reduction of the \textit{auto}-psychological to the physical in another way than by the as yet unrealisable working hypothesis of psycho-physical parallelism.)

Given this account of the mutual translatability of the psychological and physical languages, Carnap thought it possible to construct a constitution system of concepts reflecting his conception of epistemic priority (that is, by choosing the \textit{auto}-psychological domain to serve as its basis) without prejudging the ontological issue of phenomenalism versus realism or materialism. What this meant was that the ontological exclusivity claim

\textsuperscript{15}Ibid.:38.
\textsuperscript{16}Ibid.:92.
\textsuperscript{17}Ibid.:93.
of phenomenalism over realism/materialism was held at bay. It did not mean that Carnap’s methodological solipsism was immediately innocent of all ontological claims, for, as I noted, Carnap accepted psycho-physical parallelism. (Carnap required a separate argument to disarm all ontological talk as meaningless in order to avoid dualism.\textsuperscript{18}) Carnap’s supposedly non-metaphysical position of methodological solipsism was accordingly the following.

The differentiation between real and non-real objects does not stand at the beginning of the constructional system [...] We shall not claim reality or non-reality in connection with these experiences; rather, these claims will be "bracketed"\textsuperscript{19}

Carnap avoided the metaphysical thesis of solipsism because he thought of the constitution system as so designed that the scientific assertions concerning the reality or non-reality of objects could themselves be reconstructed within it: the objective world of science was thought recoverable for each epistemic subject from his resources alone.

With the acceptance of methodological solipsism, Carnap adopted the following ordering of reductions of object domains: the physical objects were constructed from phenomenal ones, other minds from physical objects, and, finally, cultural objects from other minds. Let’s ask: why not use the physical domain instead of the phenomenal one with which it is intertranslatable? It would speak for this suggestion that Carnap accepted the behaviourist claim that all talk of psychological phenomena can be reduced to talk about bodily behaviour; since the psychological is so reducible nothing would seem to be lost. What spoke against this suggestion was Carnap’s reasoning that "the further claim of behaviourism, namely that this ordering of objects is also a correct reflection of the epistemic relations would still be problematic."\textsuperscript{20} This suggests that Carnap’s decision to

\textsuperscript{18}To provide such an argument was the point of his slightly later 1928b.
\textsuperscript{19}Carnap 1928a:101; there followed an explicit reference to Husserl’s Ideas.
\textsuperscript{20}Ibid.:96.
adopt the auto-psychological as the basic object domain of his constitution system followed neither from formal considerations of constructional convenience nor from factual, strictly scientific ones; rather, it followed from a priori considerations about what an epistemic order would look like, and what would count as an epistemological justification. (I will return to this point below.)

3.2.2 The Structuralist Choices of Language Form

I begin the review of Carnap's structuralist choices with his choice of predicate kind. I then turn to his choice of the basic predicate and then to his choice of the adequacy condition of his reconstruction.

According to Carnap, science uses only relational predicates.\textsuperscript{21} As he explained, relational descriptions, unlike intrinsic property descriptions, do not make assertions about its objects as absolute individuals, but instead make assertions about them as related to each other. Carnap accepted this preference for relational predicates as a fact; his reconstruction of empirical knowledge built on it. But was all reliance on intuition banished with this step of only using relations alone?

Carnap narrowed the mere preference for relational predicates further to what he called "structure descriptions".

Unlike relation descriptions, these not only leave the properties of the individual elements of the range unmentioned, they do not even specify the relations themselves which hold between these elements [...] only the structure of the relation is indicated [...] by formal properties [...] without reference to the meaning of the relation.\textsuperscript{22}

Structure descriptions are reconstructions of descriptions which employ only relational predicates. The important feature of structure descriptions is that in them only logical terms

\textsuperscript{21}Ibid.:20.
\textsuperscript{22}Ibid.:21.
occur essentially. (The substitution of its non-logical terms leaves the truth value of a statement unchanged.) A simple example: in a universe only containing just one family, consisting of the parents Adam and Eve and the children Cain and Abel, the relation "father of" can be given as the structure description "(Adam, Cain) & (Adam, Abel)".

Carnap's structure descriptions were derived from Hilbert's and Gergonne's implicit definitions. Following Hilbert's axiomatisation of geometry, Schlick had championed these implicit definitions, which laid down "the mutual relations of the primitive concepts [of a theory] as expressed in the axioms", as fixing the meaning of concepts without reliance on intuition.23 Here the meaning of concepts was determined in terms of their formal properties alone, i.e., in terms of their implicational relations to other concepts: the idea expressed by a term was exactly that of fitting the conditions laid down for its use. The novelty of Carnap's use of Hilbert's implicit definitions consisted in his development of structural definite descriptions. They worked in the same fashion as structure descriptions but designated one object uniquely. Importantly, they were intended to do so in Carnap's constitution system without any need to specify the domain they were meant to apply.

As envisaged by Carnap, his constructional system contained only one basic descriptive relation. Even that was thought to be treatable like an uninterpreted predicate so that it could appear in structure descriptions where, as I noted, only logical concepts figured essentially. All descriptive predicates of empirical science then were held to be reconstructible by logical operations on this one basic uninterpreted descriptive relation. It was the project of achieving a purely formal determination of concepts which required

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23 Schlick 1918/25:36 (more on Schlick's and Carnap's agreement in section 4.1 below). In Schlick's Erkenntnislehre this idea was coupled with a thoroughly realist appraisal of the scientific enterprise; what Carnap did in the Aufbau was not only to adapt this conception of scientific knowledge to a metaphysically abstinent position, but also to build such a position on it as its foundation.
Carnap to renounce all reference to the meaning of descriptive terms: "each scientific statement can in principle be so transformed that it is nothing but a structure statement."\(^{24}\)

For this to be possible, the basic elements of the constitution system had to be carefully chosen. Carnap took as his "undefined basic objects" not objects, but "relation extensions", ordered pairs. As he explained,

basic relations take precedence over the basic elements which are their members; generally speaking, construction theory considers individual objects as secondary, relative to the network in which they stand.\(^{25}\)

Carnap made this choice because the basic objects of the system were "elementary experiences" and he deemed such experiences unanalysable, holistic Gestalten. Since it was, however, the very contents of these elementary experiences from which scientific concepts were to be built up in his system, Carnap devised the ingenious method of "quasi-analysis" of elementary experiences. Quasi-analysis built up sense-data as similarity classes between elementary experiences which were ordered by the basic predicate, the relation of remembered similarity. Fortunately, the details of quasi-analysis need not concern us here.\(^{26}\) It suffices to note that quasi-analysis worked with the following theoretical idealisation. The idealisation was that a pair list of all the elementary experiences of an individual as ordered by the basic relation was assumed to be given to the theorist and that, accordingly, the epistemic subject possessed an infinite memory.\(^{27}\) Given this idealisation, objects of ascending orders of complexity were to be constructed in steps from similarity classes of unanalysable experiences as determined in this basic extension list.

\(^{24}\)Ibid.:29, my italics.  
\(^{25}\)Ibid.:98/9.  
\(^{26}\)See Goodman 1951 and Proust 1984.  
\(^{27}\)Carnap 1928a:159/60.
In its finished state then Carnap's constitution system would allow the transformation of every empirical statement such that for each of its non-primitive non-logical concepts its constitutional definiens is substituted.

"Eventually the sentence will have a form in which (outside of logical symbols) it contains only signs for basic relations [...] the sentence [...] has now been so transformed that it expresses a definite (formal and extensional) state of affairs relative to the basic relation."

Every statement so transformed became in principle decidable. (Due to the Aufbau's solipsistically reduced universe and the assumption of infinite memory, even universal statements now were decidable.) The method of structure descriptions thus allowed the verifiable meaning of statements to be exhibited by what could be called their "logical form relative to the given."

The method of definite structure descriptions prescribed Carnap's choice of the criterion of the adequacy of the constitution system. Structures are individuated extensionally, that is, in terms of the network of the nodes that comprise them: two isomorphic structures are type-identical. Accordingly, Carnap's definitions of each concept in the hierarchy of the system were extensional definitions: if two apparently different concepts were assigned the same place, that is, if they received the same reductive definition, then they were considered type-identical, no matter what different meanings or connotations they ordinarily carried. That Carnap's constructional language only had to be extensionally adequate to the actually used scientific language which it reconstructed, meant that no attempt was made to explicate the ordinary sense of the reconstructed concepts. As I noted, no attempt was made to capture the actual process of cognition. Carnap's

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28 Throughout the Aufbau Carnap disclaimed any finality for the sketch he provided and once explicitly declared his aim to be only the "logical investigation of the method which would lead to such a system" (Ibid.:176).
29 Ibid.:291/2.
30 Ibid.:85.
reductions thus allowed only for translations salva veritate, "logical translations", of empirical statements into statements about the immediately given. The constructional system was "concerned exclusively with logical, not epistemic, value; it is purely logical, not psychological." Carnap was emboldened in this decision by his belief, here stated as a fact, that all purportedly intensional statements could be translated into extensional ones. Accordingly, he contended that no logically relevant meaning of an assertion could be lost by the extensional reduction of empirical statements to the given.

So much for the review of Carnap's structuralist choices and his reasons. Two worries spring to mind. One might wonder, first, whether the complete structuralisation of empirical knowledge envisaged by Carnap did not turn empirical knowledge into logico-mathematical and thus non-empirical knowledge. Here it must be noted that Carnap declared the theorems of the constructional system to divide into two kinds, analytic and synthetic. The analytic theorems (apart from those of logic and arithmetic which simply were presupposed in the constitution system) were provided by the reductive definitions which his constructional language provided for the empirical concepts of science. The synthetic theorems concerned relations between constructed objects which were not definitionally fixed, but which could be ascertained through experience alone. The synthetic theorems mirrored what a person actually knew, given the "givens" he experienced. Only the reconstructive reductionist framework of scientific concepts was thus analytic; any factual, non-definitionial claims depended on what was, in fact, "given". Thus Carnap thought the empirical nature of the knowledge reconstructed safeguarded by the synthetic theorems as which the constructional language translated empirical knowledge.

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31 Ibid.: 84. Carnap's use of "epistemic" as a predicate descriptive of that "value" of terms which his reconstruction discarded is somewhat odd, given his concern with the "epistemic priorities" in cognition (see below) but I shall not worry about it here.

32 Ibid.: 77; more on this below sections 3.4.3 and 6.3.1. In later works Carnap spoke of this as a hypothesis.

33 Ibid.: 176.
claims. Note again, that the solipsistically reduced universe allowed not only existential, but also universal propositions to be so verifiably translated. Universal laws did not have to be reconstructed as definitions. Clearly, however, they presupposed that the definitions were phrased so as to allow for given universal laws to translated into synthetic statements of the consructional language. Carnap's safeguarding of the synthetic nature of empirical knowledge thus depended on having the correct dictionary, on having the right analytical framework.

One may wonder, then, on what basis such a distinction between analytic and synthetic theorems could be drawn. Here it is important to note that Carnap admitted that the reconstructive theorist (but not the individual whose knowledge was to be reconstructed) had to be familiar with the sense of the basic relation and with the way the world was: only against this prior knowledge was it possible to determine "which constructional steps are appropriate for each level and to which entity each of them leads, even though [the theorist does] not know of what nature A's experiences are". Two distinct steps then were involved in the reconstruction of empirical knowledge. The first step was preliminary; it provided a reductive dictionary. For the purposes of this step the theorist needed no inventory list of the extension of the basic relation extension of remembered similarity. The second step provided justification: the translation or expansion of the knowledge claims enabled the theorist to compare them with the remembered similarities stated in this inventory list. The analytical theorems of the constitution system then are those constructions formulated in the first step, "independently from the individual subject". (This meant that Carnap's reconstruction failed as a fully extensional one: the theorist providing the reconstruction had to know the meaning of the

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34 This speaks against Friedman 1987 and Popper 1963 who allege that Carnap's reconstruction of empirical knowledge collapses into analytical knowledge, knowledge of tautologies.
35 ibid.:161.
36 ibid..
basic relation, the extension list of that relation did not suffice for the first step: Carnap had
to presuppose a grasp of the analytic-synthetic distinction, as I shall show further below.)

3.3 The Point of the Aufbau

What was the point of Carnap's reconstruction of empirical knowledge? We can
distinguish two answers here, corresponding to the two perspectives on and of scientific
philosophy characteristic of the traditional and the new canonical view of Vienna Circle
philosophy.

3.3.1 The Traditional Reading: Phenomenalist Foundationalism

The traditional reading of the Aufbau takes its cue from Russell's "Wherever
possible, logical constructions are to be substituted for inferred entities", adopted by
Carnap as his guiding maxim.37 (Russell, in fact, credited Whitehead with the idea of--as
Carnap put it--"apply[ing] the theory of relations to the task of analysing reality".38) We
must ask, what was justified by Carnap's substitutional reconstruction of empirical
concepts? Was it the beliefs of old about the hypothetical entities that were justified, now
that we knew how to substitute the new logical constructions for the old terms, or were the
old beliefs to be discarded? For Russell, the point of constructing "some logical function
of less hypothetical entities which has the requisite properties" of the inferred entities was
to "swe[ep] away [...] the useless menagerie of metaphysical monsters".39 Carnap
followed Russell in this anti-metaphysical vein. According to the traditional reading,
Carnap's reconstruction was meant to provide a new meaning to the old forms of
expression, and in doing so, to show the knowledge claim of science to be legitimated.

37Russell 1914:115, quoted at Carnap 1928a:5.
38Russell 1914, Carnap 1928a:7.
Appearances to the contrary notwithstanding, science was only committed to claims that could be justified by reference to sense experience.

This picture of Carnap as the fullfiller of the Russelian project fits nicely with the traditional picture of Vienna Circle philosophy and is widely endorsed. Accordingly, the Aufbau presents a recapturing of the old empiricist dream to establish conclusively that knowledge of nature is arrived at on the basis of the data of experience alone. Its importance consisted in that it constituted a "redo" with the then new tools of mathematical logic (and so went beyond Hume's associationism and Mach's theory of neutral elements) and with some of the then new insights of Gestalt psychology (and so went even beyond Russell's psychological atomism). Apart from its greater thoroughness than earlier attempts, the real novelty of Carnap's Aufbau accordingly consisted mainly in the fact that, unlike Russell, Carnap did not start his construction from the assumption of the existence of immediately given isolatable sense data.

Why would Carnap have wanted to undertake a project like the Aufbau? One of Carnap's declared tasks was to demonstrate the possibility of constructing a unified system of all scientific concepts: therewith the thesis of the unity of science would be proven and the purported inevitability of the separation into unrelated special sciences or even science types would be refuted. Accordingly, it was Carnap's project to elevate the Vienna Circle's thesis of the unity of science from the level of meta-scientific hypothesis to that of proven meta-scientific fact: no longer was it to be conceived as a regulative ideal only. Following the linguistic turn, Carnap read the unity of science thesis as a thesis about the language of science and in this form returned to phenomenalist reductionism. All the


\[\text{\footnote{Carnap 1928a:7, 9. Carnap had not always been so confident of the unity of science thesis; see his 1926:2.}}\]
concepts of empirical science were to be shown meaningful due to their relation to the "given", and all scientific statements were thus to be shown justified. Scientific knowledge was shown to rest entirely on experiential foundations. Note that this answer does not yet address the question why Carnap went beyond Russell in wanting to constitute even sense-data. Here the desire to take account of the findings of Gestalt psychology could be invoked. As for why even the one basic relation had to be treated as uninterpreted, however, no reason would be forthcoming (short of attributing to Carnap unbridled teutonic fervour).

Given this reading, what posterity has come to learn of as the failure of Carnap's phenomenalist reduction is easily seen as disposing of what is of interest in his project. For example, Quine pointed out that Carnap's reconstruction failed at precisely the point where the step from reconstructed classes of sense qualities to spatio-temporal objects was to be taken: no complete definitions of the latter were provided. With the phenomenalistic reduction failed, the foundationalist project failed as well. But was this really Carnap's main concern? There is already this prima facie reason to reject the reading of Carnap as a merely logically reconstructed traditional foundationalist. Carnap aimed to reconstruct empirical terms in accordance with their use in the empirical sciences. As a result, Carnap's reductive definitions were themselves relative to a given state of scientific knowledge. That Carnap thus came to preclude the fulfillment of the foundationalist ideal of certainty provides a first reason for doubting that phenomenalist reductionism was the main concern of the Aufbau.

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42 The details of Quine's (and Goodman's yet different) criticism need not interest us here since coming long after the protocol sentence debate they played no role in it. See Quine 1951, 1969a, Goodman 1951.

43 I discuss this matter in the course of reviewing Schlick's early criticisms of the Aufbau in sect. 4.4.
3.3.2 The New Reading: Structuralist Neo-Kantianism

The new canon of Vienna Circle scholarship disputes the traditional reading of the Aufbau's significance (though not, of course, the diagnosis of the reductionist failure). The new canon holds that Carnap was concerned not merely with the facts and limits of knowledge, but, first of all, with its possibility.\textsuperscript{44} What is of major interest in the Aufbau for this reading are Carnap's fledgling steps to effect the linguistic turn within a broadly Kantian perspective on the problem of knowledge: how is objectivity possible? Accordingly, the marriage of phenomenalism and logicism was meant to improve Kant's flawed answer to the problem he had correctly identified: it was not meant to discard the problem he had unearthed, as the traditional view implies.

The picture of Carnap as an alternative neo-Kantian is best summarised in the words of Alberto Coffa.

Hosts of philosophers had tried to develop the Kantian idea that experience and its objects are constituted through our categories. [...] In conformity with the ontological bent of traditional idealism, they agreed to compare constitution to construction, thus suggesting that what our mind does with the objects of experience is an activity comparable to what the engineer does with his bridges. [...] Carnap may have been the first among admirers of the idea of constitution to come up with a reasonable theory about the nature of that activity which grasps both the kernel of truth in the Kantian doctrine and excludes the ontological-idealistic implication.\textsuperscript{45}

Carnap was ultimately concerned not with the constitution of the objects of experience but with "the constitution of meaning".\textsuperscript{46} Carnap investigated this constitution of meaning on the part of cognitive subjects in order to reach "the articulation and defense of a radically new conception of objectivity".\textsuperscript{47}

\textsuperscript{44}See above sect. 2.3.3.
\textsuperscript{45}Coffa 1985:147/8.
\textsuperscript{46}Coffa 1986:59.
\textsuperscript{47}Friedman 1987:526.
The essential idea in this new reading is that we must distinguish between the form and content of experience. Kant's answer to the question of the possibility of knowledge was that the content of experience was given through the senses, but that it was presented to us in particular forms which were imposed upon it by both the faculty of intuition and that of understanding, and that it was the particular synthetic a priori forms of intuition and the understanding, which made for the objectivity of knowledge. To reconstruct the a priori conditions of knowledge as purely formal conditions was Carnap's project of improving on Kant. Carnap shared with Kant the conviction that the possibility of objective knowledge could not reside in the "content" of experience.

To understand the new reading of Carnap, it is important to note what "content" means here. First of all, it means the "stuff" of experience, its "qualitative material". For Carnap, what militated against basing the objectivity of knowledge in the content of experience so understood was the fact that the "stuff" of experience is by definition private: we cannot intersubjectively compare the qualitative character of our sense experiences.48 Secondly, Carnap thought not only the "stuff" of sensory intuition to be so private (to the detriment of its objectivity), but the "intuitive meaning" of language as well. This intuitive meaning concerned the "epistemic value" of expressions.49 Carnap considered this "value" relevant only to investigations of the "mental representations which accompany scientific statements", that is, to psychological, but not logical investigations.50 Carnap's rational reconstruction meant to do without either of these "intuitive contents", experiential qualities or "understood" meanings.

According to the new canon, Carnap declared the primary aim in the Aufbau in the following passage.

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49 See fn. 19 above.
50 Ibid.:83-7.
Science wants to speak about what's objective and whatever does not belong to the structure but to the material (i.e. anything that can be pointed out in a concrete ostensive definition) is, in the final analysis subjective. [...] The series of experiences is different for each subject. If we want to achieve, in spite of this, agreement in the names for the entities which are constructed on the basis of these experiences then this cannot be done by reference to the completely divergent content, but only through the formal description of the structure of these entities.\(^{51}\)

Following the new reading of Carnap, his method of reconstructing empirical knowledge by means of what he called "definite structural descriptions" was not merely a convenient ploy to make phenomenalism work, but constituted the very heart of his enterprise.

Carnap answered the question of objectivity with reference to his choice of the predicate form of the constitution system. This choice was prominently marked as follows. "[W]e shall maintain and seek to establish the thesis that science deals only with the description of structural properties of objects."\(^{52}\) It was the requirement of "the achievement of objectivity" which pressed Carnap to assert that "for science it is possible and at the same time necessary to restrict itself to structure statements."\(^{53}\) Carnap's choice of structure descriptions as the basic predicate form constituted his answer to the question whether, and just how, the objectivity of empirical science can be accounted for, if the meaning of empirical statements rests in subjective givens. Carnap's answer was that, first of all, all that was required were "intersubjectively valid assertions", that is, assertions which possess a "validity which also holds for other subjects"\(^{54}\); secondly, that such intersubjective validity was guaranteed by the kind of objectivity bestowed by structural descriptions. Carnap thus rested the objectivity of knowledge in the structures which his

\(^{51}\)Ibid.:29.
\(^{52}\)Ibid.:19; my italics.
\(^{53}\)Ibid.:30; my italics.
\(^{54}\)Ibid.:107.
constitution analysis ascribed to experiences: "all streams of experience agree in respect of certain structural properties."\(^55\)

This way of putting things suggests that Carnap preserved the intersubjective validity of knowledge because, unlike the qualities of different subjects' experiences, the formal structures of these experiences were in principle comparable. The structural properties of experiences were rendered intelligible and comparable by the reconstructive explication of their meaning in Carnap's constructional language, the reduction to the given. These formal structural properties were thought to mirror those of the experiences which provide the evidential basis of empirical claims. Carnap conceived of intersubjectivity as consisting in the relation of isomorphism between the logical structures of the linguistically explicated givens of different subjects.

I believe that this idea of intersubjectivity as the structural isomorphism of the givens of different subjects was indeed Carnap's leading idea: all givens "constitute the world" according to the same analytical theorems of the constitution system. If Carnap's methodological solipsism is not to become a real solipsism, there must be more existent givens than just one, and if the objectivity of cognition rests in the structure of the given, then this structure must be shared between those different givens. So far, so good. The trouble is, however, that this kind of objectivity was not reconstructed in the \textit{Aufbau}. (As we shall see when considering Neurath's criticism of the \textit{Aufbau}, Carnap's actual reconstruction of intersubjectivity did not redeem his promise.\(^56\)) Nevertheless, Carnap's

\(^{55}\)Ibid.; translation change. J. George's translation ("certain structural properties are analogous for all streams of experience") introduces the notion .. analogy not yet used in the present section but only in a later one dealing with the question of intersubjectivity. Since Carnap seems in fact to be suggesting two distinct conceptions of intersubjectivity in these two sections, George's translation is misleading. (More on this below, section 4.5.3).

\(^{56}\)As reconstructed by Carnap, the "intersubjective correspondence" of "the world of the other" and "my world" was not one of isomorphism between the givens of different subjects, rather, starting from a phenomenalist standpoint "the world of [the other] is constructed within my world." (Ibid.:224, my italics.) There is of course a world of difference between intersubjective validity reconstructed as the structural isomorphism between different givens and its reconstruction as structural analogies between
alternative neo-Kantianism asserted that the objectivity of knowledge rested in the form, not the content (qua "stuff") of experience. This form was not, of course, a synthetic one, as it was for Kant. It did not impute substantial theses about the subject matter of experience. For Carnap, this form, which made possible the objectivity of knowledge, was an analytic one which set out the framework of representations used in terms of their mutual implication relations. Different subjects cannot share the content "stuff" of their experiences, which is private, but they can share the form or logical structure of their experiences.

The new canonical view of the Aufbau has the virtue of explaining what the traditional view cannot explain: Carnap's insistence on the thoroughgoing structuralisation of empirical knowledge. Why did Carnap insist, first, on reconstructing the concepts of empirical science of just one predicate, and why did he even insist, second, that the "intuitive" meaning of the basic, last empirical predicate not be relied on, that it--"remembered similarity"--itself be treated as uninterpreted? The answer is that Carnap's insistence on the complete structuralisation of empirical knowledge required the prior reduction of all descriptive predicates to just one. How could, say, two basic predicates of the same language be differentiated and yet be treated as uninterpreted? The de-interpretation of the one basic predicate, in turn, followed from Carnap's alternative neo-Kantian project: if only form makes for objectivity, then even this last basic descriptive predicate must be individuated in terms of its form alone; we cannot rely on its intuitive content.

I must note that this structuralist impetus in its general form was not limited to Carnap. Carnap endorsed Poincare's view that not sensations themselves, but only

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one subject's given and that of others as reconstructed from that subject's phenomenalist vantage point. (More on this below, section 4.5.3.)
relations between them have "objective value". But was all reliance on intuition banished with this step of only using relations alone? Against Poincare, Carnap stressed the need to "go on to the structure of relations" if we want to reach totally formalized entities. Relations themselves, in their qualitative peculiarity are not intersubjectively communicable. Carnap indicated that he was indebted to Russell for recognition of "the importance of structure for the achievement of objectivity". Carnap noted that he received an important impetus from Russell who wrote that "[f]or mathematical purposes (though not for those of pure philosophy) the only thing of importance about a relation is the cases in which it holds, not its intrinsic nature". When the new canonical view reads Carnap's pursuit of the radical structuralisation of empirical knowledge as meant to further his alternative neo-Kantian project, it thus also recognises broadly Kantian tendencies also in some of Carnap's precursors. Note, however, that Russell would seem to have left it an open question what "pure philosophy" might make of the "intrinsic" natures of relations. Carnap, however disavowed any concern with intrinsicality, indeed rejected it as still stained by reliance on intuition. Thus acquaintance with Russellian sense data--something Carnap may well have taken to trade on the intrinsic nature our relations to them--was ill fit to serve as the foundation of objectivity or even its conduit, unless of course such acquaintance could itself be constituted structurally. And that's of course just what his quasi-analysis did, according to Carnap. Note then that Carnap's radical structuralism was intended to surmount sceptical objections stronger still than those of Russell's at the time, a form of scepticism perhaps first responded to by Schlick in his Erkenntnislehre.

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58 Ibid., my italics.
59 Russell 1919:60.
60 Ibid. Like the previous, this passage is taken from the relevant pages cited at Carnap 1928a:30.
61 See Friedman 1983 for relevant observations on Schlick and Friedman & Demopoulos 1985 on Russell.
62 Schlick 1918/25. See below sect. 4.1.
In this connection, consider also yet another point in favour of the new canonical reading of the *Aufbau*. Carnap's structuralism effected yet more than the explanation of objectivity by the intersubjective communion of the structural framework of the private givens of different individuals. Carnap's answer to the question of the possibility of objectivity of knowledge did not only bring the broadly Kantian response in line with the thesis of logicism and extended the rule of analytic forms to empirical science; it also provided an alternative to Kant and the neo-Kantians in yet a further respect. It also enforced the Vienna Circle's positivism. This becomes particularly clear we we turn to Russell's remarks which were so influential on Carnap.

There has been a great deal of speculation in traditional philosophy which might have been avoided if the importance of structure, and the impossibility of getting behind it, had been realised. For example, it is often said that space and time are subjective, but that they have objective counterparts; or that phenomena are subjective, but are caused by things in themselves [...] Where such hypotheses are made, it is generally supposed that we can know very little about the objective counterparts. In actual fact however, if the hypotheses as stated were correct, the objective counterparts would form a world having the same structure as the phenomenal world, and allowing us to infer from phenomena the truth of all propositions that can be stated in abstract terms and are known to be true of phenomena. If the phenomenal world has three dimensions, so must the world behind the phenomena; if the phenomenal world is Euclidean, so must the other be; and so on. In short, every proposition having a communicable significance must be true of both worlds or of neither: the only difference must lie in just that essence of individuality which always eludes words and baffles description, but which, for that very reason, is irrelevant to science.63

Reading this passage through Carnap's eyes, we find the following meaning. If it is the form of the given, not its intuitive content, which makes for the objectivity of cognition, then only these formal properties pertain to reality (in so far as reality is scientifically comprehensible). That is, by resting objectivity in the form of the logical form of cognition alone, all duplications of intersubjective reality by things in themselves become entirely unnecessary: Objective reality alone is what is structurally comprehensible. Following

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63 Russell 1919:61
through the implications of Carnap's structuralism, one can thus see that his a-
metaphysical conclusions, i.e. that science is neutral vis-a-vis the question of realism or
idealism, was rooted not so much in his phenomenalist verificationism (which would, after
all, tip the scales towards idealism), but in his structuralism.

3.4 The Tension in Carnap's Aufbau

Let me now ask, what is the relation between the phenomenalist conception of
knowledge and the program of structuralising knowledge so as to guarantee objectivity? In
terms of the two views of Vienna Circle philosophy outlined earlier: what is the relation
between the pursuit of the Russell-Whitehead program and the pursuit of the alternative
neo-Kantianism? What is the relation between the phenomenalist reductionism and the
structuralist methodology?

First, it must be noted that it is clearly not only possible but also advisable to
endorse aspects of the traditional reading in addition to the new reading of the Aufbau.
Carnap aimed both to prove the unity of science thesis by the thoroughgoing reduction of
all scientific concepts to the given, and to establish the nature of the objectivity of science.
Carnap avowed both aims. Saying this does not, however, answer all the questions about
the relation between Carnap's reductionism and his structuralism. Thus one may ask,
could either aim be pursued on its own? In order to demonstrate how these two strands of
Carnap's thought relate to one another, I now show that, according to some considerations,
Carnap's phenomenalist assumptions and his structuralist methodology are not only
compatible but that there exists a certain dependency between them. Then I point out that
the matter cannot rest there.
3.4.1 The Compatibility of Phenomenalism and Structuralism

As I explained earlier, behind the Aufbau stands a clearly mentalist conception of knowledge. True to the strategy of the linguistic turn, thoughts were considered linguistically constituted; their meaning was reduced to what was immediately given to the subject's consciousness, if the thought were true. At the same time, however, the treatment which this mentalistically conceived of knowledge received at Carnap's hands was decidedly unmentalistic: it was the formal structure that mattered, not experiential quality. Experience was treated by Carnap in the same way as all other objects of science, structurally, that is, relationally. There is clearly some at least superficial contrast between his phenomenalistic starting point and his strictly scientific way of dealing with his subject matter.

The first objection exploiting this tension would be, that we do not, as a rule, experience the structure of our mental states as such; we experience their content. Here Carnap would answer that he was only providing a rational reconstruction, not drawing a psychologically realistic picture. Carnap took an external perspective on the phenomenal given. In place of acquaintance with content he thus put structural determinations. Now, in order to do this he needed to make some strong idealising assumptions (which in turn were to be defended by reference to the project of reconstruction). It could be objected that these idealising assumptions are too strong.

What were these idealising assumptions? Carnap noted that, for the reconstructive justification of knowledge claims to be brought off, he had to proceed on the basis of the "fiction" of having a complete list of the extension of the basic predicate of remembered similarity as applicable to a given individual. He further noted that this "fiction" required
the idealising assumption of a subject's infinite memory. As noted above, this justificatory step of the reconstruction proceeded only against the prior one of fixing the reductive definitions of all empirical concepts. Fixing these definitions required that the theorist be apprised of the sense of the basic relation and know "all of reality." The implicit "fiction" required for this step then was the availability of the complete inventory list of the basic relation extension for all possible human experience. How else could the definitions be fixed on the basis of the basic relation? The reconstructive theorists' knowledge of "all of reality" had to be transposed into the phenomenalist mode in which the reductive definitions were phrased. (That Carnap required this additional "fiction" becomes clear also in his response to Goodman's criticism that an individual's peculiar experiences could confound the reconstruction of his knowledge: according to Carnap, the co-extensiveness of explicans and explicandum was meant to hold "for any person [...] independent of the accidental selection of his observations." Since Carnap aimed only for a rational reconstruction of empirical knowledge, he obviously thought the fiction of an exhaustive list of the possible extensions of the basic relation to be just as harmless theoretically as the fictions he explicitly noted. Short of rejecting the strategy of rational reconstruction, Carnap appeared safe against these objections.

A related kind of objection would hold that the concept of justification is a concept which imputes self-reflection, and which accordingly cannot be shown to apply where no reference to the contents of an individual's experience is made. Carnap's concept of justification consisted in the satisfaction of the necessary and sufficient conditions for the application of empirical concepts which were spelt out by their expansion in the constructional language, in short, their reduction sentences. His concept of justification

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64 Carnap 1928a:159/60.
65 Ibid.:160/1.
67 Carnap 1928a:80.
thus was a formal logical one, "implication".\footnote{Carnap 1928b:306.} Justification seen mathematically as
provability becomes pure deducibility, that is, in the \textit{Aufbau}, reducibility of empirical
content to the strictly verifiable given.\footnote{Carnap in 1928a:292 still seems to be aiming (unlike in 1928b:328 soon after) at strict verifiability: he
stressed that the number of elements connected in his reduction chains was finite.} Against the objection that this concept of
justification was mistaken, Carnap would again stress that he aimed only for a rational
reconstruction. It was sufficient for Carnap that his concept of justification was so
employed that it returned the focus of his structural scientific method back to the (Cartesian)
phenomenalist starting point: an individual’s experience did, after all, figure in the
reconstruction of his knowledge, albeit under a structuralist description.

On the face of it then, Carnap’s phenomenalism and his structuralism cohere: the
tension appears to be resolved. Carnap’s formalist structuralism gave a new lease on life to
the phenomenalist conception of empiricism on the one hand and, on the other, formal
science’s structural method became elevated to epistemological relevance by Carnap’s
Cartesian presuppositions.

The next question is whether Carnap’s phenomenalism and his structuralism are not
only compatible, but whether some dependence obtains between them; that is, whether
Carnap could have pursued his alternative neo-Kantian strategy in a non-phenomenalist
setting, had he so chosen. Carnap’s answer to the question of what makes objective
knowledge possible was that it was the form of cognition which made for its objectivity.
What does it mean to speak of "the form of cognition" or "the form of knowledge"? As
noted, Carnap conceived of knowledge along broadly Cartesian lines: it was effected by
epistemically self-sufficient individuals. Carnap reconstructed the form of knowledge as
the logical form of statements expressing the content of an epistemic subject’s mental state.
Ultimately then, what made for the objectivity of knowledge was the form of the
propositional content of an individual's consciousness. Our question thus becomes one of whether the propositional content of a given consciousness could be structuralised if it were not phenomenalistically reducible. The answer to the foregoing is 'no'. If propositional content were not phenomenalistically reducible, or if the phenomenalistic reduction were to stop with more than one basic relation, or even if the one remaining relation of the Aufbau could not be structuralised, then a grasp of the meaning of the relevant concepts on part of the reconstructed epistemic subject would be presupposed. And that would undermine the structuralist program which sought to avoid all recourse to grasplings of intuitive contents as unduly subjective. In the Aufbau, Carnap required the phenomenalist reduction to make his alternative neo-Kantianism work.

3.4.2 The Tension in Carnap's Semi-Naturalism

Despite the outward compatibility of phenomenalism and structuralism and the need of the latter for the former just exhibited, there nevertheless remained an underlying tension between the motivations for them. This tension concerned Carnap's desire to remove epistemology from the realm of traditional First Philosophy. This desire can be detected in the reasoning towards his adoption, first, of the phenomenalist base and, secondly, of the structuralist methodology.

Consider Carnap's structuralism. As argued by the new canon, Carnap's aim to "establish the thesis that science deals only with the description of structural properties of objects"70 was meant to further his defense of the possibility of the objectivity of knowledge. Carnap thus accepted that science met the conditions for this objectivity at least in principle. To explicate how science really did do so was the task of his reconstruction: show all relational predicates translatable into structural definite descriptions. Structural!

70Ibid.:19.
definite descriptions were the logico-reductionist transforms of the regular relational predicates of science. Carnap's project can thus be seen as an attempt to explain our empirical knowledge with the help of science itself—albeit with the help of formal science and from a traditional, philosophically motivated, starting point. (Logic and mathematics were presupposed in Carnap's constructional system.)\textsuperscript{71} In as much as logic and mathematics are part of the sciences as a whole, Carnap's conception of justification of empirical claims as their logical derivability from the given showed a certain naturalistic impulse.\textsuperscript{72}

Now consider Carnap's mentalism and recall that Carnap's adoption of methodological solipsism followed from a priori considerations about what an epistemic order providing justification for empirical claims would look like. Here Carnap, the purportedly scientific philosopher, did not follow the prescriptions of science, namely behaviourism. Rather, it was he who legislated the domain of science: Carnap demanded that there exist also an introspective psychology. "Science as a whole [...] needs a behaviouristic as well as an introspective psychology; in general it needs both an experiential and a materialistic derivation of all concepts."\textsuperscript{73} Carnap reasoned that science required a materialistic derivation of its concepts, because science itself (behavioural psychology) made reductive claims about mental objects and only in this way was able to integrate them into "the only domain (namely the physical) which is characterised by a clear regularity of its processes."\textsuperscript{74} But why did science need the experiential derivation of its concepts - was the materialistic one not sufficient to demonstrate the unity of its object domain? Why lumber science with (pre-conceived) epistemological concerns?

\textsuperscript{71}Ibid.: 176.
\textsuperscript{72}Given the contentless conception of logic and arithmetic as purely tautologous no circularity is involved for no empirical findings are presupposed in this conception of justification. Matters are different, of course, once the logicist program is recognised to fail its aim.
\textsuperscript{73}Carnap 1928a: 96.
\textsuperscript{74}Ibid.: 95.
My answer is that Carnap required empirical science to accommodate his conception of epistemic priority because he wished epistemology to become scientific. Carnap did not, of course, go so far as to declare epistemology itself an empirical science, yet he attempted to remove it from the supra-scientific domain of First Philosophy: epistemology became a formal inquiry, namely, the logic of science. If epistemology was to proceed scientifically (even if only in this "formally" extended sense) its concepts too must be scientific; if a scientific foundationalist epistemology was wanted, then science must furnish us with the required mentalistic concepts. Were behaviourism to be accepted in the form in which Carnap rejected it, namely, as claiming to mirror epistemic priority as well, then his constitution system would be quite beside the point: his reconstruction could not count as a scientific one, for any mentalistic terminology would be alien to science. Science thus also required an experiential derivation of its concepts, because without it epistemology would stand wholly apart from science.

In both his structuralist and his mentalist choices for the form of the constitution system, then, Carnap can be interpreted to be implementing a "semi-naturalistic" conception of scientific philosophy. Carnap's conception of scientific knowledge did not mean to explain empirical science by empirical science; it meant to legitimise the knowledge claim of science by recourse to the resources of formal science. Yet as I just showed, it also meant to legislate what's scientific in order to accommodate one's epistemological presumptions. Therein lay the tension of Carnap's semi-naturalism. Carnap's "scientific" impulse here was clearly conditioned by a "philosophical" conception of knowledge. Carnap's Cartesian presuppositions determined the setting of the epistemological problem, which subsequently was to be solved by the resources of formal science. (That this philosophical legislation of science represented a falling back into First Philosophy was to be a major point of
Neurath's criticism of Carnap, despite his claims to be providing only a rational reconstruction.\(^75\)

### 3.4.3 The Problem of the Basic Relation

Quite apart from this tension between the motivations for the phenomenalistic starting point and the structural methodology of the inquiry, there was an important technical hitch to Carnap's project. This hitch was different from the problems pointed out by Quine and Goodman as fatal to Carnap's project. The hitch in question was that the method of definite structure descriptions required that all descriptive concepts be dealt with purely in terms of their form. How then did the basic relation of remembered similarity fare with this requirement? Here Carnap's ambitions came to a fall, as Carnap himself came close to realising.

In three sections of the *Aufbau* (which he said "may be omitted") Carnap noted the problem.\(^76\)

[After the constructional system has carried the formalisations of scientific statements to the point where they are merely statements about a few (perhaps only one) basic relations, the problem arises whether it is possible to complete this formalisation by eliminating from the statements of science these basic relations as the last, non-logical objects.\(^77\)]

Carnap first sought to substitute, as a structural definite definition for the basic relation, the condition that the basic relation be such that it allow the higher-level structure descriptions to be applicable: "definite descriptions of the basic relations could be formulated with

\(^{75}\)See chapters 4 and 6 below.
\(^{76}\)The importance of these sections was pointed out and Carnap's problem specified first by Kueng 1963:86-90, then Kraut 1971: and Rungaldier 1984: (both of the latter following Kueng's conclusion of intensionalism at the heart of Carnap's supposedly extensionalist construction, noted by Heidelberger 1985:153n.37 (with acknowledgement to A. Kamal) and extensively discussed by Friedman 1987 (following earlier remarks in Friedman & Demopoulos 1985 and Friedman 1983). See also Cohen 1963.
\(^{77}\)Carnap 1928a:235.
reference to the behaviour of objects on a sufficiently high level." In this fashion, the reliance upon the meaning of the basic relation could be avoided and replaced by a structural condition. But did this procedure work? Carnap noted that it did not.

All we have to do is to carry out a one-to-one transformation of the set of all basic elements into itself and determine as the new basic relation those relation extensions whose inventory is the transformed inventory of the original basic relations. In this case, the relation extensions have the same structure as the original ones (they are "isomorphic") [...] Thus all statements of the constructional system continue to hold, since they concern only formal properties.

This transformed inventory of the basic relation extension did not preserve the sense of the original: with the relata changed, there was no guarantee that any sense could be made of it at all (never mind that it was understood as "remembered similarity"). This meant, of course, that the envisaged method of defining the basic relation structurally did not single out one and only one such relation as a definite description should. (Alternatively, one might conclude that it was more than the structure of the relation that mattered here.)

To complete the structuralisation of knowledge, Carnap had to delimit the conditions of applicability for the basic relation as structurally defined to just one. His solution started from his recognition that, after the envisaged transformations, the inventory lists of the basic relation extension were "lists of pairs of basic elements without any (experienceable) connection" Carnap accordingly required that the structural descriptions which were to be substituted for the intuitive descriptions of the basic relation should be restricted to "experienceable 'natural' relations" which he called "founded relations". (That means that only those relations could serve as the basic one to which we could attach the intended meaning of "remembered similarity") To complete his structuralist project

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78 Ibid.
79 Ibid.:236.
80 Ibid.,
81 Ibid.
then, Carnap required that the concept "founded relation extensions", a concept which clearly made reference to empirical conditions, be counted a logical one!

Carnap himself noted that this supposition constituted "an unresolved problem".82 Yet in defense of his ploy he wrote:

That this concept [foundedness] is concerned with the application to object domains is not a valid objection to introducing it as a basic concept of logic. The same is true for another basic concept of logic, namely, generality: 

"(x)fx" means that the propositional function of fx has the value true for every argument of an object domain in which it is meaningful. Logic is not really a domain at all, but contains those statements which (as tautologies) hold for the objects of any domain whatsoever. From this it follows that it must concern itself precisely with those concepts which are applicable to any domain whatever. And foundedness, after all belongs to these concepts.83

Carnap’s idea was to save both the generality and the uniqueness of his reconstruction by insisting that the theory is true over every domain which is "founded". (Apparently, he presumed that there is only one such founded domain.)

Carnap’s argument does not work here due to its faulty analogy. "Foundedness" is not a concept of the same order of "concern with application to object domains" as the concept of generality, the universal quantifier. "Foundedness" topicalises what the universal quantifier takes for granted and spells out and restrict what the universal quantifier presupposes, namely application to an object domain. "Foundedness" restricts application to intended domains and rules out others: only by such under-cover intensional means did Carnap’s "formalisation" procure the "structural definite descriptions" it required.

The question arises how much of a problem this was for Carnap. Carnap did not indicate why he thought that the adoption of the concept of founded basic relation

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82 Ibid.: 238.
83 Ibid.: 237.
extensions as a basic concept of logic was not the final solution of the project of complete structuralisation of empirical knowledge. Consider therefore a modern criticism. As Friedman puts it, the original motivations of the program to structuralise knowledge and disengage objective knowledge from subjective intuition "have been totally undermined by Carnap's final move". The concept of foundedness only refers us back to the "stuff" of intuition. I believe that this criticism is correct, as is that of Kueng, namely that Carnap ended up relying on something like the intensional notion of intended interpretation and so undermined his extensionalism.

It is worthwhile to note, however, why for Carnap the move of declaring "foundedness" a logical relation, while obviously seen as problematic, was not all that bizarre. To see why what Friedman called a "total undermining of the program", and what Kueng and others called an "illegitimate reliance on intensional concepts", did not appear to be so tainted to Carnap, one must consider the Russellian conception of logic he still entertained in 1928. This was the conception that logic provides the specification of what holds for any object whatsoever, that it establishes, as it were, the ground rules for the arrangement of the furniture of the universe. Only on the basis of this conception did it make sense to even only contemplate his proposal to think of the "undefinable" concept of foundedness, namely the experiencability of relations, as a logical concept. Just this conception of logic was soon overcome by that of logic as the specification, in a metalanguage, of linguistic frameworks within which the world is to be comprehended.

By the time this shift had taken place, during stage two of the protocol sentence debate, the program of structuralising knowledge too had shifted to new ground rules. As far as Carnap was concerned, it was no longer a question of completely structuralising the

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84Friedman 1987:ms.19
85Kueng 1963:89/90.
phenomenal given, but of structuralising the language in which a theory could be
expressed. Stage one of the protocol sentence debate, which took place prior to this shift,
served to spell out certain misgivings raised in the Vienna Circle against Carnap's first
attempt at structuralising empirical knowledge. It is to these criticisms which began the
protocol sentence debate that I turn in the next two chapters.

3.5 Summary

To conclude. I argued against the traditional and for the new canonical view of
what the main point of Carnap's Aufbau was. The Aufbau must be distinguished from
Russell's and Whitehead's phenomenalistic foundationalist program, which it partly
implemented, on account of Carnap's alternative neo-Kantianism. Carnap sought to defend
the knowledge claim of empirical science by exhibiting how scientific assertions could be
justified in principle. Carnap sought to provide a phenomenalistic reduction of all concepts
which meant to show the possibility of objectivity to rest in the analytical framework of
cognition.

As a final amplification, let me note then that Carnap, along with Schlick, operated
with a different conception of knowledge than Russell did. For Carnap, there was no
knowledge "by acquaintance", all knowledge was knowledge "by description"; knowledge
did not consist in the experience of content, but in the possession of appropriate cognitive
structures. Cognition was objective only in virtue of its form, not its content stuff. For the
same reason too, Carnap had to be more radical than Russell in his reconstruction: even
sense-data, for Russell the objects of direct acquaintance, had to be formally constituted!
Therein lies the reason why Carnap himself was troubled by the problem of the basic
relation. If all he had attempted to do was to give a phenomenalistic reduction of empirical
knowledge that did not even presuppose isolated sense data, why should the failure to
properly formalise the basic relation have been considered problematic? (For that matter,
why only use one basic relation?) Without a complete structuralisation, that is, with residual reliance upon intuitive, ostensive meaning, the phenomenalistic program would still have been fulfilled, but not the neo-Kantian one (at this stage).87

So much for the aims of the Aufbau on its own terms. As for its successes and failures, I defended its claim to reconstruct empirical knowledge as its synthetic theorems, but noted that it presupposed a grasp of the analytic-synthetic distinction. I noted its failure to provide a complete phenomenalistic reduction and showed its failure to live up to its extensionalist promise with respect to the problem of fully structuralising the basic relation. In passing, I also noted further criticisms that could be made of the Aufbau, and Carnap's presumable response.

According to Kueng's and Friedman's evaluation, Carnap's project comes to a fall entirely on account of its incomplete and, some would say, incompletable structuralism. Their criticism of the Aufbau is different then from Quine's which focusses on the failed phenomenalistic reduction, and from Goodman's which focusses on the alleged failure of the structuralist method of quasi-analysis to provide a universal framework for phenomenal givens, in other words, the interaction of structuralism and phenomenanism. As we shall see in the next chapters, the criticisms levelled against the Aufbau by its Viennese critics were still different ones, though similar to Goodman's in scrutinising the compatibility of phenomenanism and structuralism.88 These Viennese criticisms were of two kinds. They came from proponents of the two epistemological perspectives Carnap had striven hard to show broadly compatible: the philosophical foundationalist and the scientific structuralist-

87 It is interesting to note that in retrospect Carnap remarked that the project of the Aufbau could be pursued by using more than one basic relation. Does this mean that he abandoned (or never even embraced) what I called his alternative neo-Kantianism? Here the answer is that it was the embrace of semantics which allowed him to to specify meanings in formal ways previously not available.
88 Taking account of these modern criticism has nevertheless been necessary here, not only to give a reasonably rounded picture of the Aufbau, but also to show the first symptom of what turns out is a chronic difficulty with Carnap's work, for short, his reliance on a pre-theoretic of meaning. See below sections 6.4.1 and 10.3.1.
cum-naturalist one. Both criticisms of the Aufbau are *prima facie* similar in that both claim that something important is missing from Carnap's reconstruction. For both critics, Carnap's formal-structuralist methodology and his phenomenalist-foundationalist presuppositions are incompatible. Both critics attacked, of course, different aspects of Carnap's reconstructive abstraction. The phenomenalist-foundationalist critic objected to Carnap's methodology and held that the justification of knowledge could not proceed in the formal fashion espoused by Carnap; the structuralist-naturalistic critic objected to the Aufbau's ground plan and held that the knowledge justified was not representative of that gained in scientific practice. Both kinds of critics then objected that something in the process of justification and/or in the evidential base of scientific theories was wrongly conceptualised in Carnap's reconstruction. It is this contention that requires us to regard these early criticisms of the Aufbau as constituting stage one of the protocol sentence debate, and to view the Aufbau as the background against which this debate took its start.
Chapter 4

STAGE ONE STAGE RIGHT:

SCHLICK'S FOUNDATIONALIST CHALLENGE TO THE AUFBAU

Abstract: Schlick's dissent from the epistemological conception of Carnap's Aufbau. First the points of agreement between Schlick and Carnap (4.1). Schlick's conception of the task of philosophy and give a first diagnosis of their disagreement (4.2). The details of their differences over the issue of the certainty of knowledge (4.3) and the issue of the representability of logical form (4.4). Summary of Schlick's view and its main difficulty, contrast his conception of scientific philosophy with Carnap's (4.5).

Before I can turn to the disagreement between Schlick and Carnap I must show their far-reaching agreement. I then turn to the specification of the conflict between the conception of epistemology put forward by the Aufbau and that advocated by Schlick. This disagreement, it must be noted, was not explicitly commented upon by either. To show that it nevertheless obtained--and, furthermore, revealed their differing conceptions of the epistemological task--is the plaint of this chapter.

4.1 Schlick's Agreement with Carnap: The Depreciation of Intuitive Content

I already noted that Carnap's structure descriptions were closely related to what Schlick had long championed as "implicit definitions" (a la Hilbert). Here's how Schlick's and Carnap's views on the structural nature of knowledge interacted in a mutually supportive fashion.

Like Carnap, Schlick was concerned with the distinction between form and content and its relation to epistemology. (Again, "content" here refers to the intuitive materials of
experience.) In his Allgemeine Erkenntnislehre of 1918 Schlick declared implicit definitions to be the most precise form of meaning determination. They were the only ones which provided for defense against the sceptic, who held that nothing could ever be known due to the fleeting and amorphous character of experience. In implicit definitions, concepts were defined as exactly those satisfying certain specified axioms, and thus as standing in precise logical relations to the concepts employed in the definiens. Implicitly defined concepts remained unafflicted by the haziness and impreciseness of intuitive concepts. Intuitive concepts were defined "explicitly", that is, ostensively; their meaning made reference to the contents, the "stuff" of intuition. There was no guarantee that their meaning was held correctly fixed in memory. Only the clarity and stability of implicit definitions, that is, only the structural specification of the form of cognition, made the defense of knowledge claims against the sceptic possible.

The clarity and stability of implicitly defined concepts was bought for a price, however. Schlick noted that

it is through concrete definitions that we set up the connection between concepts and reality. Concrete definitions exhibit in intuitive and experienced reality that which is henceforth to be designated by a concept. On the other hand, implicit definitions have no association or connection with reality at all; specifically and in principle they reject such association; they remain in the domain of concepts. A system of truths created with the aid of implicit definitions does not at any point rest on the ground of reality.  

Implicitly defined concepts, and the systems of concepts to which they belong, do not of themselves identify their relation to and their bearing on reality. How then could the applicability to reality of an implicitly defined system of concepts be established? The question of the applicability of a conceptual system amounts to the question of the truth of the descriptions made with its help. What then was Schlick's concept of truth?

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1Schlick 1918/25: 29, 38.
2Ibid.: 37.
For Schlick, truth was defined for judgements by which he meant the proposition assented to in an act of judgement. A true judgement was one which "uniquely designate[d] a set of facts"; truth consisted in the "unique coordination", or correspondence, of judgement and facts. Schlick adopted this view in opposition to one which held that truth consisted in an agreement between thought and its object such that there obtained some sameness or similarity between them. Judgements did not "picture" reality; instead, judgements merely designated--did not "copy"--the relations between the objects designated by the concepts employed in it. A unique designation was one where one and only one set of facts was coordinated with the judgement; a false judgement was one with which different classes of facts were coordinated. I shall not stop to worry about the obscurities of this account of truth nor extol its merits; important for present purposes rather is the depreciation of intuitive content in the conception of knowledge implied by it. As Schlick put it, "scientific knowledge is only a conceptual sign system."  

With the concept of the truth of a conceptual system so defined, the question was, what gave a system of implicitly defined concepts its connection with those terms which could straightforwardly be coordinated with the facts? The connection between systems of implicitly defined concepts and reality was, that one could establish a satisfactory description of reality only with their help. The system of implicitly defined concepts provided an abstract logical structure which, if suitably related to intuitive concepts whose designata were ostensibly identifiable, allowed for the generation of prediction from observation. This suitable relation, the bridge between the system of implicitly defined concepts and reality, was provided by further definitions of at least some of the already implicitly defined terms in terms of concepts of measurement.  

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3Ibid.: 60-62; see also Schlick 1910:90-94. Schlick's term for "correspondence" or "coordination" was "Zuordnung" which could also be rendered as "assignment".

4Schlick 1918/25:363.

5Schlick 1917:265.
concepts of abstract theory were to be grounded in reality by operational conditions of their use. The specification of these operational conditions was effected in ostensibly defined terms.6

So far, so good. One year after the publication of the second, enlarged edition of Allgemeine Erkenntnislehre, Schlick noted a further advantage of implicitly defined concepts, one which would have made them absolutely indispensible, even if they had not been so already. Not only were knowledge claims phrased by their means immune from the sceptical doubt which foisted on the fleetingness and vagueness of immediate experience, but they, and they alone, also were also the only proper building blocks of knowledge.

Since, out of the immense multiplicity of our experiences, nothing belonging to content can be the object of a statement, no other meaning can be coupled with any statement save this, that it expresses purely formal relations. And whatever is meant here by 'formal relation' or 'property' must be drawn from the theory of implicit definition.7

Experience, the acquaintance with intuitive contents, did not amount to knowledge by itself.8 Knowledge was provided only by the logical structure of propositions or judgements.

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6 This was the move Frank lauded as decisive in providing a synthesis of the views of Mach and Poincare. Frank described the development of Viennese scientific philosophy by as one of seeking a synthesis between the theories of the positivist Mach and the conventionalist Poincare. The opposition between the two was the following. "According to Mach, the general principles of science are abbreviated economical descriptions of observed fact; according to Poincare they are free creations of the human mind which do not tell us anything about observed facts." (Frank 1949:11/2. While Frank speaks perhaps specifically of the "first" Vienna Circle his characterisation here is applicable to Schlick as well.) Frank hailed Schlick for having made the decisive move towards the desired synthesis. Schlick proposed to accept the physicist's criterion of reality, i.e., the measurability of a property as the philosophical criterion of reality (Schlick 1917:265; see also 1922:345). Schlick "operationalised" the meaning of scientific terms. Accordingly, scientific theories were to be viewed as creative constructions that were applicable to reality as long as the concepts employed in them could be related back by definitorial chains to acts of measurement, that is, to immediate observations (Frank 1949:28).

7 Schlick 1926:101/2.

8 Schlick 1918/25:83; Schlick 1926. See also above sect. 2.4.
Thus Schlick came to share Carnap's structuralist conception of the meaning of all scientific concepts. Following Carnap's "acute and irrefutable remarks" in his then still only forthcoming Aufbau, Schlick held that even a term like "green"

does not really express what we experience in looking at a green field, it has no affinity of content with the experience of green, but expresses merely a formal relation whereby all objects that we call green are linked together.9

Schlick now embraced the view that this structural conception of meaning held for all meaningful concepts whatsoever.10 Schlick's adoption of Carnap's conception meant that the apparently intuitive concepts of direct observation were, ultimately, just as structurally defined or definable as the highly theoretical implicitly defined concepts. The integration of abstract theory into the world of experience, that is, the applicability of implicitly defined concepts to reality, that is, the empirical meaningfulness of theoretical terms, did no longer require a translation into terms whose meaning was constituted differently, namely, ostensibly defined terms. (That theoretical terms no longer required such a translation did not mean, of course, that they could do without their operational definitions.) Structure ruled over content. (What role ostension did now play Schlick's scheme is an important question I return to below.)

So much for the justification of abstract theory, for the idea of wherein meaning consisted, and for Schlick and Carnap's broad agreement over it. I now turn the issue which divided Schlick and Carnap. One way of conceiving of it is as a dispute over whether the kind of justification-in-principle which Carnap offered was, first of all, possible and, secondly, sufficient for meeting the epistemological task. I shall lead into the issue by considering Schlick's conception of philosophy as formulated in his first contribution to Erkenntnis.

9Schlick 1926:102.
10Ibid.:fn.2.
4.2 Philosophy as the Activity of Meaning Determination

Schlick's "Die Wende der Philosophie ("The Turning Point of Philosophy"), must be read as his own variation on the collaboratively authored pamphlet "Der Wiener Kreis. Wissenschaftliche Weltanschauung". (Schlick was reportedly displeased with the sociopolitical overtones of the pamphlet which had begun the public phase of the Vienna Circle.\textsuperscript{11}) No less programmatic than the pamphlet, Schlick's address declared the "final" turning point of philosophy to have arrived - away from the "anarchy of philosophical opinions" towards a scientific orientation.\textsuperscript{12} Schlick's characterisation of the scientific attitude of the new philosophy agreed in great part with that of the pamphlet; unlike the latter, however, he stressed its still peculiarly philosophical nature. It was this conception of philosophy which conflicted with the epistemological conception of Carnap's \textit{Aufbau}, as I now shall show. (In doing so I shall also cite earlier and slightly later writings of Schlick's than those belonging to the years 1928/9. I can do so because Schlick's position during the protocol sentence debate remained more stable than Carnap's.\textsuperscript{13} My aim here is nevertheless only to show the conflict between Schlick and Carnap's \textit{Aufbau} conception; how Schlick's conception conflicted with Carnap's and with Neurath's later physicalism I shall show in chapter 7.)

\textsuperscript{11}The authors of the pamphlet, dedicated to Schlick on the occasion of his decision to decline a call to Bonn University and stay in Vienna, were Neurath as the principal writer with revisions by Carnap and some help from Feigl and Waismann; Carnap, Hahn and Neurath were the main co-signers (Feigl 1969a:70; Haller 1984). On Schlick’s reaction see Neider 1973 and 1977 and Menger 1982.

\textsuperscript{12}Schlick 1930a:54.

\textsuperscript{13}In fact (as some readers may have already noted) Schlick's 1918/25 anticipated many positions later adopted by the Circle as well as some developed independently by Wittgenstein's \textit{Tractatus} (Carnap 1963a:21, Feigl & Blumberg 1974:xx/xxi, Menger 1982). Among these pre-Vienna Circle and pre-Tractarian positions are the distinction between genuine knowledge and mere experience, the conception of truth as unique correspondence of a statement to a fact, the conception of meaning as given by the rules of language use, the adoption of Hilbert's method of implicit definition, the characterisation of logic and arithmetic as tautological and his dissolution of the mind-body problem as resting on linguistic confusion.
4.2.1 Meaning Determination Through Psychic Acts

Schlick denied, as did Carnap, Hahn, and Neurath in the pamphlet, that there was a domain of specifically philosophical truths. Schlick's reason was that "[p]hilosophy is not a system of statements; it is not a science."14 Nonetheless, Schlick attributed a special role to philosophy, one that went well beyond that of simply being a general theory of science:

[P]hilosophy is that activity through which the meaning of statements is revealed or determined. By means of philosophy statements are explained [literally: "clarified"], by means of science they are verified. The latter is concerned with the truth of statements, the former with what they actually mean.15

Already in the Neurath-Carnap-Hahn pamphlet the new philosophy was characterised as the "logical clarification of scientific concepts, statements and methods".16 Yet as the quotation indicates, Schlick had more in mind than mere clarification: Schlick wanted the meaning of scientific statements to be "explained".17

The first question which arises here is how concern with meaning differentiated philosophy from science. Schlick conceded that the re-definition of scientific concepts by science itself, as undertaken, for example, by Einstein's relativity theory, was both a scientific and a philosophical achievement.18 Conceptual innovation in science was not only scientific but also philosophical work. How then was meaning explanation to be accomplished?

[T]he task of philosophy does not consist in asserting statements [...] bestowing meaning upon statements cannot in turn be done by statements. For if, say, I give the meaning of my words through explanatory statements and definitions, that is, by help of other words, one must further ask for the meaning of these words. This process cannot proceed endlessly. It always

14Schlick 1930a:56. Schlick did not cite Wittgenstein 1921:#4.112 here but did so in this context on many other occasions.
16Neurath et al 1929:316.
17Schlick 1929/34:142 (a 1929 lecture not published until 1934).
18Schlick 1930a:
comes to end in actual pointings, in exhibition of what is meant, thus in real acts; only these acts are no longer capable of, or in need of, further explanation. The final giving of meaning always takes place, therefore, through deeds.¹⁹

The giving or explanation of meaning was to be accomplished with the help of something extra-linguistic which Schlick designated variously as "deeds" or "acts".

It is not really clear, however, what was meant by this²⁰: The following worries arise here. First, not all terms of a language can be defined ostensively, by actual deeds of pointing. A second, and related, worry is that, if Schlick meant that the regress was to be broken only by directly appealing to experience, did he not then retract his longstanding commitment to implicit definitions? This second worry may seem answered by a passage in which Schlick was more careful in his formulation: the "direct exhibition of what is meant [...] can only be the result of an actual operation, a physical or psychical act."²¹ Here, the reference to psychical acts would seem to allow for non-ostensive definitions like the implicit ones long championed by Schlick. However, this more careful phrasing did not fully clarify the matter. What is allowed by this phrasing is that such implicit definitions would still only count as indirect exhibitions of the meaning of concepts which receive their full meaning only when they are linked to concepts whose meaning can be directly exhibited. Even if the first worry is thereby overcome, it seems that the status of implicit definitions had changed again. Did meaningfulness now consist in having an application, rather than in its structural determinations? Third, there is this worry. Bearing in mind Schlick's own distinction between experience and cognition²², it is also not clear whether such meaning determination by experience would constitute

¹⁹Ibid.:57, his italics.
²⁰Even Waismann, hardly an adversary of Schlick's, retrospectively admitted to being mystified at this point (Waismann 1938:xxvii/xxix). He referred the reader to Schlick's 1932a for its supposedly first use, in this capacity, of the verification principle. That principle, however, already made its appearance in this capacity in the very paper at issue here, 1930a:56; more on this below.
²¹Schlick 1930c:xix.
²²Schlick 1918/25:83; 1926.
knowledge by Schlick's own criterion. (What do ostensive definitions do but provide acquaintance?) Schlick's thesis that philosophy consists in meaning determination, then, was less clear than its author took it to be. The correct interpretation of Schlick's conception of philosophy as meaning determination must resolve these unclarities.

4.2.2 The Point of Schlick's Experiential Meaning Determination

What is clear, however, is that Schlick's insistence on the psychic act of ostension as the giving of meaning stood in contrast to the purely formalist conception of meaning determination of Carnap's Aufbau. I now consider the extent of this contrast.

Carnap's Aufbau preserved the intersubjective validity of knowledge because structural properties, and not the qualities of experience were taken recourse to in the epistemological analysis. The Aufbau's explicit non-adversion to intrinsic properties of the given stood, as I noted, in the tradition of Schlick's own earlier championship of implicit definitions. Schlick endorsed Carnaps' Aufbau position: "all knowledge is such only in virtue of its form. It is through its form that it represents the facts known." Schlick thus clearly considered the formal properties of experience responsible for any representation relation obtaining. Schlick did not, however, follow the strategy of the Aufbau completely. For Schlick, as we have already seen, epistemology had to concern itself explicitly with experiential meaning. Schlick disagreed with Carnap on the methodology of exhibiting the meaning of statements and concepts.

To see this difference of methodology more clearly, note that the given was important for Schlick in two ways. First, it provided authentic knowledge in virtue of its

23Schlick 1930a:55; compared his 1926:103.
role in the logical form of statements\textsuperscript{24}, namely as the terminus of the reduction chains by which every statement exhibited the path to its verification.

\begin{quote}
Whenever there is a meaningful problem one can in theory always give the path that leads to its solution. for it becomes evident that giving this path coincides with the indication of its meaning. [...] The act of verification in which the path to the solution ends is always of the same sort: it is the occurrence of a definite fact that is confirmed by observation, by means of immediate experience.\textsuperscript{25}
\end{quote}

Secondly, the given afforded "understanding" in virtue of the meaning which, with the help of its immediate and indubitable intelligibility could be bestowed upon all empirical statements.\textsuperscript{26} Schlick and Carnap were in agreement on the first point\textsuperscript{27}, but not on the second. According to Carnap, for a representation relation to hold, no meaning interpretation need to be undertaken because there was no self around to interpret the given.\textsuperscript{28} Epistemology thus required no appeal to "experiential meaning", "meaning" apprehended by an individual in a psychic act.\textsuperscript{29} Carnap had no need for "understanding"; that Schlick, by contrast, did have such a need, turned, firstly, on his different conception of the task of epistemology and, secondly, on his different evaluation of the possibility of representing the logical form of statements.

To see the difference in their conceptions of the task of epistemology more clearly, note first that Schlick had not gone back on his earlier assertion that "it is only formal relations [...] that are accessible to knowledge, or to judgement in the purely logical sense

\textsuperscript{24}Schlick used the expression logical form at 1930a:55.
\textsuperscript{25}ibid.:56.
\textsuperscript{26}Schlick 1929/34:E384.
\textsuperscript{27}Compare Carnap 1928a:289.
\textsuperscript{28}Carnap 1928a:103f., where his concurrence with Schlick 1918/25 was explicitly noted. Why despite this apparent agreement Schlick did not follow Carnap is discussed further below.
\textsuperscript{29}In the Aufbau the 'intention relation' which provided the representational relata of an experience was considered to be a relation holding between "an element and a relational structure in which it has a place" (Carnap 1928a:26) In other words, intentionality was reconstructed as constituted by the fact that "the experience stands in a more comprehensive context. What the intention relation expressed could be read right off the formal structure of the experience.
of the word."30 In his 1932 London lectures Schlick defended this view even more staunchly than ever. Philosophy as meaning determination, however, went beyond concern with such formal relations. To understand Schlick's conflict with Carnap here, it is necessary to distinguish what they agreed about from what they disagreed about. They agreed about the broad answer to the question in what meaning consisted, namely, in structural determinations, but they disagreed about how the meaningfulness of a given system of concepts could be established. (This distinction between what meaning consisted in and how meaning was to be exhibited should lay to rest the second worry above.) It is here that the difference between Schlick's concern with the actuality of knowledge and Carnap's concern with the possibility of knowledge comes into focus. In his Erkenntnislehre, Schlick proposed that the applicability of a system of implicitly defined concepts depended on whether it stood in an appropriate logical relation to explicitly, ostensively defined concepts. For Schlick in the 1930's, ostension did no longer provide for meaning itself: its role lay rather in establishing the applicability of the conceptual system at hand, that is, in its empirical meaningfulness. This points to the answer to the question of what meaning determining acts were carried out for.

Recall Carnap's Aufbau. It was entirely a system of what Schlick would call implicit definitions: officially at least, it renounced all reliance upon intuition, it provided nothing but structural meaning determinations. Moreover, Carnap only tried to show by how the justification of empirical knowledge could proceed in principle, and what meaning consisted in such that objective knowledge was possible.31 It was on these points that Schlick dissented from Carnap. For Schlick, philosophy had to go further. It was not sufficient that the mere possibility of knowledge was established; philosophy had to show that this possibility was actualised. Philosophy as meaning determination also had to

30Schlick 1926:103.
31Carnap 1928a:190.
establish the **actual applicability** of a conceptual system if it was to discharge its epistemological task. Now, it may well be thought that Carnap had discharged this task too. It speaks against this view, however, that Carnap was unperturbed by the possibility that his constitution system might be mistaken in detail: many of its definitions depended on the results of the empirical sciences. For Schlick, this permissiveness of Carnap's was, as we shall see, highly problematic. It displayed indifference to what he deemed the overriding concern of epistemology with establishing the actual applicability of a system of concepts. In the next two sections I specify this difference in epistemological aim and the further difference, partly dependent on it, of their epistemological methodology.

### 3.3 The Issue of the Certainty of Philosophical Knowledge

Schlick required more than justification in principle. Scientific theories had to be shown in fact justified. For Schlick, this meant that philosophy as meaning determination had to show the justification of knowledge with certainty. "Philosophy must supply the ultimate support of knowledge".32

Carnap's determination of the meaning of empirical statements was not of the kind that produced certainty. Consider what goes into the formulation of the reduction chains by which the terms of empirical science were defined. The criterion of reduction was:

> We call an object a 'reducible to the objects b, c, ...' if, for any state whatsoever, relative to the objects a, b, c, ... a necessary and sufficient condition can be indicated which depends only upon objects b, c, ...33

Carnap noted that "the process of cognition [...] is overdetermined."34 How then did Carnap decide which of the conceivable reductions to adopt for his constitution system? Carnap relied on the results of empirical science upon which the "detailed execution" of a

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32Schlick 1930a:58.
33Carnap 1928a:80; see also ibid.:60/1 and 1928b:310.
34Carnap 1928a:146, 1928b:311.
constitution system would have to depend.\textsuperscript{35} Here Carnap's logical construction of the world shows its bootstraps. It was characterised by this presupposition: any result of epistemological analysis is obtained only on the prior assumption of a particular set of thorough-going empirical regularities. This assumption allowed the epistemologically necessary and sufficient constituent of a given experience to be distinguished, and thus pointed to the route of reduction. Obviously, this assumption was not unsubstantial: the assumption of a different set of regularities would result in a different constitution analysis of the concepts concerned.

Note that I am not charging vicious circularity here. Carnap held, and I follow him here, that his method did not involve vicious circularity, because, while he appealed to special sciences for indications about how the reduction of given concepts was to go, he did not "introduce this special science as a presupposition for a valid system of knowledge."\textsuperscript{36} In other words, it was not necessary for his demonstration of the possibility of objectivity that any particular science he appealed to have the correct result. What is nevertheless notable, and gave grounds for objection to Schlick, is this. Carnap's rational reconstruction gave only a conditional justification of empirical knowledge of the form: if our science is at least roughly true, then a statement is justified if and only if (i) its logical form is of a kind which is transformable into one which demonstrates how this statement is in principle verifiable on the basis of the immediate given, and (ii) it is indeed so verified.

Concentrating one's attention on the analytic theorems of the \textit{Aufbau} one is easily mislead to think of Carnap's system as providing an a priori and therefore indubitable justification of scientific knowledge claims. Now, Carnap's rational reconstruction was

\textsuperscript{35}\textsuperscript{Carnap 1928a:190.}
\textsuperscript{36}\textsuperscript{Carnap 1928b:314.}
indeed a priori: it did not consist in further empirical inquiries into the process of knowledge acquisition, but in the logical reformulation of scientific statements. The justification provided was not, however, an indubitable one: behind Carnap's definitions stood the empirical results of science, and there was little indubitable about them. Carnap's constitution system provided little by way of indubitable truths. His reconstruction of empirical knowledge is better understood as a model of an epistemic subject as a detection machine for objects, properties and states of affairs, which was programmed according to the latest scientific analyses of both objects and detection mechanisms.

Carnap's conception of meaning determination thus ran counter to Schlick who held that:

the concept of probability or uncertainty is simply not applicable to the acts of giving meaning which constitute philosophy. It is a matter of positing the meaning of statements as something simply final. Either we have this meaning, and then we know what is meant by the statement, or we do not possess it, in which case mere empty words confront us, and as yet no statement at all. There is nothing in between and there can be no talk of the probability that the meaning is the right one.\(^{37}\)

Carnap's rational reconstruction clearly failed this test of indubitable certainty: the relativity of his determinations of logical form to the assumption of certain forms of empirical regularities obtaining was just what Schlick said had no place in philosophical meaning determination. If scientific knowledge claims are to be shown as in fact justified, then a merely conditional justification would not fit the bill.

4.4 The Issue of the Representability of Logical Form

Carnap's conception of the scope of epistemology was not the only problem with the Aufbau for Schlick. The other problem was not, however, as may be suspected, the unresolved problem of its residual reliance upon intuition in the determination of the sense

\(^{37}\)Schlick 1930a:58/9.
of the basic relation (in response to which Carnap made do with declaring the concept of an
experiencable relation a logical one). Even if had Schlick noticed that Carnap was
ultimately forced to cheat (there is no clear evidence that he did) it would have provided him
with only further ammunition. Crucial to Schlick's objection to the Aufbau was rather his
view that the relation of correspondence between judgements and facts can only be
"shown", that is, experienced, but not "said", that is, propositionally specified: "The
meaning of a word must in the end be shown, it must be given." 38

4.4.1 Schlick on the Unrepresentability of Logical Form

For Schlick the actual applicability of a conceptual system had to be established
experientially. This the Aufbau clearly did not do. Did Schlick overlook here that Carnap
was merely providing a rational reconstruction? I noted in the previous chapter that
Carnap's procedure seemed secure against claims of its psychological unreality on account
of it being only a rational reconstruction. It is thus important to note that the psychological
reality of the epistemological reconstruction, towards which Schlick was driven, was not a
primary motive for Schlick's dissent from Carnap. It followed rather from philosophical
considerations about meaning and how they interacted with his conception of the aim of
epistemology.

Schlick held that the applicability and empirical meaningfulness of a conceptual
system was to be established experientially not only in actual practice; even in theory the
relation of unique correspondence between statement and fact could not be otherwise
established. Schlick was forced to retreat into the experiential realm, as it were, because of
what he perceived the limits of the formal method of inquiry to be. This perception derived
from Wittgenstein. With Wittgenstein, Schlick held that logical form "cannot itself be

38Schlick 1932a:87, his italics.
represented.\textsuperscript{39} (More on this below.) According to Wittgensteinian, any statements claiming to represent logical form were strictly speaking meaningless: there could not be such a thing. At best they were, to be viewed, like those in his own \textit{Tractatus}, as ladders which once climbed to reach a higher level must be thrown away.

At this point one may wonder why Schlick did not elect to treat the \textit{Aufbau} in the fashion of the \textit{Tractatus}. To see this, note how the question of the aim of epistemology played into the issue of the methodology of meaning determination. If only the possibility of objectivity had been at issue, then the representations of logical form devised by Carnap would, perhaps, have been acceptable as an expository device and harmless make-believe, comparable to the propositions of the \textit{Tractatus}. Since Schlick, however, was concerned to establish the actual justification of knowledge claims, such demonstrations as Carnap's of how such justifications would go in principle could not be adapted to his purpose: actual justification better not rely on strictly speaking meaningless statements. Consequently, Schlick had to reject Carnap's formal method of structure descriptions and develop his experiential approach to meaning determination.

What were the reasons for the view that logical form was unrepresentable? Schlick's use of the expression "logical form" comprehended the formal structures of linguistic expressions upon which their meaning depended and went back to Wittgenstein.\textsuperscript{40} Wittgenstein himself called "logical form" that which a proposition must have in common with what is pictured in order to picture it.\textsuperscript{41} For him, logical form consisted in the arrangement of simple objects relative to each other, an arrangement the

\textsuperscript{39}Schlick 1930a:55; see also his 1932b. Compare Wittgenstein 1921:#2.172, #4.12.
\textsuperscript{40}Schlick 1930a:55. For Carnap's use this term see 1928a:79. Carnap's constitution system could be understood as a Tractarian system exhibiting all empirical statements as truth-functional transformations of elementary propositions (Wittgenstein 1921:#5.3); the latter were sensualistically rendered as pertaining to relations of remembered similarity. Whether such a reading is true to Wittgenstein's intentions is another matter, of course, but one that need not be gone into here.
\textsuperscript{41}Wittgenstein 1921:#2.1621
type of which the representing linguistic objects had to share with the objects they
represented. Now, understanding a proposition meant to know what was the case if that
proposition was true; understanding a proposition thus meant to appreciate, however
unconsciously, the import of its logical form. This logical form, however, could not itself
be represented by propositions, for the following reasons. First of all, following the
prescription of the theory of types, on pain of paradox no proposition can make a statement
about itself. Secondly, the logical form of a proposition cannot be represented by a yet
other proposition. Given Wittgenstein's theory of picturing this other proposition would
have to have the logical form of what it represents in common with it. In order for to
specify the logical form of it follows that must itself possess the logical form of ;
thus would be saying something about itself and so violate the strictures of the theory of
types. Behind Wittgenstein's view on the non-representability of logical form stood his
view of the correspondence theory of truth: a true proposition required the commonality of
logical form of between representation and the represented. It was to guard such
correspondences from leading to logical contradictions as they would if self-referential
propositions were to be allowed that Wittgenstein ruled logical form to be non-
representable.

Schlick accepted these Wittgensteinian points. It should be noted that no
discontinuity with Schlick's pre-Wittgensteinian views is involved here. When, in his
Erkenntnislehre, Schlick had inveighed against the definition of truth as the "agreement of
thought and its objects" he had inveighed against the notion of agreement as a picture in
terms of similarities of intuitive qualities. "A judgement pictures the nature of what is
judged as little as a musical note pictures a tone, or as the name of a man pictures his

42Ibid.:#4.024.
43Ibid.:#4.12.
44Here I follow the exegesis of Hart 1971.
45Wittgenstein 1921:#3.332.
personality." Schlick's notion of truth as unique correspondence or coordination between statement and fact was preserved in the Tractarian conception: what was "picted" according to the Tractatus was, after all, only the logical structure of facts, not their intuitive qualities. It was the atomic elements of true propositions and of the represented fact that remained uniquely coordinated. Truth as correspondence just consisted in the identity of the logical structure of proposition and fact.

For Schlick then, logical form was the unrepresentable medium of representation. Yet it also was just this logical form which "alone is concerned in cognition." How was this apparent conflict resolved? I believe Schlick resolved it by adopting the following position. Logical form made for the meaning of the propositions in which it was embodied. Philosophy, as an act of meaning determination distinct from science, had to go beyond the formal aspect of the given. If logical form could not be propositionally specified, then it had to "show itself", that is, disclose itself to the the meaning determining consciousness as meaning. By barring the possibility of representing logical form, Schlick was forced to conclude that it was only by adverting to the meaning of the given as revealed in an occurrent psychic event that the representation relation of scientific statements could be exhibited.

4.4.2 Carnap's Defense of the Representability of Logical Form

Unlike Schlick, Carnap believed that it was possible to represent the logical form of statements: nowhere did he declare the Aufbau's reduction chains to be strictly speaking meaningless. On the contrary: his proof of the possibility of objectivity depended on "the possibility, in principle, of translating all scientific statements into statements within a constructional system." It depended, in other words, on the possibility of representing

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46 Schlick 1918/25:61
47 Schlick 1932b:169, 225.
logical form. Since Carnap did not address the issue between him and Schlick, one can only speculate about his response to the Tractarian qualms.

It is important to note, first, that the Carnap at issue here is the pre-Logical Syntax Carnap of the Aufbau and shortly thereafter. It would therefore be wrong to straightforwardly attribute to him the reasoning of the formal "meta-logician" and "logical syntactician" he became in 1930, following his conversations with Tarski and Gödel's groundbreaking discoveries.\(^{50}\) Secondly, it must be noted that most of the Aufbau was written before Carnap attended the Circle's second reading of the Tractatus.\(^{51}\) Carnap's response to the Wittgensteinian criticism thus came (more or less) after the fact of his transgression of Tractarian strictures.

What then was Carnap's response to Schlick's Wittgensteinian challenge? A first hint is provided by noticing Carnap's utterly unloaded notion of logical form:

> We say that a statement or propositional function has been given a logistic rendition if it is expressed in logistic symbols. By the logical skeleton of a statement or a propositional function, we mean its logical structure. [...] We shall say that a statement is given in logical form if it is expressed entirely in words of the natural language, but in such words that there is a unique way, on the basis of either explicit or tacit agreement, of giving the skeleton in logistic rendition.\(^{52}\)

Clearly, this a very neutral use of the term. This suggests that Carnap did not share Wittgenstein's conception of logical form. My guess is that his response to the Wittgensteinian criticism consisted in following at least part of Neurath's critical attitude towards the "unspeakable" of the Tractatus. Carnap once recalled that "Neurath emphasised from the beginning that language phenomena are events within the world, not

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\(^{50}\) Carnap 1963a.

\(^{51}\) See Proust 1987 for a discussion of when exactly Carnap's reading of the Tractatus hit home.

\(^{52}\) Carnap 1928a:79.
something that refers to the world from outside."\textsuperscript{53} Accordingly Carnap reasoning may be reconstructed as follows: Wittgenstein's \textit{Tractatus} was mostly not meaningless but highly instructive; something must therefore be the matter with thinking it to be meaningless, namely, the thesis that logical form is unrepresentable. Let this thesis thus be dropped. What then did Carnap put in place of Wittgenstein's view that logical form was shared between representation and the represented? To see this, we must first rule out two ways in which it might be thought Carnap avoided the issue altogether.

It might appear that Carnap avoided the Wittgensteinian censure altogether because his constructional language did not try to explicate the logical form of another language. It was rather devised as one that showed its own logical form. According to this suggestion Carnap did not specify the logical form of the usual language of science at all. Now, there is admittedly a grey area here. First, as noted, Carnap did not yet employ a meta-linguistic apparatus into the Aufbau. Secondly, his reconstructional language was radically different from the one to be reconstructed. Carnap remarked that "natural languages do not have the general rules which allow us to deduce the meaning of a word from its form."\textsuperscript{54} The constructional language, by contrast, allowed just that. But does this mean that that the constructional language was not intended to spell out the logical form of the ordinary language of science? Already Carnap's definition of logical form quoted above speaks against this. Furthermore, if Carnap did not intend this explication it would be entirely unclear how his rational reconstruction could support ordinarily phrased scientific knowlege claims. Carnap's rational reconstruction was meant to restore to scientific language the perspicuity which, as his teacher Frege had complained ordinary language had lost. Carnap could not then avoid to face the Tractarian challenge head on.

\textsuperscript{53}Carnap 1963a:29, his italics. When that beginning was Carnap did not say but Neurath is reported to have been critical of the \textit{Tractatus} already at the time of the Circle's reading of it.

\textsuperscript{54}Carnap 1928a:34.
Carnap also did not avoid the issue by drawing the kind of distinction between questions about meaning which he did draw. According to Carnap, there was first the "correlation problem" (between which pairs of objects does a given representation relation hold?) and, secondly, there was the "essence problem" (by virtue of what are these objects connected?). Carnap held that, whereas the correlation problems could be dealt with in science, "essence problems belong to metaphysics." Prima facie then Carnap simply avoided engaging the question of what meaning consisted in, what role logical form played in it and whether the latter was representable.

But Carnap did expressly forward an opinion on the essence problem of the designation problem in natural language. More importantly, however, Carnap forwarded an implicit answer to the essence problem in his constructional language. His conception of what made for meaning was the following. In his constitution system it was not the sharing of logical form between fact and statement that made for meaning; instead it was the mere possession of logical form on part of the propositional expression of what was in fact "given". (The specific logical form of an expression then determined the answer to the correlation problem of what meaning it expressed.) Because its mere possession of logical form made for the meaning of a statement, Carnap thought he could talk with meaningful statements about the logical form of other statements without the first statement having to possess the logical form itself, and thus without this first statement having to be self-referential. For Carnap, logical form was not the representational medium which connected a statement with a fact; logical form did not "show itself" in content. Instead he held that logical form was constitutive of meaning, meaning was but logical form. (It is because

55 Carnap 1928a: 34/5.
56 "[T] is somehow brought about voluntarily." (Ibid.: 37.) He continued, betokening the fact that most of the Aufbau was written before he read the Tractatus: "Only rarely has a special essential relation of 'symbolizing' been assumed."
57 Of course, Carnap happily continued to conceive of the meaning of linguistic symbols as a designation relation between sign and signified. The important difference between Carnap and Wittgenstein's ideas here was that for Carnap this relation was determined by the formal structure of the signifying material.

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of this difference between Schlick and Carnap that I wrote above that they "broadly" agreed about what meaning consisted in, namely, logical form. Just how logical form did the job was what they disagreed about.)

It might be asked: if this was Carnap's position, why did Schlick not give up the Wittgensteinian position? It is widely reported that Schlick reacted sceptically when Tarski's thesis was put to him that it was possible to represent logical form by using a meta-language to talk about a given language.\(^{58}\) (This method was not yet followed in the \textit{Aufbau}.) This scepticism is generally attributed to the influence of Wittgenstein's stand on the representability of logical form. Wittgenstein's views commanded such allegiance because they afforded Schlick the continuity of holding to his old conception of truth as correspondence. Just that view of truth was jeopardised in Carnap's syntactic period which started with his acceptance of Tarski's (and Goedel's) arguments for explicitly meta-linguistic discourse.\(^{59}\) Moreover, Wittgenstein's influence was not the only reason Schlick could have given in defense of his reticence to accept Carnap's purely formal rational reconstruction, had he been so challenged. He also required that philosophical meaning determinations possess certainty, as I showed above.

\section*{4.5 Summary: Schlick's Experiential Turn Compared to Carnap's Formalism}

To summarise. For Schlick, the grounding relation between scientific statements about the world and the given was to be exhibited as certain through the provision of the necessary and sufficient conditions of the application of the respective concepts. Because of his acceptance of Wittgenstein's strictures on the representability of logical form, Schlick held that the "explanation" of the meaning of scientific statements could not be given in

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\(^{58}\) Carnap 1963a:30; Neurath 1938a:fn.5; Menger 198.

\(^{59}\) See below chapter 6 and 8.
meaningful sentences. Because Schlick required claims to be justified as certain, the use of such strictly speaking meaningless sentences as a ladder to be thrown away once climbed (like the Tractatus itself) was ruled out (as it may not have been were it just a question of showing in principle what meaning consisted in and how empirical claims could be justified). For Schlick then, it was not through the determination of the abstract logical form but through the experiential determination of the meaning of empirical statements that the foundation of empirical knowledge could be certified. Thus philosophy was named as

the activity indeed (forming the soul of all inquiry) where the meaning of all the concepts necessary for knowledge is explained. It consists in the acts of giving or finding meaning [sense] which lend significance [reference] to all the words occurring in our propositions.60

Because of his different epistemological orientation Schlick had to return to experience as the court of justificatory appeal. The deeds of which he spoke as giving the meaning of scientific statements were what Brentano had called "intentional acts", that is, experiences of their sense.61

Schlick's meaning determinations thus came to be of a nature twice different from Carnap's reductions which equally aimed for a mooring of empirical discourse: they were non-formal and experiential, and they were distinguished by certainty. For Schlick, the system of scientific propositions became a scientific world-view by "understanding" it.62 As I noted, it was a consequence of general considerations that Schlick required the foundationalist postulations to be psychologically real. He required that the recognition of commonly only vaguely understood criteria of correct concept application was made

60Schlick 1929/34:142. The perhaps intended play in the German original on Frege's distinction of "Sinn" and "Bedeutung" is restored in the insertions.
61Schlick 1918/25:22/3 endorsed Brentano's conception of non-intuitive thinking as an intentional act; Schlick did not, of course, endorse Brentano's further characterisation of the "intentional inexistents" involved in these acts.
explicit in the experience of the epistemologist: given their certainty they could thus in principle serve as a corrective for the less self-conscious graspings on the part of practising scientists of the verification conditions which constitute the meaning of scientific statements.

Apart from the specific points I made about Schlick and his disagreement with Carnap, I must also make two general points. First, it must be noted that drawing the distinction of the contexts of justification and discovery did not coincide with conceiving of epistemology along the lines of Carnap's rational reconstruction. (In the next chapter we shall see that Neurath too represents an instance of this.) Schlick and Carnap agreed that the distinction of the contexts of discovery and justification must be drawn. Thus Schlick could consider the philosophical activity of meaning determination to be distinct, despite the parallels obtaining, from the common understanding of empirical statements which is presupposed in all scientific activity.

The second general point concerns my thesis that the protocol sentence debate was a clash of different conceptions of the aims and methods of scientific philosophy. Compared to Carnap's, Schlick's efforts fit much better into the traditional picture of Vienna Circle philosophy. They do so despite what could be termed his own "nods towards" Carnap's alternative neo-Kantianism. (Thus it must be noted that it was apparently due to Carnap's influence that Schlick around 1926 extended his previous view that only structural determinations of concepts are stable enough to rebut scepticism to the view that only structure makes for meaning, and so dethroned ostension.) His main efforts, however, remained directed towards the question of what knowledge is such that it could then be definitely shown that we indeed have knowledge in science.

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63 Carnap 1928a:xvii, 158; Schlick's endorsement is presupposed in his distinction of epistemology from general scientific inquiry in his 1918/25:3 and 1932b:177/8. For a list of endorsements of the context distinction extending back to Kant see Hoyningen-Huene 1987.
Finally, how is Schlick's conception of epistemology to be evaluated in its own terms? Here I return to the question raised early in this chapter. Schlick had long drawn the distinction between acquaintance and knowledge, experience and cognition: "The mere experiencing of an object as being there is not knowledge; it is only the precondition for knowledge."^{64} How did this distinction sit with the experiential turn taken by his epistemology? Here we must first remember the change between his early and later views. As noted, in the 1930s intuition no longer grounded the meaning of ostensively defined concepts: "it is not true that [that] which only intuition can furnish actually enters into the understanding of knowledge."^{65} What intuition did from his Wittgensteinian turn around 1925 onwards was to provide the "inexpressible" and "private" subject matter of thinking^{66}. As I noted, it provided for the applicability of the structurally defined concepts, and thus the exhibition of their meaning. As Schlick put it in his London lectures, "content is content, nothing can be done about it; it is simply there (and even this cannot be 'expressed'), that is all."^{67} Acts of ostension did not, then, provide the meaning of even only intuitive concepts by themselves, but only provided acquaintance with content: "the meaning of our words [is] contained entirely in the structure of the intuitive content."^{68} It seems to me that in arriving at this conception, Schlick noted a problem similar to the basic relation problem in the Aufbau. Structurally constituted meaning had to be anchored in experience, yet this anchoring could not be conceptualised in structural terms in turn. Unlike Carnap in the Aufbau, Schlick did not even try to do so. In place of declaring "foundedness" to be a logical term he postulated the grounding to take place by means of intuitive, i.e. non-cognitive experiential content.

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^{64}Schlick 1925:93. This passage is only in the second edition of 1918/25.
^{65}Schlick 1932b:195.
^{66}Ibid.:168, 209.
^{67}Ibid.:191.
^{68}Ibid.:195.
Schlick's solution was problematic, however, (though in different ways than Carnap's). The applicability of a conceptual system to reality remained a matter of ineffable experience. The tension between Schlick's earlier distinction of experience and cognition and his experiential method of meaning determination was not solved satisfactorily. Shifting the process of legitimisation of knowledge claims to private experience removed them from the sphere of intersubjectively intelligible activity. Schlick, we must note, seems to end up with three ineffables, not just, as often noted\textsuperscript{69}, one: first, what he called "content" and, second, logical form or structure and, thirdly, the private meaning determining, legitimising experience. (I will pursue this problem further below, with reference to the debate between Schlick and the physicalists. It will turn out that he only ended up with two ineffables, after all, but that was still two too many.\textsuperscript{70})

\textsuperscript{69}E.g. Feigl & Blumberg 1974:xx/xxi.
\textsuperscript{70}See section 8.2.6 below.
Chapter 5

STAGE ONE STAGE LEFT:
NEURATH'S NATURALISTIC CHALLENGE TO THE AUFBAU

Abstract: Neurath's challenge to Carnap's Aufbau. Points of agreement between Neurath and Carnap (5.1). Neurath's 1928 criticism in his review of the Aufbau (5.2) and his criticisms in his 1929 address (5.3). Neider's argument which was influential on the course of both Neurath's criticism of Carnap between 1928 and 1929 and on Carnap himself (5.4). Summary and a brief remark about the beginnings of physicalism (sect. 5.5). Appendix: Carnap's representational theory of mind and its failure to assuage Neurath's doubts about the given.

In the previous two chapters I presented the two epistemological conceptions most commonly recognised to be represented in the Vienna Circle. First, the Aufbau, an example of Carnap's alternative neo-Kantianism whose recognition is urged by the new canon of Vienna Circle scholarship. Second, Schlick's experiential meaning determination, an example of the traditionally ascribed project of the Circle. I now, finally, turn to Neurath. Neurath's approach to scientific philosophy differed in its methodology and its aim from both views so far considered. My first aim is to establish that Neurath opposed Carnap's Aufbau conception (and Schlick's alternative) already during stage one of the debate. My aim here, however, is not just to document that he Neurath opposed them, it is also to show how he did so. The interest held by Neurath's theorising lies, after all, not only in its philosophical context but, most importantly, in its content.

Neurath's earliest published comments on the epistemological position taken by Carnap appeared in his 1928 review of Carnap's Aufbau and his Scheinprobleme in the
socialist monthly Der Kampf. The criticisms there voiced were reaffirmed and expanded in his address to the Circle's 1929 Prague conference which (like Schlick's "Wende") was published in the first issue of Erkenntnis in 1930. The comments and criticisms here voiced constitute Neurath's position prior to the official embrace of physicalism: to be precise, they show him on the road to and arriving at the physicalist position, albeit not yet under this name. Neurath's remarks were, as usual, fairly oblique; what they aimed at becomes clear, however, once their dialectical context is explicated. As in the case of Schlick, I begin by noting the agreement with Carnap before I turn to specify the disagreement.

5.1 Points of Agreement between Neurath and Carnap

Neurath's review supported Carnap's abstraction from all experiential qualities.

[Carnap] undertakes to characterise sense impressions on the basis of certain structures, structures in which "red", "hard", "loud", "Cis" etc. do not appear, but only facts which can be captured by mathematical-logical means—and that suffices! Carnap consciously turns away from taking empathy in any form, or personal attitudes, as his starting point. He only knows that kind of insight which can be grasped by every human being! Order is the most common, the most universal [property] which we experience in things.¹

Similarly in his 1929 address, Neurath lauded Carnap's theory of constitution as an attempt to provide "the proof of the thesis of unified science".² What impressed Neurath in the Aufbau was Carnap's abstraction from all subjective elements of experience, his thoroughgoing structuralisation of knowledge by logico-mathematical means. It expressed the spirit of modern science: "this-worldly meaning, the call for empirical control and the systematic use of logic and mathematics."³ What Neurath agreed with in Carnap's

¹Neurath 1928b:296.
²Neurath 1930a:G385, translation amended.
³Ibid.:G378.
enterprise were his concern to exhibit the unity of science and his general structuralist methodology.

While warmly recommending that Marxist researchers and students study the works reviewed and the "empirical rationalism" of the "Vienna school around Moritz Schlick" in general and render it "useful for Marxism"⁴, Neurath nevertheless used the occasion of his review for some critical remarks. In a nutshell, his point was that the justification of scientific knowledge should not, indeed could not, proceed by means of a heavily idealised reconstruction which neglected the distinctive characteristics of what he deemed the real object of concern, historically developed theories. Neurath's agreement with Carnap then was broadly similar to Schlick's agreement with Carnap: both applauded the method of structuralisation in principle. Neurath's criticism of Carnap's method in practice, however, focussed on different aspects than Schlick's. While both objected that the actuality of scientific knowledge was not thereby demonstrated, their reasons for thinking so were different. Whereas Schlick criticised the lack of experiential foundations, Neurath criticised Carnap for still being too foundationalist.

5.2 Neurath's 1928 Criticisms: Against Completist Idealisations

Neurath's criticisms of Carnap's Aufbau are contained in the following passage in his review. I first quote this highly dense passage in full and then turn to the lengthy explication it requires.

In his desire to execute the logical construction of the world [Carnap] also grapples with the "ideal language" and aims to show how one has to proceed with "complete insight". Starting from a presupposition of having complete insight, he tends to view our current situation as a kind of precursor state. Perhaps this attitude stems from the fact that, basing himself on physics and the natural sciences in general, he does not analyse the social sciences in any detail. Otherwise he would have produced more

⁴Neurath 1928b:296/7.
careful formulations, and would have considered in particular the question of how one is to promote knowledge while one still has to use "clean" and "unclean" ways of thinking without being able to keep them apart - something which perhaps will always be necessary. The cleanliness of logical order grants definiteness! That is true! But how is one to overcome the ambiguity which in other areas, for example in the social sciences, confronts us already in our selection? In reality, the time- and class-bound ideology takes the place of logical definiteness! Collectivism gives a backing to the individual which cannot, and probably never will, flow from the subject matter itself.5

Clearly, Neurath held that Carnap's epistemological sketch did not paint a realistic picture of scientific knowledge. Just why he thought so is, of course, another matter; indeed, it might even appear that the only trouble with Carnap's sketch was that it did not encompass the social sciences in their actual state. To show that Neurath's charge was wider than that, and what his reasoning was, I begin the explication by focussing on two of his charges in particular, the reliance upon "complete insight" and the hankering after an "ideal language"; thereafter we shall be in position to properly interpret his reference to "clean" and "unclean" ways of thinking.

5.2.1 Against the Assumption of "Complete Insight" and the Idea of an "Ideal Language"

There is first the charge that Carnap relied upon "complete insight". Was this charge justified? In the previous chapter I showed that Carnap's rational reconstruction did not, after all, provide unconditional certainty. Much to Schlick's chagrin, his meaning determinations remained conditional on the assumption that present science conceptualised its subject matter correctly. What then did Neurath have in mind? He focussed upon two aspects of Carnap's methodology which allowed him to disregard the vagaries of actual, historically developed theories.6 The first is this. Carnap proceeded on the assumption

5Ibid.:296.
6This general point that the vagaries of actual theories should not be dismissed as unimportant was not a new one for Neurath. Already in the mid-1910's he stressed that the development of new scientific
that the reconstructive theorist has available a list of the extension of the basic predicate of remembered similarity of a given epistemic subject, and that, corresponding to this assumption, this subject possessed an infinite memory. This was clearly an idealisation of completeness. Secondly, Carnap had to presuppose that the reconstructive theorist had knowledge of what the world was like in order to construct the definitional framework with the help of which knowledge claims were evaluated against the basis of a subject's actual experience. I argued above that this presumption amounted to having a list of the extension of the basic relation which specified all the remembrances of similarity any individual could possibly have in his life. Only on this kind of data base was it possible to give the logical form of every empirical concept. This was clearly another idealisation of completeness. Both idealisations, furthermore, were "constructional fictions", as Carnap frankly called them, they were not mere extensions of characteristics of existing states of affairs. Yet Carnap's project depended upon it: only these fictions made his starting point, the given of an individual's consciousness, amenable to his structural treatment. For Neurath, however, resting one's epistemology on such an assumption was a most dubious procedure: thus his misgiving against Carnap's presupposition of "complete insight".

These idealisations indicated that Carnap worked with the idea of an "ideal language". By this term, Neurath meant to capture the following idea. An ideal language was fully cleansed of the ambiguities of natural languages. The important point of Neurath's disavowal of this idea was the extent to which clarity was presumed to be achievable; what he criticised was not the idea that natural languages stood in need of clarification. Like Carnap, Neurath recognised the need and promoted efforts to clear up theories often sprang from the exploration of what he called "margin of theories" and the "hazy edges of concepts" of antecedent theories. See Neurath 1915, 1916, 1921, and, for example, his 1935a for an explicit reference to these earlier works.

7See above section 4.4.1.
8Carnap 1928a:159-161.
the linguistic tangles which fostered metaphysical confusion: thus his famous "Index Verborum Prohibitorum". Neurath recognised that Carnap's constructional language represented a development of Mach's idea for a universal language of science which was to express the meaning of its expressions purely formally: "to read [it] would be to understand [it]." The clarity required for such a project, Neurath felt, was clearly of an unreachable degree, and, moreover, depended on assumptions which, as I shall show, Neurath already in 1928 was highly suspicious of, namely, phenomenalist ones.

Given this explication of the idea of an ideal language, it is clear that what, for Neurath, counted against the assumption of complete insight also counted against the assumption of an ideal language. Another root of Neurath's misgivings against this idea was that Neurath held that we cannot always separate "clean" and "unclean" ways of thinking. What did he mean here? Cleanliness was identified with the definiteness granted by logical order. This can be read as an allusion to Schlick's and Carnap's concern to overcome the unreliability which they thought affected only ostensively defined terms but not implicit definitions. Cleanliness then was a characteristic of logical and mathematical thinking. In "clean" ways of thinking no indefiniteness or vagueness was allowed. Things were exactly as they were said to be, nothing less, nothing more. Uncleanliness thus meant indefiniteness, vagueness: things might also be other than how they were characterised. Neurath's argument was that Carnap's justificatory account of knowledge was not only fictitiously complete but also fictitiously definite.

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9 For a brief remark on the long history of this "Index" see Neurath 1941a.
10 Mach 1883:481/2; Neurath noted the Machian provenance of Carnap's Aufbau in his 1945/6:232 and Carnap himself did so in his 1963a.
5.2.2 The Scope of Neurath's Criticism

The question arises how wide-ranging Neurath's criticism was. Neurath opposed Carnap's fictional definiteness to the "ambiguity which in other areas" already affects our "selection". On the surface, Neurath would seem to have severely limited the scope of his criticism of Carnap's Aufbau. I shall provide this interpretation first; then I shall show that we are well advised to interpret Neurath's criticism more widely.

Neurath cited the social sciences as an example where Carnap's aimed for clarity as not to be established. Accordingly, the Aufbau failed only in the higher reaches of its rational reconstruction of empirical knowledge. The social sciences could not be as neatly reduced to the given, as physics. In the social sciences the idea of the ideal language was inapplicable. Already our "selection" was affected by ambiguity. The next question is: "selection" of what? I believe that the selection at issue was that of what was to count as the explanans or explanandum. Neurath did not give an explicit example of this, but it can be easily furnished. Earlier in his review, he cited the enmity between traditional metaphysics "providing the needed ideology" for the political forces on the right and the "empirical rationalism" which opposed this "bourgeois philosophy". One question facing the sociologist was whether the said rise of "theologising" philosophy in Austria's academe indicated the inevitable strengthening and imminent victory of the forces of fascism or a last desperate attempt to stall the proletarian revolution. Another was whether efforts like those of the (yet to be so called) Vienna Circle indicated the inevitable strengthening and imminent victory of the forces of the proletarian revolution or a last desperate attempt to stall the rise of fascism. In answering either question, which social facts was one to invoke, and give what weight to them?
So far, so good. We must consider, however, whether Neurath's criticism was not wider ranging. How principled an objection against the Aufbau was this? Given Neurath's own criticism of the state of social science at the time\textsuperscript{11}, Carnap's rational reconstruction could not but fail to do justice to the practice of such immature science. Yet Neurath's disavowal of the idea of an ideal language does point to a deeper misgiving. I next shall try to show that a broader attack on Carnap's methodology was implicit in Neurath's criticism.

I return to the question "selection' of what?" What philosophical argument lay behind Neurath's claim that there were sciences where both the facts to be explained and the facts invoked in such an explanation had to be "selected"? Neurath, it must be noted, was long familiar with the thesis of the underdetermination of theory by evidence. As he had learned from Duhem, whose work he had studied in the "first" Vienna Circle, the refutation of theory by experimental results did not indicate which part of the theory was at fault. Speaking in strictly logical terms, a contrary experimental result did not even force the abandonment of the theory: the experimental result could be discounted as well.\textsuperscript{12} Thus both the explanans and the explanandum had to be selected by a choice of the theorist. (The need for the selection of singular facts as explananda paralleled the need in all of science—a need endlessly stressed by Neurath—to determine by decision, and not by some inductive logic, which inductive generalisations to make.)

Given this explication of "selection", the natural sciences were not exempt from this ambiguity. The first reason is that Neurath himself repeatedly—earlier and later—declared physical science to be affected by ambiguity.\textsuperscript{13} For Neurath, all of empirical science was

\textsuperscript{11}Locus classicus Neurath 1931c, but see also the later parts of 1932a.
\textsuperscript{12}This possibility is affirmed, if obliquely, already in Neurath 1913b:G59; see also Haller 1982.
\textsuperscript{13}Eg. Neurath 1916:28, 1935a.
characterised by ambiguity in the selection of the facts to be explained and the facts upon which an explanation was to rest. The second reason is that Duhem's argument which lay behind Neurath's assertion to be explained, concerned physical theory. Neurath's 1928 argument then was that Carnap's Aufbau was only fictitiously complete and fictitiously definite, not only compared to the actual state of social, but also natural science.

If this was Neurath's meaning, it may be wondered, why did Neurath not express it more clearly? One reason is that Neurath focussed upon social science because it provided an easy example close to the heart of the readership of Der Kampf who could be relied upon to concretise his hint. Another reason lies in the politics of his argument. As I shall show next, what Neurath ultimately objected was Carnap's methodological solipsism. Now consider the intended readership of his review, and his advice to render scientific philosophy "useful for Marxism": how likely would it have been that his advice was heeded, if he had accused the first major work of this movement of just that which Lenin had castigated as "subjective idealism" and deemed the tool of "bourgeois revisionism"?14 Neurath's own efforts to show Carnap's methodological solipsism an inessential vestige of "school philosophy" would have been deemed pointless, and thus seriously undercut.

5.2.3 Against Carnap's Solipsistic Base

As I have noted, Neurath did not object to Carnap's formal approach to meaning as such; what he objected to, when he objected to Carnap's idealisations, was the epistemological position they were supposed to support, the solipsistic base. With the present understanding of the scope of Neurath's criticism, namely, that all areas of science were affected by ambiguity, the final sentence of the critical passage quoted more clearly reveals its anti-solipsistic thrust.

14 For a discussion of Lenin's claims see e.g. Cohen 1963 and Stadler 1982b.
First, however, there is the question: what is the "subject matter" from which the determinations of what was a fact could never flow? For Neurath the arch-empiricist, the subject matter of science in its generality was simply experience as such: to exert "empirical control" over experience of the natural and the social world, the mastery of nature and history, was the very task of the scientific world conception. As Neurath indicated, gaining such mastery was a social enterprise: "Collectivism gives backing to the individual which cannot, and probably never will, flow from the subject matter itself."\textsuperscript{15} How then did Neurath's comments amount to a criticism of Carnap's methodological solipsism? The experience of, and thus the data available to, a single individual did not suffice to justify the decisions required of theorists for the construction of scientific theories. The choices made in the systematisation of experience in theory are not determined by the nature of the experience alone, but also by socially determined convention. This holds for all of science; in the extreme case, in social theory, "time- and class-bound ideology take the place of logical definiteness". Clearly, this view was in open conflict with the solipsistic position of the \textit{Aufbau}: its constructional language fully prescribed the theory of the world to be adopted, it did not leave open choices.

Now, if theory choice does not "flow from" the experience of a solitary individual, then it follows that any attempt to capture in theory the determination of theory choice requires greater resources than those that could be taken account of in Carnap's constructional language.\textsuperscript{16} In the \textit{Aufbau}, all socially determined choices were of "swept

\textsuperscript{15} Again there is a temptation here to read this sentence as applying only to social science. Again I must point out that the "collectivism" mentioned encompasses the nature of the process in which the decisions are reached in natural as well as in social science on how to overcome the ambiguity of what is to count as fact. As Neurath was fond of remarking, the development of unified science is not the task for a single individual but the work of whole generations.

\textsuperscript{16} According to the \textit{Aufbau}, the input of others, for example, would be the reconstructed input of reconstructed others, reconstructed from a prior reconstruction of the physical world from a solipsistic base. It could not, therefore, influence the construction of the physical world from which these others themselves are constructed. See above sect. 3.3.2 and below sect. 5.5.
under the rug", namely, comprehended in the idealising assumption that the rational reconstructor knew what must be the end result of constitution theory. Carnap was able to "demonstrate" knowledge as "flowing from" its subject matter as he perceived it, a solitary individual's experience, only by helping himself in this undercover fashion to the results of historically prior, socially determined theory choice, the pronouncements of the science of his day on which the rational reconstructor based himself. What undrlay Neurath's criticism of Carnap's Aufbau was thus ultimately Carnap's philosophical assumption which led to his adoption of the position of methodological solipsism: the Cartesian idea that the epistemic subject was a solitary individual.

5.2.4 The Challenge to the Relevance of Rational Reconstruction

How conclusive was Neurath's criticism? To Carnap, it doubtlessly appeared that Neurath was barking up the wrong tree altogether. Carnap was, after all, only giving a rational reconstruction, a sketch of how empirical knowledge could be in principle demonstrated to be justified; his aim was neither to show how it was in fact to be arrived at nor how it was in fact justified. It is hard to see how Neurath could have responded to this on the basis of the criticism made in his review. But I suspect that Neurath's objections to the Aufbau were not ones that occurred to him only after reading the published version. As my explication of Neurath's criticism indicated, Carnap's particular reconstructive method stood in conflict with long-held convictions of his own. I suspect, then, that already during the pre-publication stages of the Aufbau, perhaps already after Carnap's presentation of its drafts to the Schlick's circle, Neurath voiced these or similar criticisms. They were clearly to no avail: Carnap presumably defended his procedure in the way just indicated, and published the Aufbau. Neurath's misgivings were not assuaged by Carnap's reply either, however, as his review demonstrates.
My strong suspicion is that Neurath's review was but the public manifestation of a private debate that preceded it. (Thus note Neurath's reaction to Neider's anti-Aufbau argument explicated below: "Of course, Finally!"17) The criticism of the Aufbau voiced in Neurath's review ultimately amounted to the following challenge: show that rational reconstruction still bears any relevance to the justification of the scientific knowledge we do possess! This challenge expressed that the extent to which Carnap's reconstruction relied on "completest" assumptions left it an open question whether it still possessed any relation to the knowledge actually argued for and gained in scientific practice. The choices of theory construction did not "flow from" its subject matter. Merely pointing, like Carnap, to the difference between the contexts of discovery and justification did not do the trick! The pursuit of questions belonging to these different contexts requires that different aspects of knowledge be focussed upon; but these different kinds of questions still concern the same subject matter, knowledge. Short of losing sight of the subject matter altogether these aspects cannot conceived of as completely isolated from each other. It follows that the connection between rational reconstruction and actual scientific practice stands in need of illumination, on pain of the accounts of discovery and justification being objectionably disjoint.

What evidence is there of Neurath having issued this challenge to Carnap explicitly? Admittedly none other than the circumstantial one that Carnap can be read as attempting to respond to something like this challenge in his Scheinprobleme der Philosophie. (I show in the Appendix to this chapter how Carnap can be interpreted to do so, and how he failed.) Whether Neurath did issue this challenge explicitly or not, however, is of minor importance. The criticism voiced in Neurath's review does give rise to it. Since Carnap's Scheinprobleme did not persuade Neurath or advance the protocol sentence debate, I shall

17Neider 1977. See below sect. 5.5.2 and 5.5.4.
instead proceed to the next installment of Neurath's criticism of Carnap, contained in his 1929 address at the Circle's Prague conference.

5.3 Neurath's Prague Address: Against Aprioricism in Epistemology

The second of Neurath's earliest published criticisms of the Aufbau, his "Wege der wissenschaftlichen Weltauffassung" ("Ways of the Scientific World Conception"). Given the year following his review of the Aufbau, Neurath repeated his criticism of Carnap's methodologically solipsist execution of the epistemological project yet more forcefully, though still, by normal standards, obliquely.

5.3.1 Pointed Restatement of the "Ideal Language" Criticism

Without mentioning Carnap, Neurath's first critical passage recapitulated his earlier criticisms of the Aufbau as illegitimately relying on "complete insight" and falsely searching for an "ideal", presumably "clean", language.

Theological residues in science can be suspected wherever empirical statements are related to a postulated or fictitious 'complete' insight - either with or without an 'as if' expression. The determinism of Laplace's formulation is untenable, for the assumption of an unlimited cross-section of the world is totally meaningless. Perhaps there are theological residues also in the search for the ideal language and in certain applications of the concept of infinity in mathematics. [...] Of a different nature are discussions of structures of formulas where the application to empirical events is not at issue. Among these structures are also those of statements about the empirical.¹⁸

Given my explication of his 1928 review I need not spell out again the point carried by the key words "complete insight" or "ideal language".

¹⁸Neurath 1930a:42/3; translation amended and italics restored. (The two sentences dropped from the quotation endorsed the Circle's general finitist attitude—cf. Carnap 1963a:55—as a part of the conceptual "tidying up". The paragraph concluded with the affirmation of the "possibility that set theory can be faultlessly built up in the end".)
But while Neurath did not mention Carnap here, I must note that he was implicitly referred to when Neurath rejected reliance upon complete insight "either with or without an 'as if' expression". This addendum had two functions. First, to declare his dissent from the then popular philosophy of "as-if", propounded by Vaihinger and his followers in their own books and the journal Annalen der Philosophie und philosophischen Kritik (of which, within a year, Carnap and Reichenbach were to assume editorship as Erkenntnis). Ever-vigilant against metaphysical infiltration, Neurath wanted to leave no room for possible misunderstanding by the philosophical and scientific public at large. (The Circle's Prague conference was intentionally held to coincide in time and place with a major conference of German physicists.) Secondly, Neurath here referred to Carnap himself implicitly. Some of Carnap's early work bore close affinities with Vaihinger's "as-if" philosophy and was published in the Annalen.\textsuperscript{19} The Aufbau itself, furthermore, was a bit of an "as-if" undertaking without the expression: the concept of reality itself was constituted by a solitary consciousness.

In the last two sentences quoted, Neurath also endorsed, as already in his review, Carnap's general structural methodology. But even more specifically this time around—and hinting therefore at the fact that the matter had been considered by him and had perhaps even been discussed in the Circle—Neurath declared sentences about sentences legitimate. Such sentences concerned the structure of other sentences, their logical form. Here Neurath sided with Carnap and placed himself in explicit opposition to those members of the Circle who, like Schlick, followed Wittgenstein in thinking logical form unrepresentable.\textsuperscript{20}

\textsuperscript{19}Eg. Carnap 1924. Here's its opening sentence: "From Hume's test of the concept of causality up to the philosophy of the as-if of Vaihinger the insight has won in clarity that causality, if conceived of as a relation of cause and effect, constitutes a fiction at the basis of which lies the experienced relation of an active will to its deed."

\textsuperscript{20}See above section 4.4.
5.3.2 Against the A Priori in Epistemology

The second passage of Neurath's Prague address which was critical of Carnap's Aufbau went as follows.

The ambiguity of approach—which resulting from lack of knowledge or from the nature of the subject—can be overcome practically, in the last resort, only be the unambiguity of action, that is, of decision (for example, experimentally to use a certain statistical law as a basis). This unambiguity of decision lies outside the sphere of scientific argumentation. No logical reason can be given if one decides in favour of a certain conception or gives preference to a certain measure of possibilities.\(^\text{21}\)

This point too is familiar from my explication of the criticism of Neurath's 1928 review of the Aufbau. (Note that on this occasion Neurath did not even hint at a difference between the natural and social sciences in this regard.) Again, Neurath focussed clearly upon scientific practice. Neurath's point was that it was an impossibility to eliminate the multiplicity of theoretical possibilities within science: only a practical decision could establish "definiteness".

Against this point it could be objected that it did not tell against Carnap. Carnap had been explicit about what he "swept under the rug". It might be held that Carnap did take account of the conventional determination of theory choice.

The first aim [of unified science], then is the construction of objects; it is followed by a second aim, namely, the investigation of the non-constructional properties and relations of the objects. The first aim is reached through convention; the second, however, through experience.\(^\text{22}\)

What were these conventions but decisions, and had Carnap not taken account of them—admittedly not in the construction theory itself but in the theory of the construction theory which specified what the constructor had to know to succeed in his project?

\(^{21}\)Neurath 1930a:45.
\(^{22}\)Carnap 1928a:289.
In the face of this objection it is important that the second passage critical of the Aufbau in Neurath's address continued as follows.

It is of decisive importance for the scientific world-conception to become aware of the narrowness and limitation of knowledge in this way because otherwise there would be the danger that one creates a new idol by the postulate of complete definiteness, one that would take the place of the old a priori, or the infinite and the divinity. Where formerly the priest or the philosopher stood, the professor would stand. We must refrain from such hasty postulates. What we state about things must be said with caution: we find chaos and order, and we have to find out empirically which measure of order we can establish in our concept formation in view of the relationships involved.23

At this point, Neurath went beyond the criticisms already voiced in his 1928 review. In a nutshell, Neurath's point was that the conventions by which the form of the language of science was to be determined were to be arrived at on the basis of empirical, not a priori, considerations.

Now, beyond being merely a rational reconstruction of empirical knowledge, Carnap's constitution system also took recourse to empirical science for information concerning how its reductions should go: which were the epistemologically dispensable definitions?24 Carnap's constitutional conventions then do not seem to be all that a priori but decisely empirical in nature. Did Neurath simply misunderstand Carnap's procedure? I believe he did not. What Neurath focussed upon was Carnap's undefended assumption25 (which his psychological theory did little to defend26). That was also the point which Neurath's review of the Aufbau had already focussed on in his review: Carnap's choice of a solipsistic starting point.

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23 Neurath 1930a:45/6, his italics.
24 See above sect. 4.3.
25 See above sect. 3.2.1 and 3.4.2.
26 See Appendix to this chapter.
What was new here in Neurath's argument was the explicit juxtaposition of a priori and empirical reasoning. What Neurath stressed was that whatever was at issue had to be found out empirically. What was at issue here was "which measure of order we can establish in our concept formation". This meant, among other things, Carnap's presumption of the epistemic priority of the phenomenal given. Accordingly then, Neurath here argued that considerations of epistemic priority themselves had to answer to empirical considerations! Whether a rational reconstruction of empirical knowledge along the lines of methodological solipsism was legitimate was, for Neurath, an empirical issue. Clearly, this was a radical thought: epistemology itself had to answer to empirical considerations! To call for this amounted to nothing less than the abandonment of epistemology as a foundational discipline. It amounted to this abandonment both in the sense of epistemology as a discipline conceptually prior to science, and as a discipline providing the traditionally expected foundations for our knowledge claims. In the next two sections I shall discuss both points in turn.

5.3.3 Against Foundationalism

With his abandonment of epistemological foundations, Neurath clearly clashed with Schlick already in 1929/30. As I showed in the previous chapter, Schlick clearly aspired to epistemological foundations in his "Wende der Philosophie". This radical disagreement was amplified in the continuation of the passage last quoted. Neurath not only clarified this, but also specified another related, though yet different, disagreement with Schlick. Neurath's second critical passage continued as follows:

Our thinking is a tool, it depends on social and historical conditions. One should never forget this. We cannot act as prosecutor and defendant at the same time and in addition sit on the judge's bench. We confront our present thinking with earlier thinking, but we have no possibility to judge both from
a point outside. Checking statements with the events is itself part of the method adopted.27

Here Neurath did not only summarily dismiss foundationalism—in terms which closely resemble those accompanying his first two uses of the Boat28—but also presented his reasons for doing so.

Neurath rejected the correspondence theory of truth. His reason was this: the correspondence theory of truth presupposed what was impossible, namely the adoption of a still point "outside" from which what goes on "inside" becomes meaningful. What here is "outside" and "inside"? The correspondence theory of truth attacked here was Schlick's as filtered through Wittgenstein's Tractatus.29 Truth consisted in the univocal correspondence of statement and fact; what made for this correspondence was that the logical form was shared between fact and statement. Neurath objected to this view of truth and meaning. Carnap once reported that "Neurath emphasized from the beginning that language phenomena are events within the world, not something that refers to the world from outside."30 (In so far then as Neurath agreed with the structural approach to meaning he held, with Carnap, that logical form was the constitutive, not the the representational medium of meaning.31) Since meaning was a "this-worldly phenomenon"32, no transcendental, trans-empirical preconditions of meaningfulness needed to be invoked. Just that, of course, was what the doctrine of representation as the sharing of logical form between sign and signified did. As he noted, we cannot step outside of thought to see whether our thoughts or our statements correspond to reality. We can only compare

27Neurath 1930a:46; translation amended. The original of the last sentence quoted runs: "Erprobung der Aussagen an den Abläufen ist selbst Teil der eigenen Methode."
28Compare Neurath 1913a, 1913c., 1921a.
29See above sections 4.1 and 3.3.1.
30Carnap 1963a:29, his italics.
31See above section 4.3.2. Note the qualification: just how logical form "constituted meaning is left open. By 1931, Neurath had adopted a conception of meaning different from Carnap's. See below sect. 6.4.2.
32Neurath 1930a:[G385].
different thoughts or statements, our earlier ones with later ones (and, presumably our own with those of others).

Neurath did not think of this as a turn to idealism, as is indicated by the last sentence of the quote above. Neurath did not mean to throw into doubt that the statements of science remain under empirical control; what he meant was that any verificationist criterion of meaningfulness did not justify the idea that we do have a court of appeal where we can decide, unencumbered by the limitations of thought, whether thought correctly represents reality. In as much as meanings were at issue, they had already been taken account of in the process leading to the acceptance or rejection of a statement, for how could statements have been checked, if what they say has not been determined already? Beyond the clarification of scientific concepts that constituted part of the scientific activity itself, there was no philosophical gold to be mined here. Neurath's opposition to Schlick's epistemological method, namely, that of extracting meaning determinations of a particularly philosophically relevant kind from processes of verification, followed from his opposition to Schlick's correspondence theory of truth and foundationalism.

5.3.4 Neurath's Naturalism

Neurath recommended that theorists of knowledge turn their attention to the empirical conditions of thought and knowledge. In the passage at hand, Neurath clearly spelled out his own guiding conception: "[o]ur thinking is a tool". But Neurath did not adopt a bland biologist notion of thinking as a tool of survival here. To be sure, given his Machian background and his early studies with Simmel, Neurath can be assumed to have accepted a broadly evolutionary conception of human cognitive capacities. What

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33See Neurath 1945/6:230; for Mach's position see particularly his 1910.
34See the c.v. in Neurath 1906; for Simmel's position see his 1895.
Neurath added to this, however, was the recognition of the socio-historical dimension of thought and knowledge: our thinking "depends on social and historical conditions".35

What emerges here, finally, is the difference in the philosophical ends sought by the main players in the Vienna Circle's protocol sentence debate. This difference in ends corresponds, as I urged in my introductory chapter, to the different readings of Vienna Circle philosophy on offer. Schlick, as we saw in chapter 4, primarily sought to establish the actual justification of scientific knowledge, he was concerned with the fact and extent of knowledge. Carnap, so we saw in chapter 3, primarily sought to establish wherein the objectivity of knowledge consisted, he was concerned with the very possibility of knowledge. Neurath, in contrast to both Schlick and Carnap, we now can see, was concerned primarily to establish the empirical conditions of scientific thought so as to better further its chances to transform the world. Now clearly these different philosophical ends do not stand in necessary oppositions; indeed, a satisfactory answer to any one of these concerns would require answers to the others as well. Yet these different emphases lead Schlick, Carnap and Neurath to follow different methodological paths. It was over these differences of methodologies that open conflicts arose between them, conflicts which reveal their in fact conflicting conceptions of philosophy itself.

Instead of turning to experiential meaning determinations like Schlick, or to unearthing a priori conditions of objective knowledge like Carnap, Neurath recommended that theorists of knowledge turn their attention to the empirical conditions of thought and knowledge. Neurath opposed not only Schlick's, but also Carnap's conception of scientific philosophy. To see this, recall Neurath's warning not to "create a new idol by the

35This too was not a new view of Neurath's here: some 15 years earlier he had already explored the social determinants of scientific development and theory change. On this in particular see his 1915 and 1916, on the general "facticity" of thought see again his 1913a, 1913b and 1921.
postulate of complete definiteness, one that would take the place of the old a priori..."36 Neurath's juxtaposition of empirical and a priori reasoning in epistemology against Carnap gains special importance in the light of the new reading of Carnap as an alternative neo-Kantian. Between the lines of the passage last quoted stands the thought that the last thing Neurath wanted to see the old a priori replaced by was a new a priori. That, of course, was just what Carnap, according to the new canonical reading, was doing: replace the synthetic a priori as the condition of the objectivity of knowledge with a purely analytic a priori, namely the rules of (at this time still37) the language. The passage at hand thus suggests that Neurath was aware of and objected to Carnap's alternative neo-Kantianism. Neurath's proposal to "empiricise" epistemology stood thus in conscious contrast to both Schlick's and Carnap's epistemological projects.

Does Neurath's claim that "we have to find out empirically which measure of order we can establish in our concept formation in view of the relationships involved"38 amount to a proposal to naturalise epistemology? Take as the distinctive characteristic of naturalised epistemology the methodological decision to accept as legitimate epistemological notions only those which can in turn be explicated by concepts which science uses to describe and explain processes antecedently understood as natural. In this sense, I submit, Neurath's proposal does amount to the naturalisation of epistemology. The order we can impose on the pronouncements of science as a measure of their epistemological legitimation could, so Neurath, itself be arrived at only empirically.

36 Neurath 1930a: 45.
37 See Menger 1979b and 1982.
38 Neurath 1930a: 46.
5.3.5 Neurath's Materialism

I now turn to the third passage critical of Carnap in Neurath's 1929 address. With reference to it, we can lay to rest any lingering doubts whether Neurath indeed challenged Carnap's methodological solipsism, as I have urged so far. Neurath mentioned amongst the tasks of unified science the establishment of

the theory of constitution (Carnap) as a proof of the thesis of unified science which must be concretely built up and continuously enriched in order to be useful.39

Obviously, Neurath did not think of Carnap's attempt at a constitution theory as definitive. In particular, his use of the adverb "concretely" (a stock phrase in Marxist discussions of the need to tie theorising to actual practice) indicates the nature of his scepticism. What was not "concrete" enough in the Aufbau? Two sentences later Neurath spoke of "concrete experience" as that on which scientific generalisations were based. For Neurath then, Carnap's Aufbau was not based on actual experience: the experiential base of science was here misconceptualised. Given his agreement with Carnap's structural methodology, this can only mean dissent from Carnap's methodological solipsism. Neurath's misgivings against his employment of an ideal language and his presupposition of complete insight found their true object in Carnap's solipsistic base: it represented an illegitimate abstraction away from the concrete conditions of thought. Neurath thus disputed the position of methodological solipsism.

Still in 1928, Neurath had endorsed "confessions of materialism" explicitly only as expressions of a proper anti-bourgeois attitude and placed its acceptance by "exact thinkers" into the future.40 In his 1929 address, however, he clearly and repeatedly affirmed

39Ibid.:47; translation amended; his italics.
40Neurath 1928a:[G284].
materialism: he spoke of "scientists with a materialist basis", of "sociology on a materialist basis", of "materialistic empiricism" and of "the scientific world conception on a materialistic basis".\(^1\) (It should be noted that, for Neurath, the difference between materialism and physicalism was but a difference in name.\(^2\)) By 1929 then, Neurath thought the primacy of the materialistic base established and methodological solipsism refuted. Given what he said earlier, it is clear that Neurath held such issues decidable by empirical considerations. This suggests that Neurath thought he had empirical reasons for disputing Carnap's position. What were they?

5.4 The Problem of Intersubjectivity

What emboldened Neurath's open embrace of materialism in 1929 was an argument to the effect that Carnap's methodological solipsism failed to come to terms with the intersubjective nature of scientific knowledge. To show how such an argument could apply to the **Aufbau**, I must first note a subtle change of Carnap's position which was effected by 1930. Then I turn to the argument in question which lay behind this change, and which confirms the thrust of the criticism of the **Aufbau** I attributed to Neurath.

5.4.1 Carnap's Change of Position between 1928 and 1930

To see the subtle change in Carnap's position between 1928 and 1930, we must first note in greater detail than so far the position concerning intersubjectivity taken in the **Aufbau**. It might be held that Neurath's open embrace of the materialistic basis for unified science was wholly compatible with Carnap's **Aufbau** which had explicitly recognised the plausibility of a "constructional system with a materialistic basis" and affirmed that "science

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\(^1\)Neurath 1930a:G382, G384. See also his endorsement of "unified science on a materialistic basis" in 1930b:355 and 1931e:75, and yet another endorsement of "sociology on a materialistic basis" in 1930c:361.

\(^2\)E.g.g Neurath 1931d:411, 1931c.
needs both an experiential and a materialistic derivation of all concepts." Apart from the attack on methodological solipsism then, no disagreement obtained between Neurath and Carnap. Such a view, however, overlooks an important change in Carnap's reasons for requiring the materialistic derivability of scientific concepts. In the Aufbau, Carnap held the following position.

A materialistic constructional system has the advantage that it uses as its basic domain the only domain (namely, the physical) which is characterised by a clear regularity of its processes. In this system form, psychological and cultural events become dependent upon physical objects because of the way they are constructed. Thus they are placed within the one law-governed total process. Since the task of empirical science (natural science, psychology, cultural science) consists, on the one hand, in the discovery of general laws, and, on the other hand, in the explanation of individual events through their subsumption under general laws it follows that from the standpoint of empirical science the constructional system with [a] physical basis constitutes a more appropriate arrangement of concepts than the other.\textsuperscript{44}

In the Aufbau then, Carnap's reason for requiring, alongside the phenomenalistic derivation of concepts, a materialistic one was that science in fact used a materialistic system of concepts. A separate materialistic derivation was required for science because only the physical domain was held to be law-governed.\textsuperscript{45} What Carnap, in the Aufbau, did not give as a reason for also requiring a materialistic derivation of scientific concepts was the need for intersubjective intelligibility of scientific statements.

By 1930, however, Carnap stated in a lecture of the Verein Ernst Mach that for "scientific statements to be \textit{intersubjectively transferrable}, the basic concepts must be taken to be physicalistic ones."\textsuperscript{46} Similarly, in his first contribution to \textit{Erkenntnis} he noted:

The positivist and the materialist constitution system do not contradict one another. Both are correct and indispensable. \textsuperscript{47}... positivist system

\textsuperscript{43}Carnap 1928a:95/6.
\textsuperscript{44}Ibid.:95.
\textsuperscript{45}Schlick 1918/25.
\textsuperscript{46}Carnap 1930a:77.
corresponds to the epistemological viewpoint because it proves the validity of knowledge by reduction to the given. The materialist system corresponds to the viewpoint of the empirical sciences, for in this system all concepts are reduced to the physical, to the only domain which exhibits the complete rule of law and makes intersubjective knowledge possible.\footnote{Carnap 1930a:144, my italics.}

In the \textit{Aufbau}, Carnap claimed to have accounted for intersubjectivity within construction theory, yet here Carnap admitted that the positivistic system did not make intersubjective knowledge possible! This argumentative shift indicates that a subtle but important development had taken place.

\subsection*{5.4.2 Neider's Argument Reported}

May this change have been brought about by an argument of the sort Neurath talked about, namely, an empirical one? In 1929, Neurath also introduced the notion of intersubjectivity into his reasoning about the basis of unified science: "all concepts are formed in the same fashion, in which on the basis of certain rules of control everything that is asserted is reduced to singular experience, which everybody can test."\footnote{Neurath 1929:347, my italics.} A somewhat obscure formulation to be sure, yet clearly the thought here was that all concepts had to observe the requirement of intersubjective intelligibility. Just this intersubjectivity of course was what had become, by 1930, Carnap's additional reason for developing a materialistic constitution system. Still, what was the (empirical) argument to this effect?

For this argument we have to turn to a report of Heinrich Neider, then still a student member of the Circle:

I said to Carnap: "You will have to drop the auto-psychological basis, because sentences concerning the auto-psychological domain are not intersubjectively verifiable, and sentences which are not intersubjectively verifiable cannot belong to science. Philosophy can consider them in explanations of how these sentences came to be formed and once they are formulated then they must be intersubjectively verifiable." We talked for a
long time and then Carnap asked me: "That is indeed a correct observation [...]" [...] Neurath was delighted. Neurath had not been with us then, but the following evening we were at his house and told him about it. He said: "Of course, finally!", for with this a bridge was built towards materialism, which he valued so much as the philosophy of the workers' movement.\footnote{Neider 1977:29/30.}

Neider did not elaborate further the point he pressed against Carnap. His report requires explication. Science required intersubjectivity, on this all agreed. Neider argued that statements in the constructional language could not be intersubjectively verified. Neider was therefore concerned with scientific practice: its constraints on theory building were not captured by Carnap's reconstruction. But did Carnap not already provide for intersubjectivity in the \textit{Aufbau}?

5.4.3 Intersubjectivity According to the \textit{Aufbau}

In order to reconstruct Neider's argument, we must consider in greater detail than so far how intersubjectivity was treated in the \textit{Aufbau}. As I briefly noted above\footnote{See section 3.3.2.}, the \textit{Aufbau} treated of intersubjectivity in two different ways.

In section 66, entitled "The Autopsychological Basis and the Problem of Objectivity", Carnap wrote:

Our problem now is how science can arrive at intersubjectively valid assertions if all its objects are to be constructed from the standpoint of the individual subject, that is, if in the final analysis all statements of science have as their object only relations between "my" experiences? [...] The solution to this problem lies in the fact that, even though the material of the individual streams of experience is [...] altogether incomparable [...] all streams of experience agree in respect of certain structural properties. Now, if science is to be objective, then it must restrict itself to statements about such structural properties, and as we have seen earlier, it can restrict itself to statements about structures, since all objects of knowledge are not content, but form, and since they can be represented as structural entities.\footnote{Carnap 1928a:106/7, translation amended, his italics.}
The point here is that certain structural properties of the given, the analytical framework explicated in constitution theory, were the same for all individual subjects. Unlike the material of the stream of experience, these structural properties were (at least in principle) comparable, they "agree[d]", and thus made objectivity possible. Objectivity as a "validity which also holds for other subjects" was achieved only because the validity of one subject's knowledge claims held also for another because, were he in the same spatio-temporal position and thus received the same stimulation, the reconstructive justification of the claim would be of exactly the same type. The objectivity of knowledge was made possible by, and consisted in the fact that, the analytical theorems of the constructional language, in terms of which different givens were explicated, were identical in both cases.

So much for Carnap's "general remarks" on "achieving objectivity in the sense of intersubjectivity"; when he turned to "demonstrat[ing] the precise method"\textsuperscript{52} for doing so quite a different picture emerges. In sections 146-149, Carnap attempted to reconstruct intersubjectivity within the constructional language, that is, from the solipsistic standpoint. Here's his result.

From the indicated way of constructing the "world of M" [the world of another], it follows that, between this world and "my world", there exists a certain analogy; more precisely, the analogy holds between the constructional system as a whole (S) and the "constructional system of M" (Sm). It must be remembered, however, that Sm is only a partial system within S; the world of M is constructed within my world; it is not to be considered as formed by M, but as formed by me for M. The analogy between S and Sm amounts to a very far-reaching, but not to a complete, agreement. To begin with, for almost every construction in S, there is a corresponding construction in Sm, which has an analogous definitional form and whose symbol is marked by an index M. Furthermore, corresponding assertions hold almost without exception for correspondingly constructed objects. This holds especially for the levels prior to the construction of the space-time world. Later on, however, in the construction of the physical and heteropsychological domain this simple agreement, which depends on analogous construction, no longer holds; on the other hand, a new type of agreement occurs. [...] A one-to-one correspondence holds between the

\textsuperscript{52}Ibid.
spatiotemporal world of physics in $S$ and that in $S_m$, 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 $S$. [...] call this correspondence _intersubjective correspondence_. [...] Two intersubjectively corresponding objects of $S$ and $S_m$ represent (in realistic language) the "same" object, once as it is recognised by me and the other time as it is (so far as I know) recognised by $M$.\(^{53}\)

On the basis of the intersubjective correspondences obtaining for the physical world, namely, the one-to-one correspondences between the objects of myself and the other as I know him, further correspondences between psychological objects can be constructed. Finally, just as an intersubjective correspondence with one other can be constructed, so it can be with yet others: such multiple intersubjective correspondences make up the "intersubjective world".

5.4.4 Neider's Argument Explicated

Against this background we can now evaluate Neider's argument as reported. Impressive as Carnap's ingenious reconstruction is, it has one fatal flaw: the objectivity qua intersubjectivity invoked in section 66 is a different one from that which Carnap reconstructed from the solipsistic base in sections 146-9. In the former, intersubjectivity arose from the fact that all givens could be explicated by the same definitions. In the latter, intersubjectivity was explicated as the agreement of a subject's own determination of spatiotemporal relations of the world around him with his reconstructions of other's determinations of the spatiotemporal relations of objects in the world.

There is a considerable difference between these two pictures of the grounds of intersubjectivity. In the former conception the intersubjective validity of knowledge claims followed from the identity of the representational framework within which all subjects' claims had to be evaluated; this framework which was explicated by the analytical theorems

\(^{53}\)Ibid.:224/5, his italics.
of the constructional language provided the a priori presupposition of meaning and reason. In the latter conception—which Carnap advertised as the detailed working out of the former-intersubjective validity was reconstructed as a subject's ability to map his or her own reconstructions of other's perceptions of the world onto his or her own. Clearly, there is a world of difference between these two conceptions: whereas the former assumes the existence of others, the latter only assumes a solipsistic base.

Now, where in all this did Neider's argument fit in? According to Carnap's Aufbau conception, a materialistic derivation of scientific concepts would be more convenient and practical, if one only wanted to reconstruct the concepts of science as belonging to one domain, the physical. His own concern, however, was an epistemological one which, he assumed, needed to reduce all concepts to the given. Yet Carnap thought that there was nothing relevant to science that was lost thereby: the solipsistic language accounted for science just as well. It was here that Neider's argument engaged Carnap. That within a solipsistic world agreement with reconstructed others could be established did not show the sentences of the solipsistic language to be intersubjectively testable. If that could be shown at all, it could be shown only along the lines of Carnap's first explication of intersubjectivity as a commonality of a priori frameworks of cognition, and that strategy required a non-solipsistic language in which to talk of different givens in a realistic mode. Only this first explication of intersubjectivity then was able to underwrite the intersubjective practice of science, but that required a non-solipsistic language. To underwrite the practice of science then a non-solipsistic language was required. That, of course, was just the thrust of Neider's argument and the then new position of Carnap's in 1930.

Neurath's delight with Neider's argument should be readily understandable now: it established that the adoption of Carnap's methodological solipsism was radically discontinuous with the practice of science, and therefore not a defensible idealisation of its
practical conditions. (It also made sense of the inadequacy of Carnap's attempt (if such it was) to defend the relevance of his methodologically solipsist reconstruction to scientific practice by his psychological sketch of the given.)\(^{54}\) Carnap's attempt to reflect the order of epistemic priority by reconstructing empirical knowledge in the phenomenal language inevitably exacted the price of failing to underwrite the intersubjectivity of scientific practice: only a non-solipsistic language could do that. With Neider's argument, the primacy of the materialistic derivation was established for an account of scientific knowledge which was by definition intersubjective knowledge.

5.5 Summary: The End of Stage One and the Beginning of Physicalism

To conclude. Already during stage one of the protocol sentence debate, Neurath neither endorsed Schlick's method of experiential meaning determination nor Carnap's method of rationally reconstructing scientific knowledge from the position of methodological solipsism. Furthermore, Neurath also declined to answer the sceptical challenge to science as it might be expected to be answered (if it was answered at all), namely, by exhibiting the foundations upon which science rested. According to his famous metaphor of the sailors repairing their boat on the high seas--a metaphor Neurath first used in 1913--there simply were no such foundations. Accordingly the task of the philosopher was not that of convincing the sceptic of the existence of what he doubted, namely, secure foundations, but rather that of showing the sceptic's conception of knowledge to be mistaken: what we call "knowledge" does not possess indubitable foundations. Quite typically, Neurath rarely said so explicitly: his sporadic polemics against "pseudorationalism" from 1913 onwards, however, have just this as their point.\(^{55}\) Now it could conceivably be held that Neurath temporarily abandoned this long held perspective in the

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\(^{54}\)See appendix to this chapter.

\(^{55}\)Neurath 1913a, 1913b, 1921, 1935a, 1935b.
exuberance of the early days of the Vienna Circle, only to return to it with a vengeance fed by disappointment. Such a view, supported by reference to the compromise-ridden collaboratively produced pamphlet of 1929, would be mistaken. As I have shown in this chapter, Neurath’s challenge to foundationalism is clearly documented by his challenge to Carnap’s Aufbau already during stage one of the debate. I had less to say in this chapter about Neurath’s disagreement with Schlick’s conception. That it did obtain, however, is clear not only from some of Neurath’s oblique remarks, but also from Neurath’s criticism of Carnap’s conception: it is hard to see how he could at the same time agree with Schlick’s position.

It may be wondered why Neurath was not clearer on these points. Above, I already noted one reason for Neurath’s obscurity. More generally, that Neurath’s disagreement with Carnap and/or Schlick found only oblique expression during stage one of the protocol sentence debate, and was voiced clearly only during stages two and three is explained, I believe, by the politics of the Viennese scientific philosophy. Neurath was a firm believer in the principle that no comfort was to be given to the enemy, metaphysical "school philosophy", by public quarrels amongst proponents of the scientific philosophy itself, unless it really could not be helped. As a result of Neurath’s maneuvering, however, his early opposition to the core of the traditionally ascribed aim, and that of the newly recognised alternative neo-Kantian one, has been overlooked.

Neurath’s early opposition to Carnap fell, as I showed, into two substages. During the first, Neurath opposed Carnap’s idealisations without quite being able to undermine

56See above Appendix to chapter 2.
57It is typical therefore that Neurath should declare his by 1932 long-standing and quite crucial dispute with Carnap over the tenability of methodological solipsism to be only a marginal one (Neurath 1932d); Carnap, less concerned with the socio-political dimension of Vienna Circle philosophy than Neurath, responded by bluntly declaring the issue as he understood it—namely that of the form of protocol sentences—to be one very central one for epistemology (Carnap 1932e).
Carnap's central, and to him objectionable, assumption that knowledge could be reconstructed on the basis of the phenomenal given of a solitary epistemic subject. During the second substage, Neurath relied on Neider's argument to establish what he took to be the irrelevance of Carnap's phenomenalistic reconstruction. As explicated in the Aufbau, scientific statements could not be intersubjectively tested. Neider's report did not give the date of his conversation with Carnap. It is clear, however, that this point converged on that recognised by Carnap's additional argument for the need for a materialistic constitution system: intersubjectivity required explication in the physical language. It follows therefore that these conversations preceeded both Carnap's 1930 lecture and Erkenntnis paper I quoted above, for this very point made its first appearance in his writings there. In view of Neurath's strident materialism since his 1929 address it follows too that these conversations took place before the Prague conference as well. Carnap (and Frank) placed "the beginnings of physicalism" at "ca. 1930", the year of his public adoption of the argument. Close attention to Neider's report and Neurath's Prague address requires us to place it in 1929: there the decisive step towards "physicalism" was taken with the acceptance of Neider's argument. (What exactly "physicalism" amounted to, however, became the central point of dispute between Carnap and Neurath during stage two of the protocol sentence debate.)

Finally, let me ask: how then did Neider's argument constitute the "empirical" argument towards which Neurath had hinted? It focussed upon scientific practice. Science was, as a matter of fact, a social activity and a legitimisation of its knowledge claims had to respect the empirical facts of it. In response to Neider, Carnap did so in a negative way: he rejected the claim of the solipsistic constitution system to reconstruct the intersubjectivity of scientific practice. But note that Carnap retained his Aufbau position for the purpose of his

58Carnap 1963a; see also Frank 1949:36.
epistemological project of rational reconstruction. By contrast, Neurath himself--so his remarks indicate--was inclined to draw a more radical consequence still, namely that epistemology had to positively embrace empirical argumentation. This meant, in the very first place, that epistemology too should abstain from reliance upon the ideal solipsistic language. Here the theme for act two of the protocol sentence debate was set.
Appendix to Chapter 4: Carnap's Psychological Sketch of the Given as a Response to Neurath's Challenge to the Relevance of Rational Reconstruction

In his short monograph *Scheinprobleme*, which, unlike most of the *Aufbau* was written after Carnap had moved to Vienna and joined Schlick's circle, Carnap proceeded to elaborate what amounted to his representational theory mind. There Carnap presented, unlike in *Aufbau*, a psychological theory-sketch of the given. In the *Aufbau*, Carnap decreed the scientific legitimacy of introspective psychology on the grounds that only its phenomenalist language could capture the order of epistemic priority relevant for epistemology. This was possible because of the presumed fact of the intertranslatability of the solipsistic and physical languages. Both languages were considered extensionally equivalent; so even if actual cognitive processes were to operate with the language of physical objects straightaway the reconstruction in terms of the solipsistic language still captured all the extensional content there was to a subject's statements. There Carnap let the matter rest in the *Aufbau*. But this clearly did not yet redeem Carnap's methodology: an indication was needed of how it was possible for him to assume a certain order of epistemic priority and justify empirical assertions along this line, while he freely admitted that in actuality subjects did not, neither consciously nor unconsciously, compute their empirical statements from elements of the immediate given. What Carnap did, therefore, was to provide an account which "psychologised" the meta-theoretical distinction between the contexts of discovery and justification: corresponding to these contexts of inquiry, there existed different aspects and/or dimensions of the given.

59 The much sharper condemnation of metaphysics as not only lying outside of science but as strictly meaningless betrays his new alliance.
60 Carnap 1928b:310.
The *Aufbau*, following Gaetschenberger's (and, unbeknownst to him, the early Schlick's) lead, treated the given as a sign.\(^{61}\) To determine the internal structure of this sign, the given, Carnap developed the method of quasi-analysis of elementary experiences. He defended the adequacy of his extensional reconstruction of the meaning of the given (and so, ultimately, of the intertranslatability thesis) by drawing a sharp distinction between two kinds of statements: "nominatum statements", which concerned which objects were designated by an expression or sign, and "sense statements", which concerned the "content of a representation or thought" involved in the use of signs or expressions.\(^{62}\) Apparent failures of truth-functionality, and thus extensionality, were explained as finding their explanation in the fact that the statements exhibiting such failures, e.g. sentences about what a person believes, did not concern what statements obeying truth-functionality were concerned with, namely propositional functions, but rather psychological contents. Nominatum statements and sense statements applied to different domains.

In response to the challenge to the relevance of rational reconstruction, Carnap attempted to give an account that sought to bridge the gulf between the logical and the psychological meaning of expressions and signs. In *Scheinprobleme*, Carnap tried to spell out how the given constituted a sign such that both nominatum and sense statements could be made about it. In this way Carnap hoped to establish that and how accounts of scientific practice and rational reconstructions were related. Whereas in the *Aufbau*, Carnap abstracted from all psychological considerations and discarded any concern with sense statements, he now sought to integrate the conception of meaning developed there into a psychologically realistic picture of cognition. The given, previously a pure abstraction,

\(^{61}\)Gaetschenberger 1920 (cited by Carnap); his first tenet was: "the psychic given is a sign". Compare Schlick 1910 (not cited by Carnap).

\(^{62}\)Carnap 1928a:74-7.
now was portrayed as a psychologically real entity: Carnap set out to investigate the sense of the given.

Carnap's account is somewhat obscure, but here's the gist of it. He distinguished between two different kinds of mental representations accompanying a statement or thought; both represented two aspects of the sense of the given as a sign. "Factual" or "stated" representations assert the obtainment of a fact, that is, they are capable of truth and falsity. Merely "accompanying" (namely, accompanying a stated representation) or "object" representations constituted associations which did not find expression in the factual representation. To give an example: the thought that a given bench is small possesses the factual representation of a given bench being small; yet there may also be accompanying representations, for example, that the given bench is green or that it is one the subject sat on a long time ago; there may even be accompanying representations of apparently unrelated things, like musical tones or of a particular mood.

In Carnap's terminology, only the factual representations possessed "theoretical content". Accompanying representations, on the other hand, did not express a theoretical content and so needed not to be considered in the rational reconstruction of knowledge claims for statements.

[A] factual representation can form the content of a statement, while an object representation cannot. [...] The accompanying object representations, since they cannot become the content of a statement, are beyond truth and

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63 Carnap 1928b:329-331. According to Carnap it depended "essentially" on the subject's intention whether a representation was "factual" or not: there are "object" representations which could, given the right intention, become "factual" ones. (For the terminology see the next three sentences in the text above.) What does it mean to "intend" a mental representation as an assertion? Are there, as it were, "accompanying factual" representations?

64 Note that "theoretical content" here is one aspect of the sense of the given as a sign. This Carnapian use of "content" diverges from the previous one, followed in accordance with Schlick, which designated the apparently non-structural "content-stuff", the "material" of experience. It is nevertheless consistent with the prior usage: as an explication of the "sense" of the given it is concerned with what rational reconstruction, concentrating on structure, is not concerned with; on the other hand the addendum "theoretical" indicates the non-standard, non-intuitive nature of this kind of "content".
falsity. While the theoretical content must be justified by reference to some criterion [...] the object representations which accompany a statement are not subject to any theoretical control; they are theoretically relevant but frequently of great practical importance.65

Carnap explained what this practical importance consisted in with reference to statements about other minds. Carnap held, as we know by now, that the auto- and hetero-psychological languages were (at least in principle) intertranslatable with the physical language. Further comparing them Carnap remarked:

Aside from having greater simplicity, the psychological language also expresses more than the physical language but this more does not consist of theoretical content; it expresses only accompanying representations; these are merely object representations, that is, representations which do not stand for any fact, and hence cannot form the content of a statement.66

Carnap thus was able to employ the distinction between the two kinds of representations in defense of his reduction of the hetero-psychological language to the physical. This reduction proceeded in conformity with the rational reconstruction's concern with "logical" instead of "epistemic" value. To hold that talk of other minds expressed more facts than talk of others' behaviours accordingly was to confuse the content of stated representations with accompanying object representations. Yet more than that: since the solipsistic language was basic in Carnap's constitution system, any appearance of another language expressing more than it was mistaken. To hold that the physical language expressed more facts than the solipsistic language would thus be also to confuse the stated with the accompanying representations.

Clearly, Carnap's distinction of types of mental representations is of interest to the question of the relevance of rational reconstruction. Carnap mentioned the practical importance of "association" in learning and generally spoke of actual cognition as

65Ibid.; my italics.
66Ibid.:335.
proceeding "intuitively":67 he even conceded that psychologists relied upon empathy in their work.68 But he warned that "empathy is not cognition; it does not produce any theoretical content or anything that can be stated." The results so gained still needed "a justification that does not depend upon empathy".69 Such a justification would explicate the theoretical content of the claim by retracing the constructional steps of his constitution system and reducing them back to the given of the subject making the knowledge claim. By contrast, actual cognition of other minds proceeded by association of object representations. According to Carnap, the later took as their basis representations of the subject's own mental states and ascribed them to the other by something like analogical reasoning.70 The "practical value" of object representations then consisted in the facility which they afforded to cognitive processes in contrast to the cumbersome computations by which constitution theory established the justification of any knowledge claims.

Carnap's proposed solution to the challenge of the relevance of rational reconstruction accordingly was the following. The bridge between justificatory accounts of knowledge and accounts of the actual cognitive processes was provided by a representational theory of mind which distinguished, within the psychologically real stream of consciousness, two aspects of the sense of the given considered as a sign, namely two kinds of mental representations, factual and object representations. (The theoretical content possessed by factual representations corresponded to what nominatum statements about the given expressed.) Whereas the psychology of reasoning and discovery focussed on the entire sense of representations, epistemological inquiries into justification for knowledge claims focussed only upon the extensions of the "stated" represented thoughts, that is,
upon nominatum statements about the given. Consequently, Carnap held that the justification of knowledge claims provided by rational reconstruction was relevant to the knowledge gained in actual cognitive practice because they shared this common object: the given as a sign.

Did Carnap's psychological theory succeed in showing the relevance of his method of rational reconstruction to the knowledge gained in actual practice? Carnap's psychological theory instantiated his epistemological model, it did not defend it: the relevance of rational reconstruction was not established thereby. Let's consider the relation between the two kinds of representations and their relation to rational reconstruction and actual cognitive processes. (1) Actual representations provided for theoretical content, object representations for intuitive content. (2) The content of factual representations was explicated by the constructional language, that of object representations (if at all) in the natural language. (3) Theoretical content was structurally determined, intuitive content apparently not. So far, no interaction between the two kinds of content took place. Yet it may well be held that actual cognition nevertheless had to rely on factual representations, for the presumably merely associational "content" of object representations was not enough for gaining knowledge. But how could it do so? By definition, the theoretical content of factual representations was concerned with the given alone, it was solipsistic. (4) Theoretical content did not provide for intentionality proper, that is, transcendent object relations such as thoughts of a genuine (not reconstructed) other, intuitive content did. This means that (5) whereas the reconstruction of knowledge proceeded via factual representations, actual cognition could only proceed via object representations alone; theoretical content was not psychologically salient, whereas intuitive content did possess psychological reality. So no account was given of how the factual representations contributed to, or interacted with, actual cognition.
Furthermore, since factual representations were explicated purely solipsistically, no account was provided of how object representations came to have the intuitive content they have. Carnap apparently held a psychologised Fregean view according to which the sense of an expression or sign was the mental representation of its referent or nominatum.\textsuperscript{71} The sense of the solipsistic constructional language and the realistic natural language accordingly mentally represented objects of entirely different domains, for the referents of either language were different. This meant that in Carnap's psychological theory the flat of the intertranslatability thesis between the physical and the auto-psychological language was at best illustrated, but not defended: no account was given of how the contrast (4) was to be bridged. Without such a bridge, however, the two systems of representations remained as far apart as actual cognition and rational reconstruction were in the first place.

To rephrase Carnap's project and its failure slightly differently. Accounts of sense, that is, of mental representations, were held competent to answer questions about the context of discovery. The psychological theory split the sense of the given qua mental representation in two: factual representations carried the theoretical content, object representations carried the intuitive sense. What then did the work in actual cognition of the physical and social world? As we saw, it was the object representations! As far as actual cognition was concerned the factual representations remained completely idle. Since the factual representations remained idle in this account, they were wholly unnecessary, unless certain epistemological assumptions had to be accommodated. Their presence was required only to accommodate Carnap's assumption about the epistemic priority of the phenomenal given.

\textsuperscript{71}Carnap 1928a:74/5.
Suppose then again that Carnap thought that his psychological theory sketch integrated the assumptions about cognition made by his methodologically solipsist rational reconstructionism into accounts of actual cognitive processes. Given his failure to motivate these assumptions independently, one can see why Neurath was unimpressed with this response to the challenge. to the relevance of Carnap's method of justifying knowledge claims to scientific practice. Thus the criticism of his 1928 review of the Aufbau and Scheinprobleme was phrased as it was, namely, obliquely doubting the adequacy of methodological solipsism to the epistemological task.\textsuperscript{72}

\textsuperscript{72}Note also the reasons for Neurath's obliqueness adduced in sect. 5.2.2 and 5.5.
Chapter 6

THE BACKGROUND OF STAGES TWO AND THREE: CARNAP'S SYNTACTIC TURN AND ITS RECEPTION BY NEURATH

Abstract: The new framework of Carnap's theorising against which stages two and three of the protocol sentence debate proceeded. Neurath must be read as following Carnap's "syntactic" lead and that, to understand him, we must understand Carnap's syntactic (6.1). The main features of Carnap's "logical syntax" (6.2) and more controversial theses: despite appearances to the contrary, reasoning about meaning was not barred in the syntactic framework (6.3). The failure of Carnap's syntacticism. Neurath's variant of escaped such objections (6.4). Summary (6.5).

The first stage of the protocol sentence debate was concluded in 1930. To talk of a "conclusion" is misleading, of course: the between-the-lines debate of Carnap and Schlick remained unsolved, and the only slightly less oblique debate between Carnap and Neurath was not resolved either. As already indicated, Neurath's challenge was far more radical than Carnap's response to Neider's argument could accommodate. Nevertheless, stage one had this result: due to the debate between Carnap and Neurath, the physicalist language was placed in the center of efforts to work out the form of the language of unified science. The second stage of the protocol sentence debate was concerned with the question of what the thesis of physicalism entailed. It was taken up wholly by Neurath's efforts to press his radical point against his reticent fellow physicalist Carnap.

It must be noted that this debate was most likely not the problem foremost in Carnap's mind at the time: the second stage of his debate with Neurath coincided with the syntactic turn which marked his syntactic period of philosophising, culminating in his 1934
Logische Syntax der Sprache (Logical Syntax of Language). By "going syntactic", Carnap sought to escape the problem that beset the Aufbau, namely, the need to declare "foundedness" a logical concept. (As noted, the problem in fact was the ineliminability of the need to presuppose an interpretation of its basic descriptive relation and, therefore, the failure to structuralise knowledge completely.)

As we shall see, the syntactic turn "allowed" Carnap to fix the interpretation of scientific theories without appeal to intuition. The second stage of the debate between Carnap and Neurath must accordingly be read against the background of this changed framework.

Just as my review of the first stage of the protocol sentence debate required Carnap's then current project to be introduced as the common reference point of the disputants, so my review of stages two and three requires the salient features of Carnap's project during this period to be introduced. Before continuing my reconstruction of the debate, I therefore consider, in this chapter, Carnap's syntactic turn and its reception by Neurath. First, I show that Neurath's thinking followed Carnap's on the matter of logical syntax and meta-logic and that, therefore, understanding Neurath's arguments requires understanding Carnap's syntactic turn. Then I turn to Carnap's view; I show that, despite common presumption to the contrary, Carnap's syntactic conception did allow reasoning about meaning as long as it was conducted in formal terms, and how it could do so. Thirdly, I consider Neurath's adoption of Carnap's conception.

6.1 Neurath's Adoption of Carnap's Metalogic: Their Common Front Against the Wittgensteinians

The central point of the second stage of the protocol sentence debate, Neurath's attack upon Carnap's methodological solipsism as a "weakened residue of idealist metaphysics"\(^2\), was explicitly presented as a contribution to the cooperative venture of

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\(^1\)See above section 3.4.3.
\(^2\)Neurath 1932a:65.
Viennese scientific philosophy as pursued particularly with Carnap. Neurath was less deferential to the Wittgensteinian wing of the Circle. In the light of Neider's intersubjectivity argument--granted by Carnap--this is readily understandable for that argument runs squarely against the apparently solipsistic doctrine of the *Tractatus*.\(^3\) An anecdote of Hempel's about the Circle's reading of the *Tractatus* recorded Neurath's earliest misgivings against Wittgenstein's "metaphysics"\(^4\); already from early on Neurath took issue with Wittgenstein's doctrine that certain things (logical forms) could not be said but only shown. For Neurath, whatever facts there were expressible in words and, being expressible in words, were public facts. In this anti-Wittgensteinian stance Neurath was in agreement with Carnap, and with him in opposition to Schlick.

### 6.1.1 Neurath's Adoption of Carnap's Syntactic Turn

To demonstrate Neurath's adoption of Carnap's new framework, and to lead into what's at issue in the syntactic turn which Carnap's thinking about language had by then taken, let me note Neurath's anti-Wittgensteinian charge in his 1931 paper "Physikalismus" which opened the second stage (publicationwise\(^5\)).

[M]uch of what Wittgenstein and others say about the elucidations and the confrontation of language and reality cannot be maintained if unified science

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\(^3\)E.g.: "The world is my world: this is manifest in the fact that the limits of language (of that language which alone I understand) mean the limits of my world." (Wittgenstein 1921:#5.62, his italics.) Whether Wittgenstein himself is best understood as advocating (methodological) solipsism is an exegetical question beyond my purview here; what matters is that this seems to have been the preferred interpretation in the Circle. Similar considerations apply to the question whether Wittgenstein meant his simples indeed to be sense data; again that seems to have been the preferred interpretation of his Viennese readers. Thus note the anti-Wittgensteinian anti-solipsist thrust of Weinberg's 1936 critique of Logical Positivism and also that around this time Max Black judged Wittgenstein to be "the most solipsist, therefore, by conventional implication, the most anti-social of all philosophers" (1934:11.).

\(^4\) "Neurath made frequent interjections, 'metaphysics', during the Circle's reading and discussion of Wittgenstein's Tractatus, to the irritation of Moritz Schlick who finally told him that he was interrupting the proceedings too much. Hans Hahn, as a conciliator, suggested to Neurath to say 'M' instead. After much humming [...] Neurath made another suggestion to Schlick: 'I think it will save time and trouble if I say "non-M" every time the group is not talking metaphysics." (Hempel 1969 as related in Neurath 1973:82/3n.) See also Haller 1982d:xxi for Neurath's question, in the Circle meeting of 5/21/1931, concerning the thesis that the picture must share with the pictured its form: "Should we not even discuss the metaphysics that rests in these sentences? Should we simply accept these mythological explanations?"

\(^5\)See above Appendix to chapter 2.
is built on the basis of scientific language from the beginning; scientific language is itself a physical formation whose structure, as physical arrangement (ornament?), can be discussed by means of the very same language without contradictions.\(^6\)

Two things need explication here: the criticism of Wittgenstein and the indicated alternative.

Wittgenstein's project of grounding linguistic representations exempted the grounding moves themselves from the strictures placed on the discourse to be grounded. Talk about the logical form of expressions could not proceed within science and was strictly speaking meaningless - and uncontrollable. Neurath rejected Wittgenstein's conception of philosophical elucidations as a ladder to be thrown way once climbed, for in it he detected an attempt to construct scientific language with the help, so to speak, of pre-linguistic means. Here we also find the attempt to confront language with reality; to use reality to verify whether the language is servicable.\(^7\)

The Wittgensteinian elucidations illegitimately relied on "pre-linguistic means" to establish the sevicability of language. For Neurath, as Carnap reported, language was of this world, it did not refer to it "from without".\(^8\) I must add again that Neurath's criticism did not mean to impugn the testability criterion of meaningfulness: he noted that "[a] statement which cannot be controlled is a thesis devoid of sense."\(^9\) Neurath rather insisted that there was no need to fall into what he thought was Tractarian obscurantism. Given the testability criterion of meaningfulness there were no private facts and what is public can be talked about. Linguistic facts were as public as any others.

Neurath's formulation of his preferred anti-Tractarian alternative is bound to raise the eyebrows of presentday readers. Neurath required the language of science to be "universal" in the sense of "complete" advocated by Carnap, namely, as providing one

\(^6\)Neurath 1931b:53.
\(^7\)ibid.:52.
\(^8\)Carnap 1963a.
basic language for all of science.\textsuperscript{10} But Neurath also required the language of science to be "complete" in the sense of being able to talk about itself, that is, in the sense forbidden by Tarski's theory of truth. In other places too, Neurath claimed "[t]he one scientific language can talk about itself, one part of the language about the other"\textsuperscript{11}, and that it was "certainly possible to speak about one part of language with the help of another part."\textsuperscript{12} It is this sense of a language's completeness which will appear scandalous to modern readers.

It is important to note that Neurath must be understood as talking about Carnap's proposal for a "meta-logic", the project of what was to become known as his \textit{Logical Syntax} of 1934. About this project Carnap had \textit{Erkenntnis}-papers: "It will be shown [...] t! at the metalogic which speaks about the sentences of a given language can be formulated in that very language itself."\textsuperscript{13} Clearly, at this point in the development of his meta-logic, Carnap himself endorsed the very same eyebrow-raising conception of meta-linguistic discourse which Neurath gave expression to. Neurath's claim that "the one scientific language can speak about itself" followed Carnap's lead. Carnap's logical syntax incorporated Goedel's method and so too, therefore, did Neurath's adoption of Carnap's project. What Neurath had in mind as an alternative to Wittgenstein's mythical prolegomena was Goedel's method of arithmetising syntax which had already proved extremely influential on Carnap. Goedel had communicated his method and his results to Carnap before the publication of his seminal paper and still prior to the conference on the foundations of mathematics in September of 1930 in Koenigsberg.\textsuperscript{14} Carnap conceived of his Goedel-inspired meta-logic "like a vision during a sleepless night..."

\textsuperscript{10}Carnap 1932b:title; see also his 1963b.
\textsuperscript{11}Neurath 1931b:54.
\textsuperscript{12}Neurath 1932a:60.
\textsuperscript{13}Carnap 1932a:78.
\textsuperscript{14}At this conference Goedel referred to his incompleteness result in the discussion with Hahn, Carnap, Von Neumann, Scholz, Heyting and Reidemeister ( \textit{Erkenntnis} 2:135-149, see esp. 148). The published transcript of these discussions was followed by a two-page "Nachtrag" in which Goedel reported in summary form the results of his "Uber formal unentscheidbare Saezte der Principia Mathematica und verwandter Systeme" (Goedel 1931b; compare Goedel 1930 and 1931a). See also \textit{Davison} 1986 and Kleene 1986a.
in January 1931", and gave three lectures to the Circle on this topic in June 1931 before taking up his professorship in Prague.15 Clearly, even if Carnap had not communicated his thoughts on this matter to Neurath in private discussions (as I suspect), Neurath would nevertheless have known about Carnap's project.

6.2.2 The Origin, Point and Limits of Carnap's Syntactic Turn

To decide what to make of Neurath's and Carnap's apparently scandalous claims then, we must consider: what was Gödel's method and what were his results, what was Carnap's view of these matters, and what, finally, was Neurath's understanding of them? A progressively hairy question.16 Let's start with Carnap. Carnap's syntactic turn was influenced not only by Gödel's work, but also by Hilbert's and Tarski's.

[T]he members of the Circle, in contrast to Wittgenstein, came to the conclusion that it is possible to speak about language and, in particular, about the structures of linguistic expressions. On the basis of this conception I developed the idea of the logical syntax of a language as the purely analytical theory of the structure of its expressions. My thinking was chiefly influenced by the investigations of Hilbert and Tarski in metamathematics. I often talked with Gödel about these problems. In August 1930 he explained to me his new method of correlating numbers with signs and expressions. Thus a theory of the forms of expressions could be formulated with the help of concepts of arithmetic. He told me that, with the help of this method of arithmetization he had proved that any formal system of arithmetic is incomplete and incompletable.17

Carnap's metalogic was conceived against the background of the Wittgensteinian objections to the Aufbau. Carnap's metalogic "aimed at the construction of a general theory linguistic forms."18 Carnap adopted, and intended to generalise, Hilbert's formalist method. This was, in Carnap's terminology, "the syntactic method, [which] consists in describing a language together with its rules of deduction by reference only to signs and the order of

16 For example, it is not all that clear what Carnap's earliest understanding was, given his notorious sleight of hand in "demonstrating" analyticity to be a syntactic notion; see Kleene 1939, Quine 1940, Beth 1963, Carnap 1963a:56 and 1963b:927-33, Coffa 1976, Koehler 1985:199, Friedman 1988, Dawson 1986. More on this issue below, section 6.3.1.
17 Carnap 1963a:53.
18 Ibid.:54.
their occurrence in expressions, thus without reference to meaning." According to the tradition of Hilbert, such formalist-syntactic reasoning about language must take place in a meta-language. When Tarski came to Vienna in February of 1930 to deliver some lectures on metamathematics, he stressed to Carnap that "concepts used in logical investigations, e.g. the consistency of axioms, the provability of theorems in a deductive system, and the like, are to be expressed not in the language of axioms (later to be called the object-language) but in the metamathematical language (later called the metalanguage)." Tarski pointed out that the metatheory could itself be put into an axiomatised form. What Goedel's method finally showed was that the language of arithmetic could serve as its own meta-language (any consistent theory in which was incomplete if recursively axiomatisable). Carnap's "general theory of linguistic forms" sought to build on Goedel's method. The important points of Carnap's syntactic turn were, therefore, that talk about a given language had to proceed in formal, syntactic terms, and that such talk proceeded in a meta-language.

What then about Carnap's and Neurath's "scandalous" claims? Goedel's method showed that not all had the language of arithmetic speaking about itself only after a fashion: the language of arithmetic could form its own meta-language, but this meta-language only spoke of its formal features. If such a language was to be consistent, then, so he proved, it had to be incomplete: there were truths expressible with a predicate co-extensive with those of the language in question which could not be derived within that language. Consider now Carnap's later disclaimer that he did not intend the universal language to be able to express "everything". Carnap's disclaimer does not rule out what is perhaps nowadays

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19 Carnap 1963b:928. Note, however, that Carnap rejected Hilbert's doctrine of formalism, the thesis that applying the formalist method "is the only way of constructing an adequate system of mathematics, since it is impossible to give an interpretation for (classsical) mathematics". Carnap instead accepted Frege's and Russell's thesis of logicism, namely the thesis "that all terms of mathematics can be interpreted in terms of logic" (ibid.).

20 Carnap 1963a:30.

21 Tarski 1930.

22 Carnap 1963b:880.
most naturally assumed to be ruled out, that the universal language could talk about itself. Instead, Carnap's disclaimer merely ruled out that the envisaged universal language stated everything that could be truthfully stated about arithmetic, and that it did not speak about its own meaning. What is to be noted, then, is that Carnap's and Neurath's claims did not go against (most of) Tarski's later strictures, because as their envisaged languages did not speak about meaning (at least not officially).\textsuperscript{23}

Insofar then as the language of arithmetic belonged to the language of science, it was this arithmetical part which could talk about itself, and by means of which one part of the language of science could talk about its other parts. The attraction of Goedel's method was for Neurath undoubtedly its "universal" character, in the sense of "making do with one language". If it was "impossible to turn back behind or before language"\textsuperscript{24}, then it better be possible to use one part of language to speak about the other (and itself). Throughout his physicalist and post-1934 encyclopedist periods therefore, Neurath continued to hold to Carnap's recommendation to limit metalinguistic talk to talk of the forms of expressions: after all, according to Carnap, the syntactic meta-language of \textit{Logical Syntax} did everything metalogical required of it.

\textbf{6.2 Meta-Linguistic Talk According to Carnap's Logical Syntax}

I now turn to a closer characterisation of nature of meta-linguistic talk according to the model provided for it by Carnap's logical syntax.

\textbf{6.2.1 The Material and the Formal Mode of Speech}

For Carnap, metalinguistic talk had to be handled extremely carefully. In his "Ueberwindung der Metaphysik ..." ("The Elimination of Metaphysics ...") Carnap had

\textsuperscript{23}Of course, they went against Tarski's strictures insofar as Carnap intended to cover the concept of logical truth by such reasoning which was considered a syntactic notion, unlike that of factual truth (Carnap 1934/7:216). The parenthetical remark will be cashed in below, sections 6.3 and 6.4.2.

\textsuperscript{24}Neurath 1931b:54.
declared "correct" the metalogical formulation of the questions which were often also pursued under the guises of logic (namely, in terms of truth), epistemology (namely, in terms of verification), and phenomenology (namely, in terms of meaning).\textsuperscript{25} Why an approach to these questions in the meta-logical terms of syntax (deducibility) was superior was explained by Carnap in his "Die physikalische Sprache als Universalsprache der Wissenschaft" (translated as \textit{The Unity of Science}).

Carnap drew attention to the distinction of the "formal mode" of speech from the "material mode" of speech. This often misunderstood distinction of Carnap's applies to talk about language, and thus for Carnap, to philosophical talk. The "material" mode of speech speaks about the objects which the language under investigation refered to; the "formal" mode of speech, by contrast, speaks only of the formal aspects of the language under investigation. Whether this formal mode of speech precluded talk of the meaning of language for Carnap, as is generally held, remains to be seen; this depends on what Carnap took the formal aspects of language to comprise. Let me note, then, that the formal mode of speech was first and foremost the meta-linguistic mode of speech. When Carnap held that certain sentences apparently "material" ought to be understood "formally", this meant, then, that the material mode of speech was inadmissible for the meta-language because it concerned empirical facts of the world, not of the language. The subject matter of the meta-language was not the world the object language talked about, but the form of the object language itself. Thus Carnap's (and Neurath's) insistence on the use of the formal mode of speech in the theory of science, that is, on the use of a meta-language for the investigation of the (object) languages of science.\textsuperscript{26}

Carnap distinguished between object-sentences, syntactical sentences and pseudo-object and quasi-syntactical sentences. Carnap's concept of pseudo-object sentences relied

\textsuperscript{25}Carnap 1932a:62.
\textsuperscript{26}Carnap 1932b:37/8,40.
on the distinction of formal and material talk. The delimitation of this class of sentences was crucial to the implementation of his distinction between the material and formal mode of speech in the service of overcoming metaphysics. Object sentences were concerned with whatever the objects of empirical inquiry were. Syntactical sentences were concerned with the formal properties of assertions. Pseudo-object sentences were sentences which had the form of object sentences but were not object sentences after all. Unless analysis showed them to be quasi-syntactical sentences, they were meaningless. Philosophical sentences typically were pseudo-object sentences. If it was possible to make sense of them, then it turned out that they in fact concerned syntactical questions, that is, they turned out to be quasi-syntactical sentences. In such a case, there existed a proper syntactical predicate with the help of which their content could be expressed; it was merely their form which mislead people to think of them "in the material mode" as a peculiarly philosophical type of object-sentence. If they could not be so translated then they remained mere pseudo-object sentences. Metaphysical philosophical sentences, so Carnap, were mostly pseudo-object sentences incapable of being turned into syntactical sentences for they were "autonomous": their "material mode" of expression disguised a syntactic category mistake. (Non-syntactical philosophical sentences not convicted in this manner were convicted of being metaphysical by exhibiting their lack of a deductive relation to scientific protocol sentences.)

6.2.2 The Principle of Logical Tolerance

Carnap held that his logical syntax answered the problem which had occasioned Wittgenstein's obscurantism. Given Goedel's method of arithmetising syntax, a language could talk about itself without falling into the type-theoretic paradoxes which had scared Wittgenstein. Given Carnap's non-standard reconstruction of 'analytic' (to be explicated

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27 Carnap 1934/7:302-308.
29 Hart 1971; see above section 4.3.1.
below), a language was even completely specifiable in formal terms. The possibility of
specifying logical form was thus mathematically “proven” in all its desired generality: no
longer was it merely a question of contrary conviction. Furthermore, no longer was it
necessary to exhibit the structure of the language: if one language could be structurally
described then so could any other.

It is not our business to set up prohibitions but to arrive at conventions. [...] In logic there are no morals. Everyone is at liberty to build up his own
logic, i.e. his own form of language, as he wishes. All that is required of
him is that, if he wishes to discuss it, he must state his methods clearly, and
give syntactical rules instead of philosophical arguments. 30

Carnap embraced "the principle of logical tolerance". (In the Circle, the principle of the
conventionality of language forms had been long urged by Menger. 31) The principle of
logical tolerance also had a more general consequence. It prescribed that there were no
logical limits (beyond consistency) to the choice of the languages of science describable and
constructible by the method of logical syntax. (That could be so described and constructed
irrespective of what other objections might be raised against, e.g., non-finitist languages
which many in the Circle regarded with suspicion.)

6.2.3 Pure and Descriptive Logical Syntax

Carnap's logical syntax could describe and construct the language of both the
formal and the empirical sciences. The former could be done in "pure" logical syntax, the
latter in "descriptive" logical syntax. Both kinds of logical syntax were distinguished by the
kind of constants employed in the meta-language. For the treatment of the logical and
mathematical languages by pure syntax the requirement for the kind of constants used was
the following.

No reference to the meaning of the signs and expressions is made in logical
syntax. Since only the logical structure of the expressions is involved,

30 Carnap 1934/7:51/2.
31 Menger had made his first plea for logical tolerance in 1927 (see Menger 1979b).
syntax-language, i.e. the metalogic serving for the formulation of logical syntax, contains only logical constants.\footnote{Carnap 1963a:54.}

With a meta-language containing only formal logical constants it became possible to give a description of any and all languages of the formal sciences in purely logical terms. For the languages of the empirical sciences to be treatable by Carnap's syntactic method, it was necessary to use descriptive logical syntax. In descriptive syntax, terms descriptive of the form of non-logical terms of a particular language were admitted into the meta-language, in addition to the logical constants. What this descriptive syntax imposed, we note, was a sharp distinction between logical talk and descriptive talk.

\section*{6.3 The Anomalies of Carnap's Syntacticism}

The task which Carnap had assigned for the syntactic method was extremely broad. It encompassed the description of the language of strictly constructivist arithmetic (language I of \textit{Logical Syntax}) as well as that of classical mathematic and classical physics (language II). Let me now ask "Did Carnap's program work?" I shall pursue two questions. First, just exactly how the notion of analyticity itself was to be defined syntactically in the face of Goedel's result, second, just exactly how the descriptive terms of empirical science were to be formally described. It turns out that Carnap's proposals for both his pure and his descriptive syntax were seriously flawed, if they are read with a contemporary understanding of the key terms involved. But even read with Carnap's now non-standard meanings in mind, they remain problematical. I first deal with the proposal for his pure and then with that for his descriptive syntax.

\subsection*{6.3.1 The Tenet of Pure Syntax that Analyticity is a Syntactic Notion}

Carnap's central claim in \textit{Logical Syntax} was that the formal method $\text{[car]}$ represent concepts which are sometimes regarded not as formal and designated as concepts of meaning $\text{[or concepts of a logic of}$
meaning), such as, for instance, consequence relation, content, relations of content, and so on. Finally we have established the fact that even the questions which refer to the interpretation of a language and which appear, therefore, to be the very opposite of formal can be handled within the domain of formal syntax.\(^{33}\)

Analyticity in the sense wide enough to encompass classical mathematics was viewed as a syntactic notion. Carnap's conception (or definition) of analyticity as formal or syntactical is problematical. There are two aspects to this problem. The first concerns the expressive power of the meta-language in which the analyticity of given deductive relations in the object-language was stated as a formal notion. The second concerns the contrivance which allowed Carnap to claim analyticity as a syntactic notion. Both aspects of the problem concern Carnap's definition of all logical, i.e. analytical sentences as "determinate"\(^ {34}\); yet this concept was "indefinite", i.e. non-demonstrable in a meta-language which, as Carnap's program required, was equal in expressive resources to the object-language.

The first aspect of Carnap's problematic definition was this. On the one hand, Carnap recognised that his proof of the non-contradictoriness of language II (the language of classical arithmetic) "is essentially dependent upon the use of such syntactical terms as 'analytic' which are indefinite [non-demonstrable] to a high degree, and which, in addition, go beyond the resources at the disposal of language II."\(^ {35}\) But, on the other hand, Carnap also "answered in the affirmative" the question "can the phrase 'for all properties' (interpreted as 'for all properties whatsoever' and not 'for all properties which are definable in S') be formulated in the symbolic syntax-language S'?\(^ {36}\) Carnap thought it possible that the meta-language contained quantifiers ranging over proofs which were not constructable in the object-language. Just this capability of the meta-language of "defining" terms ranging over an unlimited domain is not nowadays viewed a "syntactic" operation, but instead viewed as a semantical operation par excellence. As Carnap himself noted

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\(^ {33}\)Carnap 1934/7:233.

\(^ {34}\)Carnap 1934/7:179.

\(^ {35}\)Ibid.:129.

\(^ {36}\)Ibid.:144.
implicitly, the definition of analyticity-in-L-II required recourse to a language of higher type than language II itself.\textsuperscript{37} Just that ascent in the interpretation of the quantifiers of the meta-language, namely the widening of its domain beyond that of the object language to non-constructable objects, is a semantic ascent, as Tarski was perhaps the first to note in publication.\textsuperscript{38}

The second aspect of the problem of Carnap's definition of analyticity as formal was that (as Carnap admitted in response to Beth's later query) that his logical syntax presupposed an interpretation (and thus incorporated semantic considerations).\textsuperscript{39} This presupposition was in fact crucial to his enterprise. The gap of the non-demonstrability of the determinate concepts of the syntax language, that is, the unprovability in the syntax-language itself that the conditions of the use of all determinate concepts were satisfied, was filled by Carnap's notion of interpretation.

For a meta-language to "speak about" the object language in Carnap's sense required the translation of expressions of the latter into expressions of the former. Now, to translate an analytic sentence of the object language into the meta-language obviously required the analytic relations to be preserved in the meta-language. Yet, as we saw, the syntactic definition of "analytic-in-L" could not be given in L itself. What then guaranteed the adequacy of the translations of analytical sentences, namely, that they themselves be analytical? It was here that the notion of interpretation came in. Interpretation allowed for "true-to-sense" translation, a translation which respected "historically known speech habits"\textsuperscript{40}, and thus respected analyticity. Consider what is involved in interpretation according to the account of Logical Syntax.

\textsuperscript{37}Ibid.:88, 113.
\textsuperscript{38}See especially the 1935 postscript to Tarski 1933/5. Goedel seems to have drawn similar consequences in unpublished work several years earlier: see Wang 1986:144. The point was driven home for all to see in Kokoszynska 1936a (see also her 1936b).
\textsuperscript{39}Carnap 1963b:928; Beth 1963:477.
\textsuperscript{40}Ibid.:228.
The interpretation of the expressions of a language S1 is thus given by means of a translation into a language S2, the statement of the translation being effected in a syntax language S3; and it is possible for two of these languages, or even all three to coincide. [...] The interpretation of a language is a translation and therefore something which can be formally represented; the construction and examination of interpretations belong to formal syntax. [...] a syntactically given translation, Q1, of S1 into S2 is true-to-sense if it is equipollent in respect of S3.\textsuperscript{41}

For Carnap, translation was a transormance of expressions of a language S1 into expressions of a language S2 which was effected in a language S3 which contained S2 as a sublanguage. Consider now the translation of S1 into S2 which preserved the analytic relations of S1 in S2 as a true-to-sense translation would. This translation, so Carnap, was in effect conducted in S3. Now the question arises: could S2 coincide with S3 or need it be a proper sublanguage of it? The answer is that, to guarantee its adequacy, the translation had to be conducted in a language S3 which contained S2 as a proper sublanguage: only in such a language S3 could the notion of "analytic-in-S2" be defined.\textsuperscript{42} From the standpoint of S2, analyticity-in-S2 consisted in the relation of consequence from a set of premises in an infinite series of derivational steps. The notion of L-determinacy, namely determinacy in S2, as which analyticity was here spelled out, thus depended for its intelligibility on the postulatory fiat that S2 was a proper sublanguage of S3. Yet not any "bigger" language would do. S3 had to be such that what were claimed to be analytical sentences in S2, namely, statements about the formal properties of expressions of S2, also held for these objects as conceived in S3.\textsuperscript{43} The language S3 with which "analytic-in-S2" was to be defined thus had to be chosen according to an intended interpretation.

That an interpretation was presupposed in order to make sense of the analyticity of Carnap's syntax language, of course suggests, as it did to one of the earliest reviewers of the English version of \textit{Logical Syntax}, that "[t]o say what is formal, and what is non-

\textsuperscript{41}ibid.:228/9. By "equipollence" Carnap understood the sameness of meaning of two sentences s1 and s2, syntactically comprehended as the co-extensionality of the sets of non-analytic sentences derivable from S1 and S2 (ibid.:42).

\textsuperscript{42}ibid.:224, 228.

\textsuperscript{43}ibid.:114.(?)
formal, it appears that we must presuppose essentially the desired distinction in the syntax language."\(^{44}\) Carnap saw no problem in this presupposition of a sharp distinction between logical and descriptive terms.\(^{45}\) The problem was, however, that this distinction of logical and descriptive terms, and synthetic and analytic sentences, which was not formally reconstructible in the object language (and not even in the meta-language as conceived of in the \textit{Logical Syntax}, namely, as equal in expressive power to the object language), but only in the meta-meta-language. Since the description of this meta-meta-language lay by definition outside the reach of Carnap's syntax language, calling analyticity a "formal" or "syntactic" notion was highly problematical. Carnap's definition of analyticity as the relation of consequence from a set of premises in infinite steps, which was meant to bridge the gap of non-demonstrability in a "formal" way, was not sufficient to make up for this: it only redescribed the problem. (Appropriately enough, Tarski was to point out not only the semantic character of the kind of "formal" operations envisaged by Carnap, but also raised the first doubts over the sharp distinction of logical and descriptive terms.\(^{46}\)

Having demonstrated the two problematic aspects of Carnap's definition of analyticity as formal, I now turn to the question: are the two problematic aspects of declaring analyticity to be formal just terminological points? Carnap's remarks about the "defectiveness" of "every language of arithmetic" make clear that his notion of "formal" (syntactical) and "formalisable" (syntactically explicable) is, to say the least, non-standard: the logical determinacy of "analytic" in the languages of mathematics "can be formalised but [...] cannot be exhausted by one system; it requires an infinite series of ever richer

\(^{44}\)Kleene 1939:85.

\(^{45}\)Indeed, in a fashion reminiscent of his handling of the problem of the outstanding elimination of the basic descriptive relation in the Aufbau, Carnap noted about theorem 50.1 of his \textit{Logical Syntax}: "Every logical sentence is determinate; every indeterminate sentence is descriptive. With the given form of definition for 'logical' this follows directly. If 'logical expression' is defined in some other way (for instance, by a statement of the logical primitive symbols, as in languages I and II) \textit{then the definitions of the terms 'valid' and 'contravalid' (which in I and II coincide with 'analytic' and 'contradictory) must be so contrived that every sentence is determinate.}" (Carnap 1934/7:179; my italics.)

\(^{46}\)Tarski 1936.
languages."\(^{47}\) Noting the mere non-standardness of Carnap's terminology is not the end of the matter, however: noting what kind of considerations Carnap thought pursuable in the syntactic formal mode requires us to revise the common conception of what Carnap in fact attempted to do in his "syntactic" phase: not what we nowadays understand by "syntax" (however "logical")!

6.3.2 The Underlying Tenet that Form is Meaning

Consider what Carnap took to be the domain of logical syntax. Not only did Carnap think analyticity a formal notion but empirical content as well! In his 1932 reply to Zilsel, Carnap as usual noted that his "theory of the structure of the sentences of any language [...] characterises this structure only by their (serial) order and the type of signs without referring to the meaning of the signs." At the same time, however, he thought of and used the names "logical syntax", "meta-logic" and "semantics" as cognates!\(^{48}\) This suggests that Carnap thought of this syntactic method as equivalent to semantics: he thought the cognitive meaning of expressions (what he called "content")\(^{49}\) to be fully captured by the explication and specification of the formal structures in which it was possible for them to appear. This suggestion is born out when Carnap in another paper of the same period spoke explicitly of "semantics" and "semantic type[s]"\(^{50}\) What is remarkable is not only his happy embrace of the term "semantics" at this stage, but--what is more--what was dropped from the translation here: where the translation reads "... according to semantics, if ..." there the original reads "... according to semantics (logical syntax of language), if ..."\(^{51}\) Unless we declare that Carnap here made an unintentional howler which was quietly corrected in translation we must conclude that Carnap believed at

\(^{47}\)Carnap 1934/7:222.
\(^{48}\)Carnap 1932d:177.
\(^{49}\)This term of Carnap's must not be mistaken with Schlick's as a term for the intuitive material of cognition.
\(^{50}\)Carnap 1932c:177; my italics.
\(^{51}\)See Erkenntnis 3:119.
the time that form expressed meaning, that form is meaning under a different description. This was, of course, not a new view, but one which already animated his Aufbau, where the formal reduction specified the meaning of empirical concepts.

Note also how Carnap's view that analyticity is a formal notion played into this question. When Carnap rejected formalism as a doctrine and accepted only the formalist method he allowed for the possibility that classical mathematics be interpretable. Retrospectively, he noted that if an interpretation is given "this interpretation does not enter into the syntactical rules of L".\textsuperscript{52} Here Carnap remained carefully ambiguous with hindsight. Obviously, what was allowed was the addition of a semantical rule system to the syntactic one. Yet Carnap's formulation also leaves room for what seems to have been his understanding of the relation between what he then distinguished as semantical and syntactical concepts during his syntactic period: that form and at least one kind of meaning (cognitive meaning) were two aspects of the same thing. There was a kind of meaning whose specification did not require the additio.n of any rules over and above those specified by syntax.\textsuperscript{53} To understand Carnap' syntacticism, it must be noted that its non-adversion to meaning was understood as a non-adversion to meaning-as-such, namely, as meaning over and above syntax: there remained notions of meaning accessible in syntactic terms, namely, that of the sameness of the "content" of statements defined in terms of the equipollence of their non-analytic consequences.\textsuperscript{54} Putting together his view of the formal nature of analyticity and his view that form is meaning it must be concluded that Carnap did not do ordinary extensional semantics as syntax--he did a kind of emaciated intensional semantics as syntax! The meaning he claimed to have reconstructed was that determinative of analytic truth. Clearly then, extension was "covered" by his formal approach to just that degree to which talk of intension "covered" the same territory: given only intensional

\textsuperscript{52}Carnap 1963a:928.
\textsuperscript{53}E.g. "equipollence", see Carnap 1934/7:42.
\textsuperscript{54}Carnap 1934/7:42, 1934d:11.
notions one can fix the concept of the sameness of the extension of two expressions even though questions of the factual truth of their application must perforce remain undecided. However scandalous this view of Carnap's may strike us nowadays, it was not yet the end of the matter, as I shall show in the next section.

6.3.3 The Tenet of Descriptive Syntax that Ostensive Definitions are Syntactic

Carnap did not only require recourse to unreconstructed "higher syntax" to retain the "formal" character of analyticity, and so achieve the logicist ambitions through the application of the formalist method. Carnap's hidden reliance on semantic notions also had stunning consequences for descriptive syntax, the kind of syntactic inquiry that dealt with the language of the empirical sciences. These consequences are of outmost importance to the understanding of stage two of the protocol sentence debate. Carnap's syntactic reasoning did not preclude reasoning about meaning. The conception of "formalist meaning" outlined in the previous section followed straight from his conception of analyticity as a formal syntactic notion. Very importantly, however, there was in play also a yet different aspect of "translation" which had a direct bearing on the pursuit of descriptive syntax. Herein lies Carnap's specific problem with descriptive syntax. Carnap possessed two ways of reasoning about meaning in descriptive syntax: one which continued to broadly rely on the type of reduction familiar from the Aufbau, and one which was "physicalistic"; one each, in other words, for the materialistic reduction of the concepts of science and one for the phenomenalist epistemological reduction.

How reductionist, phenomenalist meaning theoretic questions figured in descriptive syntax was made clear by Carnap's characterisation of the task of descriptive syntax in his Erkenntnis papers of 1932. As outlined there, "logical syntax" or "metalogic" was to provide philosophy with its distinct method of logical analysis for eliminating pseudo-
statements and laying logical foundations. Metalogic qua "applied logic" was to serve as epistemology. It was to do so by providing a syntactic reworking of the Aufbau's theory of constitution. In so far as it comprised studies of the deducability of sentences from others, metalogic was deemed equivalent to studies of the truth, the verification procedures, and the meaning of sentences. Given Carnap's identification of truth-conditions, truth-criteria and the meaning of sentences, metaphysical sentences were easily shown to be meaningless: even if they were not strictly speaking ill-formed sentences—a common failing for metaphysical assertions, so Carnap—they still lacked a deductive relation to the class of elementary sentences.

The reductive picture of the meaning of empirical sentences developed in the Aufbau was here presupposed within the new syntactic framework. Carnap presupposed that syntax could distinguish between elementary and non-elementary sentences. How could it do so? The Aufbau's elementary sentences contained only the basic relation symbol as a descriptive term. In order to eliminate metaphysics, metalogic thus had to comprise accounts of both (i) the strictly formal rules of the formation and transformation of sentences and (ii) of the delimitation of a class of elementary sentences with respect to which other non-analytic sentences were deemed admissible or inadmissible. This class of elementary sentences was of course that of protocol sentences. In so far as the unity of science thesis was defended against the metaphysicians—and was so defended epistemologically on "syntactic" grounds—the picture of the unity of science presented by Carnap's meta-logic continued to be that all meaningful empirical discourse concerned "the given".

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55Carnap 1932a:62,75,77.
56Carnap 1932b:E433.
57Carnap 1932a:62; see also section 8.3.2.
58The same conflation is in evidence in Schlick 1932b:181 and in Hempel 1935c.
That such a phenomenalistic, epistemological account did not handle well the knowledge acquisition in scientific practice, Neider and Neurath had objected already back in 1929. It thus became imperative for Carnap to deal with the other, the "materialistic" derivation of scientific concepts, that is, to demonstrate the unity of science without reliance on the phenomenalist reduction of scientific concepts. Carnap had to be able to cash in talk of sameness of content (equipollence) without recourse to what's "immediately given". This he attempted to do in his Warsaw-lecture-turned-Erkenntnis-paper on the language of psychology and its complementary Erkenntnis-paper-turned-translated-monograph on the language of science in general.\textsuperscript{59} There he combined his syntactic approach with the thesis of physicalism and set out to provide a "materialistic" interpretation of the elementary sentences. Next, I first show Carnap's "materialistic" understanding of the tenet of descriptive syntax that form is meaning and then point out that Neurath's adoption of Carnap's formalist approach did not need to rely upon Carnap's (as it turned out) pseudo-materialism.

The important question at issue here was this: could the tenet that form is meaning be retained in the absence of a phenomenalist reduction not only for the languages of the formal sciences, with the explication of which Logische Syntax was concerned, but also for the languages of the empirical sciences? In the Aufbau conception, such a thesis made sense: there Carnap had explicated the logical form of empirical content by reduction to the given. (Now, it just so happened that Carnap himself continued to hold on to the possibility of a phenomenalist reduction of the physicalist language, as I shall show in the next chapter.\textsuperscript{60} But this need not concern us now.) Does this mean then that the view that form is meaning depended on the retention of phenomenalism? If it did, Neurath would

\textsuperscript{59}Carnap 1932c, 1932b respectively; see appendix to chapter 2.

\textsuperscript{60}Coffa 1976 voiced the same suspicion. My judgement is supported by Carnap's continued endorsement of the intertranslatability thesis in his 1934/7 and his insistence on the symmetry of the effect of only non-eliminative reduction sentences on both the phenomenal and the physical language in his 1936b and 1936/7. See below sections 7.3.1, 7.4, 7.7, 8.2.2, 8.3.3, 9.6.
not have been able to follow Carnap on this point because of his militant anti-
phenomenalism. But Neurath suggested that the structural/formal approach to the
validation of empirical knowledge be retained. So how was the structuralist methodology
to be pursued in the absence of a phenomenalistic reduction?

As of 1932, Carnap's answer to the question "what is the meaning of a word?"
consisted in two determinations: what must be fixed were, first, "the model of its
occurrence in the simplest sentence form in which it is capable of occurring", and,
secondly, the conditions of its "deducibility" from protocol sentences, "its criterion of
application". But Carnap went further yet. Carnap did not only recognise reductive
definition as permissable forms of definition in meta-logic but also admitted interlinguistic
translations, non-reductive "nominal" definitions, and even ostensive definitions.

In order to characterise a definite [particular] language it is necessary to give
its vocabulary and syntax; i.e. the words which occur in it and the rules in
accordance with which (1) sentences can be formed from these words and
(2) such sentences can be transformed into other sentences, either of the
same or of another language (the so-called rules of inference and rules for
translation). But is it not also necessary in order to understand the 'sense'
of the sentences to indicate the meaning of these words? No; the demand
thereby made in the material mode is satisfied by specifying the formal rules
which constitute its syntax. For the 'meaning' of a word is either given by
translation or by definition. A translation is a rule for transforming a word
from one language to another (e.g. 'cheval' = 'horse'); a definition is a rule
for mutual transformation of words in the same language. This is true both
of so-called nominal definitions (e.g. 'elephant' = animal with such and
such distinguishing characteristics) and also for so-called ostensive
definitions (e.g. 'elephant' = animal of the same kind as the animal in this or
that portion in space-time); both definitions are translations of words.62

Inasmuch as Carnap's meta-logic specified linguistic frameworks it had to specify both the
syntax and the vocabulary of a language - and thus "so-called ostensive definitions" fell
under its purview. Clearly then, Carnap had "found" a way of talking of meaning without

61 Carnap 1932a:62/3.
62 Carnap 1932b:38/9; my italics. Hanfling 1981a:88 notes (with Ayer) the strangeness of this "ostensive"
definition from an implicitly Schlickian point of view. (Compare Schlick's own comments in his
1936:354.) Neither Hanfling nor Ayer, however, explored the systematic gains Carnap thought to derive
from this peculiarity.
relying on the phenomenalist reduction: with ostensive definitions comprehended as "formal" the materialistic derivation could be effected in the syntactic mode.

It was this non-reductive aspect of Carnap's syntactic method of dealing with meaning that Neurath could focus upon when he adopted Carnap's view that form is meaning. Clearly, such definitions would hardly be called formal or structural today: to know the meaning of 'elephant' we have to know what animal was at such and such a point of space-time. That is more than a matter of the form of representation. For Carnap, however, the mere possibility of providing "translations" of words by their ostensive definitions served to support the tenet that form is meaning in virtue of their logical equivalence. With criteria of application, translations and ostensive definitions considered encompassed amongst what the formal mode of speech permitted to be specified, Neurath could happily accept Carnap's syntactic meta-linguistic methodology⁶³: no longer did a purely structural view of meaning require a phenomenal reduction. The belief that form is meaning thus remained viable even in the absence of a phenomenalist language into which physicalistic statements could be translated, given the "resources" of Carnap's descriptive syntax!

6.4 The Syntactic Turn Evaluated

Before I return to the protocol sentence debate proper, let me summarily evaluate the success of Carnap's syntactic turn and Neurath's adoption of it.

6.4.1 Evaluation of Carnap's Logical Syntax

Carnap's project failed. The early flowering of the syntactic turn betrayed expectations of Carnap's which the actual development of the syntactic program did not,
after all, bear out. Carnap once hopefully claimed that "the metalogic of elementary number theory is formulatable in sentences of the elementary number theory". Yet even Carnap's amended definition of analyticity was not up to Carnap's billing: it required a language of higher expressive power than the language of elementary arithmetic itself. What is nowadays reckoned his failure to comprehend classical mathematics purely formally only repeated this earlier mistake. In *Logical Syntax* Carnap concluded that "everything mathematical can be formalised" while adding "but mathematics cannot be exhausted by one system; it requires an infinite series of ever richer languages." That this rider deprived Carnap's assertion of all force was noted between the lines of Quine's conclusions of his "proto-syntactical" investigations: "Logical truth, like syntactical truth, is syntactically indefinable. Logical truth may [...] be said to be informal." That his conception of mathematics as purely formal required that formality be explicaded semantically was the conclusion Carnap later drew from Tarski's theory of truth.

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64 Carnap 1932a:E238.

65 In order to show this in detail it may be helpful to distinguish three substages of Carnap's syntactic period. The first extended until mid-1931: by then Carnap had specified his project for a metalogic as far as the language I of his later *Logische Syntax*, the language of elementary number theory as acceptable to finitists (Carnap 1963a, 1934/7:83). The second substage covered Carnap's elaboration of the metalogic for the language II and the general syntax of *Logische Syntax*. The third substage apparently concerned the supplementation and revision of his intermediate draft (read by Behmann and Goedel) for the final version. Some of these were rejected by the publisher of the German original (1934a) for reasons of "lack of space" and published separately as 1934c and 1935b and incorporated into the English translation of 1937 (Carnap 1934/7:x, xvi). The second version of *Logical Syntax* ran into difficulty with the definition of the consequence relation for the language II of classical arithmetic which allowed for infinite orders of type (compare Wang 1988 and Coffa 1987). The difficulty of comprehending the completeness of such languages in purely syntactical terms also was what--independently of Carnap's moves--led Tarski to abandon his "morphological" approach to the definition of truth after learning of Goedel's incompleteness results. Just exactly how this problem was to be solved may not have been fully clear to Carnap at the time the German original of *Logical Syntax* went to press. I am inclined to think that the versions of paragraphs 34, 38, 60 and 71 of Carnap 1934a constitute early rewrites of passages from the second substage that were criticised by Goedel and/or Behmann: they constitute (partly) first repairs to the version of the second substage. The proposal to use "evaluations" (presented in paragraph 34c of Carnap 1934/7) was only hinted at--and not under this name--in the broadest possible terms (Carnap 1934a:99) and first published in his 1935b which was received for publication six months after 1934a had gone to press (Carnap 1934a:vii, 1935b:190).

66 Carnap 1934/7:222. (With reference to the remarks of my previous footnote, compare to this Carnap's similar remark in 1935c and the absence of an equivalent statement in 1934a:par.34.)

67 Quine 1940:318.

68 Carnap 1943:introduction.
his conception could not be so defended of course constituted Quine's plaint against Carnap from the late 1930's onwards.69)

So much for the failure of Carnap's main objective in Logical Syntax. But did Carnap at least solve the structuralisation problem left unsolved after the Aufbau? This depended first of all, of course, on the success of what held the main interest for Carnap in his Logical Syntax, the complete structuralisation of the languages of the formal sciences. Since that failed, the repair to the Aufbau failed as well. It was a purely formal description which was to give an analytic characterisation of linguistic frameworks. That, of course, the formalist method of Logical Syntax did not achieve, and so repeated the structuralist failure of the Aufbau. Carnap's formalisation continued to rely, as I showed70, on the silent presupposition that the "formal" concepts analysis carried some intended meaning. Finally, Carnap's illegitimate assimilation of ostensive definitions to the class of purely formal specifications of linguistic forms rendered his further efforts towards a descriptive syntax for the languages of empirical science even more problematical.

Still, apart from these retrospective criticisms, did Carnap's logical syntax solve the "unresolved" problem of the Aufbau at least by Carnap's own lights? Supposing pure syntax successful, as Carnap thought it was, was descriptive syntax able to show the applicability of the scientific system of concepts by purely formal means? To answer this question, we must consider the changes in Carnap's strategy for legitimising scientific concepts since the Aufbau.

Carnap's first change of strategy consisted in structuralising not "the given" but the language of science. It may seem at first glance that it was no longer necessary to exorcise even the last descriptive notion, the basic relation of remembered similarity: the new

69Quine 1939, etc. On why this opposition should not be dated already to Quine's yet earlier "Truth by Convention" (1936) see Creath 1987a.
70See section 6.3.1 above.
method left intact the descriptive character of the constants of the object language, namely those of the empirical sciences, while describing them in structural metalinguistic terms. Against this view, it can be objected that Carnap did not render the significance of all terms of the scientific language itself independent of any reliance upon intuition. That all first-order terms could be structurally described in the meta-language did not render them, like logico-mathematical terms, purely structurally meaningful in the first-order language. The intuitive content of the basic relation thus still needed to be exorcised if all meaning really was only matter of structure. However, it can be argued that Carnap had come to change the nature of his structuralisation problem. "Intuitive" terms were admissible into the language of science once their meaning was fully explicable in structural terms in the meta-language alone. Read in this way, the problem of the Aufbau seems overcome (supposing pure syntax successful).

One further objection still remained, however. The Aufbau's problem was not only that of generally explicating the meaning of all terms purely structurally, but also of indicating that they uniquely identified their domain, of showing that empirical terms were really structurally definite descriptions. Now, in his syntactic period Carnap offered two derivations of concepts, a materialistic and a phenomenalistic one. The old Aufbau problem only applied to the latter. Concerning the former, the problem did not arise with the same urgency. The explication as definite description was not necessary for physicalist terms to be meaningful; this problem was only acute for the project of completely structuralising or formalising the phenomenalist language. Thus Carnap could admit about the physicalistic language that no demonstration of its applicability to reality could be given.71 Carnap no longer thought it obligatory (as he still did in 1928) to show that a language structurally described did in fact apply to reality. But did this reasoning also apply to the phenomenalist language? Carnap did not say so explicitly. We must conclude

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71 For more on the exchange between Carnap and Zilsel see below section 8.2.1.
that if he simply ruled out the problematical demand to demonstrate the applicability of a conceptual system as mistaken, then, in so far as for epistemological justification the Aufbau model still remained in force, the basic relation problem remained unresolved.\footnote{For discussion of the in fact related problem of distinguishing between protocol and other factual statements see below section 8.2.2.} The objection thus remained that the question whether a language syntactically described could uniquely identify its domain, once the basic relation was stripped of its intuitive content, had to be answered negatively, as long as Carnap did not re-recur to the condition of "foundedness".

That Carnap no longer seemed concerned with the question of the applicability of a language (now "syntactically" described) to reality, of course, commonly lead to the to the view that in Carnap's syntacticism no questions of meaning whatsoever could be addressed. Such a view is mistaken, however encouraged it may have been by Carnap's own rejection of questions of reference as incompatible with the syntactic approach.\footnote{Carnap 1934/7:288-292.} Carnap's position, properly understood, followed from the rejection of Schlick's view of the correspondence relation as involving the sharing of logical form between statement and fact. Since prior to his acquaintance with Tarski's theory of truth, he saw no way of speaking about a language-world relation without assuming a standpoint outside of both, Carnap simply dropped all such reference and correspondence talk. In the face of these restrictions with which he saw theorising of meaning confronted, Carnap's assimilation of ostensive definitions to definitions by translation in fact represents an ill-fated attempt to break through the limitations and encompass as much of reference talk in the "syntactic" formal mode as possible. As I noted, Carnap's syntax language was intended as a regular meta-language, it did not talk of the world, but of how the world was represented. What alone rendered the formal mode less than fully meta-linguistic in the modern sense, namely, also able to address semantic matters, was its abstinence on the matters of reference and
truth on account of the troublesome accounts of correspondence then available. So, if the Aufbau's problem is understood to be that of whether the meaning of statements can be completely formalised (in the sense of "formal" acceptable nowadays, namely, excluding issues of meaning), then the answer is that Carnap's Aufbau problem remained unresolved until his semantic turn, when language world-relations could once again be topicalised.

By 1935 Carnap had come to distinguish questions of form from questions of meaning in the fashion of Tarski's semantics. During his syntactic period, Carnap thought he had developed his logical syntax entirely along Tarski's suggestions of 1930. After their meetings in Vienna and Warsaw, however, Tarski had come to abandon the conception of a strictly syntactic inquiry into language. This came as something of a shock to Carnap when they met again and Tarski reported the result of his dissertation to him. As we saw, Carnap had thought of logical truth as a purely formal syntactic notion. (Factual truth was not, of course, deemed a syntactic notion: it concerned not only the meaning of terms but also actual affairs in the world. What rendered semantics so attractive to Carnap was, after all, its ability to accommodate just such "factual" considerations.) Much of the semantic method, despite appearances to the contrary, was already anticipated by his illegitimately inflated syntactic method: Carnap's "formal" definition of "analytic-in-L" in effect made use of the semantic method of meta-linguistic ascent. Carnap's problematic view of analyticity as formal in fact allowed for his relatively smooth procession from the syntactic to the semantic method - smoother, in any case, than commonly realised thanks to employing in the meta-language predicates of a higher order

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74 See below section 8.3.1.
75 As is evident from his 1932 and 1933/5, Tarski himself was first attracted to the idea of explicating 'true' in "morphological", i.e. syntactic terms. It was the study of Goedel's incompleteness results which brought home to him the fact that the distinction between object- and meta-language (such that the latter was richer in predicate types than the former) amounted to a principled one which set syntactic and semantic inquiries categorically apart. (Just this realisation took Carnap somewhat longer--until his next meeting with Tarski, it seems--and it is its long incubation period which accounts for the peculiarity of his Logical Syntax.)
76 Carnap 1934/7: 216.
77 Carnap 1963a: 60/1. More on this below; see section 8.3.1.
than were found in the object language. Most of the logical strength of the semantic method was thus already at hand in Carnap's supposedly syntactic method. To be sure, this was still decidedly less smooth than Carnap presented it in retrospective: "one of the main theses was formulated too narrowly." That philosophical problems were not only syntactical, but also semantical problems constituted not only a broadening of Carnap's conception of what were legitimate philosophical problems, but also an outright contradiction of his earlier views that semantical questions not answerable in syntactic terms were strictly meaningless. Carnap's 1934 attempt to show all philosophy to reduce to logical syntax had failed.

Finally, I must note that a significant problem emerges from the non-standardness of Carnap's "formality" (even if we stay with his picture of things) for his alternative neo-Kantian project. The notion of form as the guarantor of intersubjectivity is undermined if it again depends on an unspoken intention to use words in a certain way. Such an intention can be shared as a communal convention, to be sure, but to admit this reliance of "formal analyses" on intentions (shared or otherwise) is simply to admit that the notion of form cannot explain everything about (even only) theoretical (non-psychological) meaning. It can present a framework, but that framework itself need be seen in context and that context can apparently not be formalised. In this respect then, Carnap's Logical Syntax ran into the same kind of problem as his Aufbau did: a residual reliance on intended meaning which not only resisted formalisation but in fact was crucially relied on for the formalisation to "work". Once again, only the recognition of semantics was to save Carnap's project, his alternative neo-Kantianism, if it was to be saved at all.

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79 Carn. 1963a:56.
80 This was first stated explicitly in Kokoszynska 1936a:161.
81 On the Aufbau's similar problem see section 3.4.2 above.
82 See below section 10.3.
6.4.2 Neurath's Adoption of the Syntactic Turn: Meta-linguistic Investigations in the Framework of Physicalism

What then was Neurath's reception of Carnap's inflated syntactical method? His happy endorsement of the program for logical syntax suggests that he was enthusiastic about the method of investigating any and all languages structurally. I now consider how his understanding of the project differed from Carnap's.

Consider Neurath's endorsement of the conception of the structural nature of cognitive meaning as developed by Hilbert, Schlick and Carnap.

In language nothing is essential than what is already represented by a sequence of signs of Morse code. 'Intersubjective' and 'intersensual' language in general depends on order ('next to', 'between', etc.) that is, on what can be expressed by sign sequences in logic and mathematics. All predictions are formulated in this language.\(^{83}\)

Nothing essential to this language in which empirical predictions are formulated could not also be expressed in terms of logical structural relations. Like Carnap, Neurath was concerned with the structural relations of language, not of "the given", and with any of the physicalistic languages of science, not the \(\text{ur}\)-language.

Neurath was particularly enthusiastic about Carnap's descriptive syntax, for it constituted the tool for the regimentation of the unity of science (as far as it could be so regimented). Needless to say, what holds as far as the modern criticisms of the failure of Carnap's syntactic structuralisation of meta-linguistic discourse is concerned, also holds for this aspect of Neurath's theorising. As far as the abstinence from talk of world-language relations was concerned, however, Neurath pursued a different strategy from Carnap's. Here Neurath's difference from Carnap's view about the language of scientific protocols rendered Neurath's adoption of Carnap's view that form is meaning less arcane than Carnap's own. First, it did not depend on the phenomenal reduction of descriptive terms: \(^{83}\)

\(^{83}\) Neurath 1932a:62; translation amended. Compare his 1931a:49.
Neurath continued to reject Carnap's idea of the need for a phenomenalistic epistemological reduction.\(^{84}\) Second, Neurath did not follow Carnap's, but presented a different strategy for dealing with questions of meaning in a non-reductive fashion. I showed in the previous chapter that Neurath attacked Carnap's methodological solipsism from 1928 onwards and in 1929 explicitly embraced materialism. Consequently, Neurath held that the fundamental sentences of sciences, with reference to which hypotheses were confirmed and rejected, did not speak of the phenomenal given, but of states of affairs in the spatio-temporal world. How Neurath argued for this is the topic of the next chapter; here I shall only show how Neurath's "syntactic" methodology varied from Carnap's as a result.

For Neurath, meta-linguistic discourse was no less physicalist than the first-order discourse of science. "Unified science based on physicalism recognises only statements with spatio-temporal data."\(^{85}\) This pronouncement of Neurath's must be considered carefully. Did he assert that only the space-time locations of physical objects could be topicalised in the language of unified science? That would be to read him most uncharitably, and bar the understanding of his approach to "syntactic" theorising. For Neurath, the statements of unified science spoke only of what can be spoken about in physicalistic terms—and all of that.\(^{86}\) Note also that "physicalistic" did not mean the strict language of physics, it meant talk "about the spatio-temporal order, such that predictions about controllable events become possible. Empirical mechanics and empirical sociology are factual sciences of the same kind..."\(^{87}\) "Physicalistic" descriptions included "behaviourist" descriptions\(^{88}\) which were not understood in Watson's eliminative sense,

\(^{84}\)See section 5.2.2, 5.4.4, 5.5.4 above and chapter 7 below.
\(^{85}\)Neurath 1931b:55.
\(^{86}\)Note the quote below from Neurath 1931b:55, which speaks of a ball being "electrically charged".
\(^{87}\)Neurath 1931e:531.
\(^{88}\)Neurath 1932a:61.
but comprehended all psychological talk that did not suppose its subject to be non-material or "spiritual". Now let's see what Neurath made of all this.

First of all, the spatio-temporal data at issue included structural relations. Structural relations are relations of order, i.e. serial order; they themselves could thus become the objects of a suitable scientific inquiry. The sentences of the one part of language which speak about sentences of the other (and the same) part of the language of unified science, speak of these latter sentences' "structure[s] as physicalistic ordering". This meta-linguistic inquiry proceeded within the following enlarged conception of the language of unified science: the physicalistic language of unified science had various parts. The language of natural science is one part of the language of unified science and the syntax language is another. Both, however, speak of spatio-temporal objects or events.

Against Neurath, Carnap once pointed out that "only the structural patterns, not the physical properties of the ink marks, were relevant to for the function of language." Neurath presumably accepted this point. His conception of physicalism was wide enough to encompass investigations of abstract linguistic structures. Neurath's physicalism allowed for the study of abstract types under its banner if the following condition was fulfilled: it must be possible to see tokens of these abstract types as instantiated in physical processes, say speech behaviour. Any talk of language was physicalistically acceptable as long as that language was physically describable or instantiatable and thus related to possible physical structures, just as any talk of thoughts was acceptable as long as it was translatable into conceivable behavioural dispositions and talk of specific scientific

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89 Neurath stressed that again; see particularly his 1933 which, while rejecting some of their claims, sought to exhibit the physicalistic content of different psychologies like Gestalt-theory and psychoanalysis, for example. Neurath 1936d:714 even envisaged talk of people's "plans" and "intentions" to be possible without falling into metaphysical traps. See also below sect. 7.3.2

90 Carnap 1963a:29.

91 Such a condition was consistent with Carnap's acceptance of logicism which also rendered the abstractness of mathematics harmless and integratable into the framework of physicalism.
statements was acceptable as long as the conditions of their assertion had been specified.92 For Neurath then, talk of language in what Carnap called the "formal mode" was consistent with his physicalist requirement to talk only about spatio-temporal objects and events: "[t]he study of language can perfectly well be combined with the study of physical processes; for one always stays in the same field."93 The formal, meta-linguistic mode of speech allowed talk of language as one physicalistic phenomenon amongst others. In parallel to his objections to an independent "Geistewissenschaft" dealing with affairs of the spirit, Neurath merely discouraged any attempt to consider language in non-physicalistic terms (where "physicalistic" is given his wide interpretation).

It is important to note that within this physicalistic framework, Neurath hit upon and exploited a way of speaking of language-world relations which did not violate Carnap's strictures against speaking about language in the "material" mode. Talk in Carnap's formal mode was, as noted, also talk of the meaning of a language insofar as that was capturable in translations employing reductive and ostensive definitions. (What cognitive meaning couldn't be so captured?) To the extent that for Carnap translations were a formal matter, talk of form was equivalent to talk of meaning. Nevertheless, Carnap abstained from talk of reference and truth. But was it impossible in principle that any reference to language-world relations was made in the "formal" mode? Here it all depended on how such relations were theorised about. Thus note Neurath's remark about the qualitative terms appearing in non-phenomenalistic protocol statements:

The words 'blue' or 'hard' or 'shrieking' are then used in a physical sense alone. They either indicate that a man shows a certain behaviour under certain conditions, that he speaks words or exhibits nervous changes ('field statements'), just as, for example, a test body in the neighbourhood of a charged ball in some experiment; or they indicate that there is a certain oscillation somewhere.94

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92 Neurath considered reasoning about possibility to have a place in scientific reasoning from early on. See, e.g. his 1910 and 1917.
93 Neurath 1931b:53.
94 Ibid.:55.
Neurath noted here not only that the fact that language was intersubjective meant that the language of scientific protocols had to be the physicalistic language. He also noted how, in the "syntactic" fashion, what Carnap had called the correlation problem of the designation relation of the scientific language was to be investigated: physicalistically. Signs and sign systems were to be considered as themselves physical objects whose relations to others could be talked about. Contrary to common presumption language-world relations could be topicalised in Neurath's physicalism! Neurath, in fact, worked with a conception of meaning as correlated information.

Like Carnap's, Neurath's conception of the meaningfulness of linguistic expressions required no relation of correspondence in the Schlick-Wittgenstein sense. Instead, language-world relations could be captured in tcrrm of the two relations termed by Carnap in the **Aufbau** the "expression relation" and the "designation relation".\(^{95}\) Under the former "expressive" aspect, observational predicates and statements bespeak-- qua dispositional predicates/statements (Neurath's "field statements")\(^{96}\)--the utterer's states as a physical object. Under the latter "designative" aspect observational predicates and statements bespeak what they are correlated with, states of the world. Conceived as a structurally defined symbol system then, language can be investigated physicalistically as one of the relata of the expression relation as well as of the designation relation. (These relations themselves are structural, they are correlations of objects. Structured objects then stand in structural relations to other structured objects.) Both the expression relation and the designation relation of language could be, as they were here, rendered in physicalistical language.\(^{97}\) The study of language thus was integrated into the framework of the study of the world and language was to be studied as a "this-worldly" phenomenon.

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\(^{95}\)Carnap 1928a.

\(^{96}\)Neurath 1932a.

\(^{97}\)Carnap did not exploit this avenue of talk of meaning yet neither did he bar it: his account of language learning in his reply to Zilsel in fact presupposes it (Carnap 1932d: ).
Note that, for Neurath, it was not only talk of language that was rendered possible and physicalistically acceptable, but also talk of thinking.

We speak not of 'thinking' but straight away of 'speech-thinking', that is, of statements as physical events. [...] All too often the discussion proceeds as if, through refutation of any individual claims made by behaviourists, the principle were somehow affected that only physicalist statements have a meaning, that is, can become part of unified science.98

Without endorsing any one particular behaviourist psychological theory--while, in fact, distancing himself from Watson's specific version of behaviourism--Neurath endorsed a broadly behaviourist program. In the light of Carnap's concurrent efforts it is clear that that Neurath here endorsed the position known as philosophical behaviourism: without requiring the elimination of psychological terms it was held that the attribution of psychological states was equivalent to statements about their bearers' behaviour.99

Neurath's philosophical behaviourism was no mere after-thought to his physicalist outlook but was integral to his project of the physicalisation of metalinguistic discourse. For Neurath, even meta-linguistic terms had to be defined physicalistically. "'Equivalent statements' are constructed physicalistically; for statements are physicalistic structures, written or spoken words."100 The physicalistic definition of the equivalence of two statements was to be effected behaviouristically:

We expose a man to a system of commands that are connected with all sorts of statements. For example: "If A behaves in such and such a way, do this!" We can now fix certain conditions and then observe that the addition of a certain statement causes the same change of response as that of another statement. We will then say that that the first statement is equivalent to the second. By the addition of tautologies the stimulus of the system of commands remains unchanged.101

98Neurath 1932a:67; his italics.
99Carnap 1932c; see below sect. 7.3.2.
100Neurath 1931b:55.
Neurath suggested that his proposals for the "physicalisation" of metalinguistic discourse be empirically tested with the help of a Jevons thinking machine.\(^{102}\)

Now the question: was Neurath any better off than Carnap for his physicalisation of meta-linguistic discourse? Here I would like to withhold judgement for the moment. Instead, I'd like to note that Neurath's strategy opened this possibility: language conceived of structurally-physically in his fashion allowed for meaning to be theorised about as a property of behaviour. Since meaningful behaviours can be individuated only with reference to the context within which they occur the possibility opened by Neurath's strategy is this: meaning is not constituted by form alone but by form in context; it is a property of behaviour in context. This possibility of thinking about language and meaning, of course, turned on this difference between Neurath's and Carnap's syntactic turns, however, and thus pertained to what the debate between the two focussed on: whereas for Carnap (and Schlick) meaning consisted in the structure of an essentially private vehicle of cognition (mental representations), for Neurath it consisted in the structure of a public medium (language). The discernment of such a structure was thus an equally public affair. This difference had profound consequences, as I shall show in the next chapter.

Now admittedly, all this talk of "meaningful behaviour" is, of course, terribly un-Neurathian. Indeed, one might think that my reading flies in the face of Neurath's proud claim that "I myself do not use the term 'meaning', nor a substitute for it, ..."\(^{103}\) The reason for Neurath's animadversion to talk of, say, meaningful behaviour in the fashion of Max Weber's interpretative sociology, which was, for him "hampered by metaphysical

\(^{102}\) Some 33 years later, Carnap outlined a similar experimental procedure in the course of defending his conception of intension in natural languages. This move of Carnap's in his 1955a has always struck me as one of the earliest moves towards functionalism in the philosophy of mind. The comparison between brains and Jevons thinking machines-- without deriving any operational suggestions from it--had earlier been drawn by Schlick in his 1918/25:146.

\(^{103}\) Neurath 1945/6:241. Note that the sentence goes on: "in a context which would allow the application of this remark made by Kallen on my writings", namely, ""Crossing logistics with a pragmatic theory of meaning"". Here, it seems to me, Neurath reacted to Carnap's by then fully developed semantic theorising which he never accepted, for, it seems ever so broadly Quinean reasons. See also sect. 10.3.
formulations": sociology studies only correlations between physicalist entities. Neurath's desired to set scientific philosophy as far apart from what he called academic "metaphysics" as possible: proponents of the latter extolled concern with meaning and meaningful behaviour as the province of an anti-physicalist "Geisteswissenschaft". But Neurath's polemics must not mislead us. Compare, for example: "Logic and mathematics show us which linguistic transformations are possible without adding to meaning, however we may formulate this fact." Or: "The claim that these expressions [of theology: 'earthly' and 'unearthly'] are as meaningful as those of science, pronounces open conflict." Clearly, matters of meaning were not dismissed as "meaningless" here! That and how concern with meaning figured not only in Carnap's syntacticism, but also in Neurath's own theorising I have demonstrated in this chapter.

6.5 Summary

To recapitulate the main points. First, Neurath's 1931/2 remarks about languages talking about themselves followed Carnap's conception of his logical syntax. Neurath's theorising about the language of science since then must accordingly be read as incorporating the general conception of logical syntax. Second, by today's standards, Carnap's syntactic method was flawed by its definition of analyticity as formal; furthermore, the open-endedness of Carnap's notion of "formal" undermined his alternative neo-Kantian aim by presupposing an intended interpretation. (Only once semantics was recognised as an additional formal dimension was this defect repaired.) Third, Carnap's syntactic theorising did not bar theorising about meaning. Carnap endorsed a view according to which form was meaning, and Neurath one according to which form-in-context was meaning. Fourth, Carnap's syntactic understanding of empirical meaning was

104 Neurath 1932a:83. Neurath rejected Weber's attribution of causal efficacy to something like "the spirit" of Protestantism etc.
105 Neurath 1932a:60 and 70; my italics.
defective in as much as ostensive definitions were essentially relied on. By contrast,
Neurath pursued an alternative method of determining meaning as correlated information.
Chapter 7

STAGE TWO:
NEURATH'S NATURALISTIC CHALLENGE RADICALISED

Abstract: Overview of stage two (7.1). Neurath's physicalist charge of 1931 against Carnap's methodological solipsism (7.2). Carnap's intermediate response of early 1932 (7.3). Results of the first substage of stage two (7.4). Neurath's restatement of his objections (7.5). Carnap's final response to Neurath of late 1932 (7.6). Differences which still divided Carnap and Neurath at the end of stage two (7.7).

The previous chapter explored the (compared to the Aufbau; changed) philosophical background which Carnap and Neurath shared in their attack on the Tractarian wing of the Circle and showed that, despite appearances to the contrary, reasoning about language in the "syntactic" framework did not preclude theorists from reasoning about meaning: meaning-theoretic considerations were instead assimilated to formal considerations. I now return to the debate between Carnap and Neurath over the tenability of methodological solipsism.

7.1 Overview of Stage Two

In November 1930 Carnap gave several lectures in Warsaw in which he first put forth his syntactic approach and specified the thesis of physicalism by applying it to the discipline of psychology.¹ For Carnap, the thesis of physicalism said that the physical language was the universal language of science: all the languages of the various special sciences were thought translatable without cognitive residue into the language of physics.

¹Amongst them was what became Carnap 1932c; compare Benson 1963:1023/4.
In many places Carnap credited Neurath with providing the impetus for the detailed
development of this thesis. But was this all that Neurath had in mind in championing
physicalism?

Neurath was not satisfied by Carnap’s reading of the thesis. Carnap believed that
two different derivations of concepts were needed. The language of physics and science in
general needed a materialistic and epistemology needed a phenomenalistic (positivist)
derivation of concepts. This two-pronged strategy required Carnap to show the auto-
psychological language to be part of the universal physicalistic language of science, in other
words, to rework the intertranslatability thesis of the Aufbau. Neurath not only was not
impressed by Carnap’s efforts to do so, he outright disputed the need for—even further, the
applicability of—the positivist system for epistemology. (The exact sense of this
"inapplicability" will be specified below.) With the materialist system shown, by Neider’s
argument, to be the only one to provide for practical intersubjectivity, Carnap’s presumed
need for another system exhibiting a phenomenalistic order of epistemic priority became not
only redundant but outright fallacious.

Carnap rightly attributed Neurath’s championship of the unity of science thesis to
his rejection of the then common views of a radical difference between the natural and the
moral or cultural sciences ("Geisteswissenschaften"); Carnap supported him here on
theoretical grounds. But Carnap also reported Neurath’s early preference for materialism
and later support of physicalism as springing from his "pragmatic-political", namely
Marxist orientation; this form of argument Carnap did not support. Now, while Carnap’s

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3 See above section 5.4 and Appendix to ch. 5.
4 Carnap 1963a:23/4. Carnap's portrait is widely accepted; see e.g. Creath 1987b:474. Note that I am here
concerned with claims about the relation between politics and philosophy. Carnap wrote: "All of us in
the Circle were strongly interested in social and political progress. Most of us, myself included were
socialists. But we liked to keep our work separated from our political aims. [...] Neurath strongly
criticised this neutralist attitude [...] We in turn insisted that the intrusion of practical and especially of
political points of view would violate the purity of philosophical methods." Such disagreement is

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characterisation is in many ways correct it also misleading insofar as it discounts the philosophical arguments Neurath gave for his position. For Carnap, the physicalist language had amongst its "advantages"\(^5\) that it is already intersubjectively intelligible. For Neurath, by contrast, the physicalistic language was not just something to which we should give a "strong preference"\(^6\); rather, it was simply indispensable. The present chapter reconstructs Neurath's often overlooked arguments to the conclusion that methodological solipsism is inevitably untenable.

The second stage of Neurath's and Carnap's debate, lasting from 1930/1 to late 1932, fell into two substages, from 1930/1 to mid-1932 and mid- to end-1932. What Neurath challenged already in the earliest publications of this period (and presumably from its yet earlier beginning\(^7\)) were Carnap's ideas to the effect that, first, the statements against which scientific hypotheses were tested were indubitable and not in need of justification, second, that they were in fact expressed in the phenomenalistic language, and, third, that it was possible in principle to express them in the phenomenalistic language. Carnap did not respond to Neurath's challenge at all during the first substage, but was rather still concerned to shore up his response to Neider's argument. Against Neurath's vigorous restatement of his criticisms during the second substage, Carnap's responded by accepting Neurath's objections to the first two points. This concession marked the end of the second stage of the protocol sentence debate, but it did not satisfy Neurath, as we shall see. (I should add that the criticism of methodological solipsism did not exhaust the points Neurath sought to impress upon Carnap. During stage two Neurath also developed further his earlier proposal to empiricise, that is, naturalise, epistemology. This point, whose

\(^5\)Carnap 1963a:51.
\(^6\)Ibid.:23.
\(^7\)See appendix to chapter 2.
relevance today is greater than the anti-phenomenalist one did not, however, gain prominence in the protocol sentence debate itself. Instead, it was mostly ignored.)

7.2 Neurath's Physicalist Charge of 1931

As I showed in the previous chapter, Neurath went along with Carnap's view that meaning was a matter for formal considerations. I also showed that Neurath's distinctive version of this view allowed him to integrate meta-linguistic investigations into the materialistic framework of unified science he had advocated since 1929. In the present section I seek to show three things. First, Neurath's physicalisation of meta-linguistic discourse allowed him to launch another attack on the supposed need--even the mere possibility--for basing scientific assertions on phenomenalistic protocols. Second, it allowed him to develop what Haller called the "Neurath-principle" into what looks like an underdescribed coherence theory of justification within the physicalistic framework. (The Neurath principle says that scientists must choose for every statement whether it be integrated into the existing body of scientific statements, requiring adjustment within it or not, or be rejected as unusable.8) Third, the physicalisation of meta-linguistic discourse allowed Neurath to press for the thoroughgoing naturalisation of epistemology. (My consideration of all three points is termed a "first look": that is so because with respect to point one the explication will be further sharpened in response to the development of the debate, and because the explication of points two and three will be continued in my analytical chapters 9 though 11.)

7.2.1 Neurath's Private Language Argument: a First Look

Due to his physicalisation of talk of thinking9, Neurath's physicalism deflected attention from the first person to the neutral third. Whether a purported protocol sentence

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8Haller 1979a, 1979b, 1982a.
9See above section 6.4.2.
was to be accepted became a public affair, no longer could the protocollist claim privileged access and therewith save a sentence's protocol status. Neurath thought so because he held that the phenomenalist language was inservicable across the board, it could not even be invoked for purposes of rational reconstruction. First person privilege was unreconstructable.

Neurath's argument to this conclusion was first published in the fall of 1931 in the Italian journal *Scientia*. It went as follows.

Carnap [...] distinguished two languages: a 'monologising' (phenomenalist) one and an 'intersubjective' (physicalist) one. He tries to deduce the physicalist one from the phenomenalist. However, in my opinion it can be shown that this division cannot be carried out that on the contrary only one language comes into question from the start, and that is the physicalist.\(^\text{10}\)

Here Neurath has everybody on the edge of their seat - well why is Carnap's distinction impossible? Did Neurath, famous for his anticipations of post-Received View Analytic philosophy, also anticipate Quine's point, that, at a crucial stage of the *Aufbau*, Carnap switched from full to partial reductions of the content of the explicated concept to elements of the given?\(^\text{11}\) Or did he anticipate Goodman's criticism of the companionship problem which challenged the universal applicability of Carnap's meta-theoretical data-base and thus rendered being isomorphic to another an elusive property of constitution systems explicating subjective givens, and so jeopardised even the only the formal intersubjectivity the phenomenalist constitution system allowed for?\(^\text{12}\) Nothing of the sort. Neurath did not give an argument which disputed Carnap's logical finesse by demonstrating technical insufficiencies, neither in the *Aufbau* nor in the *Logical Syntax*. Neurath rather continued his argument as follows:

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;

\(^{10}\)Neurath 1931b:54/5; italics restored; translation changed.
\(^{11}\)Quine 1951, 1969a.
\(^{12}\)Goodman 1951.
in short, the person supposedly 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.\textsuperscript{13}

What Neurath provided was a private language argument: while not hitting on Quine's and
Goodman's objections to the Aufbau, Neurath nevertheless hit the jackpot!

It is not exactly news that some such thing as a private language argument was
"about" at the time of the Vienna Circle, and was thus "about" quite independently of
Wittgenstein, to whose posthumously published Philosophical Investigations most
discussions of private language arguments refer as their source.\textsuperscript{14} What is news, I
believe, is that Neurath employed such an argument in the course of his call for the
naturalisation of epistemology: Neurath did more than vaguely anticipate Wittgenstein and
more even, as I shall show, than follow Carnap's pre-Wittgensteinian discussion of the
private language problem.\textsuperscript{15} To say just what kind of a jackpot Neurath's private language
argument constitutes, however, requires careful excavation. Neurath did not elaborate his
private language argument much further anywhere, though he alluded to it in several other
places.\textsuperscript{16}

\textsuperscript{13}Neurath 1931b:55; translation changed.
\textsuperscript{14}Castaneda 1967 lists Carnap 1932c and Weinberg 1936 as pre-Wittgensteinian discussions of the private
language problem. Compare also Davidson's recent aside 1986:442. I may add that, apart from Neurath,
Stebbing 1934 and L.J. Russell 1934 are two further examples of such discussions within the wider
horizon of the Vienna Circle and its sympathisers.
\textsuperscript{15}Koppelberg 1987:28 notes about a related passage in Neurath's later 1932d that "Wittgenstein will
elaborate in detail this thought in his private language argument" but does neither explicate or follow up
Neurath's meaning of the argument nor note the earlier presentations of Neurath's argument. Only
Heidelberger 1985:177 briefly notes and summarises Neurath's argument here at 1931b:55 but does not
designate it as a private language argument nor note or discuss Neurath's later presentations of it.
Heidelberger's reluctance to call it a private language argument perhaps stems from not noticing the
unspoken communalitarian conception which Neurath's argument needs to really work against Carnap.
Heidelberger also does not comment on the difference between Neurath's and Carnap's private language
arguments. As he presented it, even Carnap would agree, but that only shows that not all of Neurath's
points were recovered. (More on all these points below.)
\textsuperscript{16}Neurath 1932a:62, 65; 1932d:96; 1933:G596; 1941a:228/9. With this private language
argument, Neurath dealt with a question he had raised polemically already in his Anti-Spengler (Neurath
1921). There Neurath asked what a solipsist could do to establish the coherence of experience over time.
Neurath did not have to (and did not in fact) deal further with this question against Spengler, the cultural
relativist, but he had to against Carnap, the methodological solipsist. (In the course of his anti-relativist
argument against Spengler Neurath appealed to broadly structural relations: the interpretation of another
depended on discerning relations between a person's behaviour and his/her environment.)
What is clear, in any case, is that this was indeed a private language argument, and what it was in outline. Neurath held that even a solitary thinker required a system of symbolic representation for the ordering of his/her experiences over time and sought to establish that such a system is of necessity intersensual and intersubjective. Neurath's argument was that 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. If physicalistic statements like instrument readings need themselves be translated, in order to be meaningful statements, into phenomenal terms directly related to a scientist's experience then no touchstone at all would be available by which the constancy of his language use could be established: what could such phenomenal terms refer to but my experience now? Neurath suggested that once on a solipsistic base there was no preventing solipsism of the moment. The incoherence and impossibility to conceptualise experience in this solipsistic fashion showed that the constancy of the language use of an individual over time was dependent upon being detectable by others by reference to spatio-temporal determinations of physical states of affairs. Now, if language use was so controllable then that language was already intersubjective (and intersensual): the constancy of its use could also be checked by another speaker. Thus it was shown that already an individual epistemic agent requires the intersubjective language for the coherence of his systematisation of experience.

It is important to note what Neurath did not argue here. Neurath did not argue that phenomenal languages were logically impossible: Carnap had obviously shown how to construct one. Neurath nevertheless contended that such constructed phenomenal languages were not the stuff science was in fact nor could in principle be made of: not only did the phenomenalist language happen to fail to underwrite scientific practice, but there could not be any science to start with if only one's own experiential given formed the content of our statements. What underlay Neurath's reasoning was a further restriction of
the criterion of legitimate physicalistic meta-linguistic discourse to arrive at a criterion of what languages it makes sense to theorise about in epistemology. Following the earlier noted anti-metaphysical strategy of "no type without physicalistic tokens", this was what talking about language physicalistically came down to: any system of logical syntax (pure or descriptive) dealt with language as a physically instantiatable system of symbols. This condition was, of course, trivially fulfilled by any given natural languages as well as any constructed languages actually investigated. Whether a language was humanly possible in practical terms, however, was not yet established by the possibility of giving a formal description of it. Just this practical possibility--what I called "serviceability"--of instantiating a language in human use, was, for Neurath, the criterion of the relevance of any rational reconstruction.

It was with this point that Neurath's private language argument of 1931 introduced considerations not yet in play in the previous round of the protocol sentence debate. Already during stage one of the protocol sentence debate Neider's and Neurath's insistence on the requirement of accessible intersubjectivity had occasioned Carnap's acceptance of need for a materialistic derivation of concepts for the language of science (albeit alongside the phenomenalistic one for epistemology). What is new here in Neurath's 1931 argument then? Neurath's overall argument of course presupposed the view--as did Neider's argument of 1929--that only the physicalistic language provides for the public referents whose accessibility makes for the intersubjectivity of language. Thus Neurath noted (in another paper of the same substage of act two) that the "'phenomenal language' in its present form does not seem to be even suitable for 'predictions', that which is essential for science."  

While he did not spell this out, his meaning is clearly that predictions formulated in the phenomenal language could not be tested by others: apparently Neider's

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17 Neurath 1932a:65; translation amended.
argument was here invoked. What was new here in 1931, then, was that Neurath's argument against the phenomenalistic protocol language during act two did not restrict itself to these points. He added his private language argument which focussed on what was required for an individual agent to come to comprehend and systematise his own experience. (It did not focus only on what was required for human communication.) It is this emphasis that distinguished Neurath's 1931 argument from Neider's anti-solipsistic argument of 1929. Neurath fortified his long held conviction that Carnap's phenomenalist reconstruction of empirical knowledge was irrelevant to actual science with his argument about what the constancy of language use requires.

Neurath's private language argument also helped to articulate his earlier voiced view of the social nature of cognition. To see this, consider the following objection to Neurath's argument. According to his own private language argument only reference to to non-phenomenal states of affairs allowed for the constancy of an individual's language use. But how could such checks of constancy be effected? Surely the individual him/herself could not do so. Whether his or her language was phenomenalistic or physicalistic his/her understanding of his/her own language use still remained prone to the vagaries and fleetingness which had convinced Schlick long ago that the objectivity of knowledge was not to be found in its reliance on ostensibly defined terms but in the structural determination of its terms. To be sure, a physicalist language was intersubjective, but it was not the physicalistic character of the language by itself which allowed for constancy of language use. That was made possible only by the intersubjectivity which the physicalist language made possible. Because its referents were public objects or events, others could correct an individual's language use: that was what ensured constancy.

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18It was this aspect of Neurath's anti-solipsist argument in his 1932a and 1932d that was explicated (with recourse to his contention there that protocols require that the name of the protocols be stated) in L.R. Russell 1934 and recapitulated in Weinberg 1936:275.
19Neurath 1930a:46, see section 4.3.4.
Neurath reaffirmed his private language argument in the published version of his March 1931 lecture in *Erkenntnis* in 1932.

Unified science expresses everything in the unified language that is common to the blind and the sighted, the deaf and those who hear, it is 'intersensual' and 'intersubjective'. It connects the statements of a man talking to himself today with his statements yesterday; the statements he makes with his ears closed with those he makes with his ears open.\(^{20}\)

That unified science expresses everything in the intersubjective and intersensual language means, of course, that also the protocol sentences are expressed in it. That this language was intersensual implied that the language of science only employed concepts whose meaning was determined structurally. Neurath thus still followed Carnap's basic idea for a constitution system and argued for a "mere" change of its base.

To recapitulate this important turn of the protocol sentence debate. The meaningfulness of a symbol system required that the discernment of the structural order which makes for its meaning be intersubjectively controllable: by definition, however, the structural form of a private language is not intersubjectively intelligible. According to Neider's 1929 argument, Carnap's *Aufbau* failed as a reconstruction for the language of science because its propositions were not intersubjectively controllable. To carry his argument for the complete physicalisation of meta-theoretic investigations, Neurath required a second point, over and above that provided by Neider's argument, for Carnap had conceded this point as far as the practice of science was concerned. Yet Carnap insisted that the phenomenal language could be used for epistemological purposes. Thus Neurath had to attack the idea that scientific knowledge could even be "rationally reconstructed" on a phenomenal basis. This second additional point was provided by his private language argument: the symbolic representation system required to make experience cohere cannot be an essentially private one. An individual's constancy of language use cannot be simply assumed. According to Neurath's 1931 argument only a physicalist

\(^{20}\)Neurath 1932a:62; see also ibid.:63.
language can assure the constancy of language use for only it can assure the
tersubjectivity needed; by contrast, the constancy of the use of a private language, and
thus the reliability of his/her cognitions was potentially jeopardised. The statements of
such a system must refer to physical objects and thus are already intersubjective. Neurath's
1931 argument made Carnap's two-pronged strategy impossible. So spelled out,
Neurath's 1931 argument not only fortified Neurath's suspicion of the irrelevance of
phenomenalist reconstructions but also specified further his concepts of the social nature of
cognition. The social mechanisms determinative of language use were left out of the
phenomenalist's picture.

7.2.2 The Neurath-Principle Reaffirmed: A First Look at Neurath's
Coherentism

The rejection of the possibility of a private language was not the only radical
consequence Neurath drew from the physicalisation of meta-linguistic discourse. In
considering the scientific language and scientific thinking physicalistically, Neurath
discouraged any explicit aversion to truth as correspondence.

Thus statements are always compared with statements, certainly not with
some 'reality', nor with 'things', as the Vienna Circle also thought up till
now. [...] If a statement is made, it is to be confronted with the totality of
existing statements. If it agrees with them, it is joined to them; if it does not
agree, it is called 'untrue' and rejected; or the existing complex of statements
of science is modified so that the new statement can be 'incorporated'; the
latter decision is taken mostly with hesitation. There can be no other
concept of 'truth' for science.21

What Haller called the "Neurath principle"—that either a statement is to be integrated into the
body of scientific statements (requiring change in the latter or not) or itself to be rejected—
was clearly endorsed here. This extremely important principle of Neurath's—his
radicalisation of the Duhemian legacy adhered to since his first Boat of 1913—lies at the
root of his coherence theory of justification. Neurath's principle has some striking

21Neurath 1931b:53.
consequences. The first is that the fundamental statements of science, against which all hypotheses are tested for their acceptance or rejection, were themselves not indubitable. The Neurath principle held for all scientific statements, as already asserted in Neurath's March 1931 lecture.\textsuperscript{22} Protocol sentences themselves could be challenged and rejected. They did not provide for secure foundations of scientific knowledge.

The second consequence is that, at least for Neurath himself, the Neurath principle implied that coherence between statements was the only criterion of "truth". This meant that talk of correspondence between statement and fact became "metaphysics" and meaningless.\textsuperscript{23} Recall the possibility of argument raised by Carnap's distinction of the essence problem and the correlation problem of the designation relation: Tractarian correspondence, the sharing of logical form of referent and expression, was determined to be an answer to the metaphysical essence problem of the designation relation (in virtue of what did it obtain?) whereas the correlation problem (what were the relata) could be tackled quite independently and with scientific means.\textsuperscript{24} With the advent of either Carnap's suitably inflated syntactic method or Neurath's physicalist variation on it, this correlation question could be handled "formally". Note then that, when Neurath ruled out talk of correspondence here, he ruled out any attempt to answer the (metaphysical) essence question in the formal mode. What Neurath polemicised against was the idea that consideration of the designation relation would allow the theorist to step out of the physicalist constraints on theorising. What Neurath took aim at here was Schlick's (Wittgensteinian) idea that world-language connections be established from outside of language by experiential determinations and that they could only be shown and not talked about.\textsuperscript{25} "The definition of 'correct' and 'incorrect' as proposed here abandons the

\textsuperscript{22}Neurath 1932f; see above appendix to chapter 2.
\textsuperscript{23}Neurath 1932a:67. I preserved Neurath's scare-quotes around "truth" for reasons discussed below.
\textsuperscript{24}Carnap 1928a: ; see above section 3.3.2.
\textsuperscript{25}But does any talk of meaning presuppose such pre-linguistic foundations, has Neurath not overlooked the possibility that "showing" may be Wittgenstein's way of expressing the fact that talk of meaning and logical form is internal to a practice of language use? Neurath did not overlook this possibility at all

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definition that is usually accepted in the Vienna Circle and recurs to 'meaning' and 'verification'.

In the seemingly paradoxical sense which is rendered intelligible by the conception of form as meaning, Neurath held that syntactic reasoning was all the reasoning about meaning the scientific philosopher was going to get--and need.

A third point to note which, in view of the many suggestions to the contrary, is very important is that Neurath did not propose a coherence theory of truth. To little avail, this was pointed out by Neurath and Carnap themselves. What Neurath did propose was the beginning of a theory of justification. Neurath held that only by "defining"--better: reinterpreting--talk of truth in this way was it possible to keep the "metaphysics" at bay which lay in wait even behind terms like "verification". This reinterpretation was part of the work of unified science:

Science tries to transform everyday statements. They are given to us as 'bundles' ["Ballungen"] consisting of physicalist and pre-physicalist components. We replace them by the 'unifications' of physicalist language. [...] our task is still to free the 'bundles' of our language from metaphysical trappings and to define everything that occurs in them physicalistically. [...] Frequently we can go on using existing 'bundles' after re-interpretation.

"Truth" was just such a "bundle": only its reinterpretation, not its disquisition, was required. So Neurath thought in view of the close relation of the notion of truth to the notion of empirical control. (As I noted, Carnap identified truth-criteria with truth-conditions.) Here Neurath miscalculated badly: many a misunderstanding would have been avoided had he announced his theory as a theory of justification, rather than as a re-definition of truth. That most details of Neurath's coherence theory of justification remained so far unspecified did not help matters either, of course. Nevertheless, the

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26Neurath 1932a:66.
27At least some of Neurath's militant anti-metaphysicalism was rooted in his attempt to prevent the misuse of the new logico-linguistic tools of inquiry.
29Neurath 1932a:63/4.
quotation marks Neurath put around terms like "truth" already indicated that his conception of the business of the theory of knowledge was not the traditional one.\footnote{There is, for example, no reason for the quotation marks in his "There can be no other concept of 'truth' for science" (Neurath 1931b:53). See below sects. 7.7.3, 8.3.2, 10.5.3, 11.2, 11.3.}

7.2.3 Neurath's Naturalistic Epistemology: a First Look

The adoption of a physically acceptable approach to language, the rejection of Wittgenstein's ladder, and the reassertion of the Neurath principle as a universal epistemological principle were not the last of the momentous consequences which Neurath drew from the marriage of Goedel's meta-mathematical methodology and the abandonment of the phenomenalist basis for science. They were but preparations for Neurath's, as opposed to Schlick's, "final" turn of philosophy: its naturalistic abandonment.

First, let me note the innocent sense in which Neurath's conception of the scientific language was a 'natural' one. True to his Machian roots Neurath viewed the abstract calculi of the "strict symbolic language of science" as a development not wholly divorced from the common natural language.\footnote{Neurath 1932a:64.} Clearly, in Neurath's opinion the physicalistic scientific language did not emerge \textit{de novo} in a particularly smart meta-theoretician's head. "The language of physicalism is nothing new, as it were; it is the language familiar to certain 'naive' children and peoples."\footnote{Ibid.:66.} Natural language, so Neurath, already was a physicalist language.

It would be a mistake to believe that the physicalist rendering of everyday affairs must be complicated because very complicated physical formulas are needed - some of which are not yet at hand - for the calculation of certain correlations. The physicalist everyday language comes from the prevailing everyday language: only certain parts are dropped, others correlated in a different manner, and certain additions are made. [...] Children can learn the physicalist everyday language. They can advance to the strict symbolic language of science, can learn to make predictions of all kinds with success,
without even having to resort to 'elucidations' that supposedly function as a meaningless introduction.33

Children can easily learn the physicalist common language because that is already the "natural" language to speak, the language of bodies and objects.34

Yet there is in play in Neurath's physicalism also another sense of 'natural' and 'naturalistic', and with this I return to Neurath's rejection of Wittgenstein's ladder.

All members of the Vienna Circle agree that there is no 'philosophy' with its own special statements. Some people, however, still wish to to separate the discussions of the conceptual foundations of the sciences from the body of scientific work and allow this to continue as 'philosophising'. Closer reflections show that even this separation is not feasible, and that the definition of concepts is part and parcel of the work of unified science.35

Note the barb directed at Schlick's conception of philosophy as meaning determination here: Neurath denied what Schlick (following Wittgenstein) affirmed, namely a separate activity of philosophy over and above science. Instead, he considered the clarification of scientific concepts to be but the most general of scientific activities so that philosophy was at best distinct from the individual sciences in the scope of its clarificatory business. Within unified science there was no radical break between the sciences and the theory of science. Yet did this leave room for philosophy at all? With the syntactic turn taken, the Neurath principle in place, the phenomenalistic protocol language rejected and the reinterpretation of truth talk by talk of justification under way, Neurath declared the end of philosophy.

It is one of the tasks of our time to aid scientific reasoning to attain its goal without hindrance. Whoever undertakes this task is concerned not so much with 'philosophy', properly speaking, as with 'anti-philosophy'.36

33Ibid.:64.
34This speaks against Hempel 1979 who imagined Neurath to require proper "physicalistic" children to speak Churchlandese, i.e. "I'm in brain state XYZ".
35Neurath 1931b:52; see also 1932a:58/9.
It was not only on account of its past metaphysical and theological associations that Neurath sought this radical break of the meta-theory of science from philosophy. Neurath held to the slogan "The last consequence of empiricism: science without philosophy!"\(^{37}\) for he saw no need for philosophy. Conceptual clarifications could proceed within unified science, so philosophy was also no longer needed to defend science against the sceptics.

The possibility of science becomes apparent in science itself. We enlarge its domain by augmenting the mass of statements, by comparing new statements with statements taken over from the past, thus creating a consistent system of unified science that can be used for successful predictions. As makers of statements, we cannot, so to speak, take up a position outside the making of statements and then be prosecutor, defendant and judge at the same time.\(^{38}\)

Given the acceptance of the constraints of physicalism upon scientific theorising, there could not be, for Neurath, any legitimisation of science from without. Here Neurath further specified his oblique opposition (in his 1929 address) to the two perspectives on scientific philosophy I characterised as the traditional and the new canonical picture.\(^{39}\) Neurath's final turn of philosophy went clearly against both the conceptions of philosophy held by Schlick and by Carnap: Neurath thought their ways of going about establishing the actuality and possibility of knowledge misguided.

Against the Wittgensteinian conception of philosophy of the Schlickean wing of the Vienna Circle, Neurath held that scepticism was not to be answered by showing how it is really true what science tells us. Once an inquirer accepted the rules of science as an activity guided by the principle that all its assertions must be testable in principle, and that scientific theories must be accepted or rejected on the grounds of intersubjectively available evidence, then, so Neurath, no ground remained for him to ask for more. And should the inquirer be unwilling to abide by these rules, well, that was just tough on the inquirer: he

\(^{37}\)Neurath 1932e:311, his italics.
\(^{38}\)Neurath 1932a:61, my italics.
\(^{39}\)See above sections 2.3.2, 2.5.1, 3.3.2, 4.5 and 5.3.4.
was hankering after something that was meaningless in cognitive terms. Supra-scientific philosophy, and therewith scepticism, was finished for want of its intelligibility. Now clearly, Neurath thought so not only because the longstanding Viennese criterion of meaningfulness had ruled such philosophical inquiries out of court, but more particularly because of the conception of the essential intersubjectivity of language which turned on the mutual control of language use. The criterion of scientific language had thus become the criterion of the intelligible as such!

Against Carnap, Neurath held that the possibility of objective knowledge was not to be established by the aprioricism of his alternative neo-Kantianism. Rational reconstructions could not help themselves to assumptions about epistemic priority, no matter how "natural"40, that could not themselves be explicated physicalistically. For a truly scientific philosophy, that is, for unified science without philosophy, to be established, it was not enough that Kant's synthetic a priori be rejected. It was also required that the attempt be dropped to devise a transcendental framework which rendered knowledge possible by means of a purely formal a priori. As Neurath put it (anticipating the motto of naturalistic epistemology as "explaining science by science"): "The possibility of science becomes apparent in science itself." That Neurath here explicitly talked about the possibility of science, and not just the actual justification of its knowledge claims, suggests once more41 that he was not unaware of Carnap's alternative neo-Kantianism; it suggests, in fact, that Neurath consciously rejected it.

But Neurath was not wholly negative in his conclusion. Since there was no hope for any illumination of scientific knowledge that would go beyond what the resources of scientific reason themselves are equipped to illumine, whatever remained of the theory of

40Carnap 1928a:107.
41See above section 5.3.4.
knowledge was identified with science as its own general theory of itself. It was this move which prevented Neurath's "anti-philosophy" from mere nihilism.

From all this it becomes clear that within a consistent physicalism there can be no 'theory of knowledge', at least not in the traditional form. It could only consist of defence actions against metaphysics, i.e. unmasking meaningless terms. Some problems of the theory of knowledge will perhaps be transformable into empirical questions so that they can find a place within unified science.\(^{42}\)

Unfortunately, Neurath did not develop his alternative conception any further here, except to illustrate the physicalisation of meta-linguistic discourse, and to propose a physicalistic version of scientific protocols\(^{43}\). Nevertheless, it is clear that, translated into modern terms, Neurath suggested that scientific knowledge was not to be investigated without presupposing scientific knowledge. Yet this was not the end of all theorising about knowledge. Instead, epistemology proceeded as an empirical inquiry into the conditions of knowledge. Such a naturalistic theory of knowledge was all the theory of knowledge a physicalist was going to get.

Neurath's proposal for the naturalisation of epistemology stood in conscious contrast to Schlick's and Carnap's conceptions. When during this first substage of stage two of the protocol sentence debate Neurath, in fact, renewed his earlier proposal for the naturalisation of epistemology\(^{44}\), it was not yet clear that it was to lead him into permanent conflict with Carnap. Thus Neurath urged again, as he did in 1929, that the attempt at the unification of the language of science as undertaken by Carnap in the Aufbau be repeated, on a physicalist, non-phenomenalist basis, of course.\(^{45}\) Little did Neurath suspect that Carnap was to resist his anti-phenomenalist charge on the very grounds of lingering "philosophical" ambitions.

\(^{42}\)Neurath 1932a:67.
\(^{43}\)See above section 6.4.2.
\(^{44}\)See above section 5.3.4.
\(^{45}\)Neurath 1931b:56, 1932a:65.
It may be wondered whether Neurath's argument for the naturalisation was conclusive. Even granting the impossibility of recourse to a phenomenalistic language, why had traditional epistemology become impossible? Surely, not all those theorists of knowledge who reject phenomenalism are naturalists! What then turned the need for a physicalist language as the basic language of science and epistemology into an argument for the naturalisation of epistemology? What did the trick for Neurath was his verificationism. Whatever statement was meaningful had in some sense to be testable. Being testable it fell within the purview of science. "There are always spatio-temporal formulations behind which we cannot reach without saying something meaningless." Global scepticism (all the world a dream?) was meaningless, given verificationism. For Neurath then, as long as we stayed within the realm of the non-phenomenalistic language, our discourse had to be scientific, unless it turned into "poetry or fiction." In addition, Neurath held that we cannot step outside of language in order to "confront language with reality; to use reality to verify whether the language is servicable." That would be like "construct[ing] the scientific language with the help, so to speak, of pre-linguistic means." Such pre-linguistic or pre-propositional cognition is plainly senseless (verificationism or not) Recall Neurath's other metaphor. "As makers of statements, we cannot, so to speak, take a position outside the making of statements and then be prosecutor, defendant and judge at the same time." Given verificationism, and the impossibility of leaving language behind in our cognitive enterprises, only the recourse to a phenomenalistic epistemology could now rescue traditional epistemology. Neurath's argument against Carnap's methodological solipsism thus did close the case: We are like sailors who cannot pull into dry-dock, nor can abandon ship. We cannot leave the language of science behind, nor can we ask the traditional questions of epistemology in the language of science. What questions we can

46 Neurath 1931b:53.  
47 Neurath 1931a:49.  
48 Neurath 1931b:52.  
49 Ibid..  
50 Neurath 1932a:61.
still ask about knowledge could no longer be the questions of the old epistemology, but could only be question of naturalistic epistemology.

7.3 Carnap's Intermediate Response To Neurath's Charge

What then was Carnap's response to Neurath's charges raised against the background of their shared syntactic turn? The answer is: minimal, that is, as minimal as it could be under the circumstances.

7.3.1 Carnap's Retention of the "Original" Protocol Language

The published versions of the two lectures of March 1931 show that he retained the view which Neurath challenged already then, the view that protocol sentences referred to the phenomenal given and did not stand in need of justification. Carnap continued to hold that "verification is based upon 'protocol statements'" which "include statements belonging to the basic protocol or direct record of the scientists' experience"51 in Carnap's sense of a "primitive" or "original" ("urspruenglich") language:

The simplest statements in the protocol-language are [...] statements needing no justification and serving as foundation for all the remaining statements of science. // The simplest statements in the protocol language refer to the given, and describe directly given experience or phenomena, i.e. the simplest states of which knowledge can be had.52

What is remarkable about this response of Carnap's is that Carnap saw no conflict with Neurath in this. In a long footnote he wrote:

Neurath was the first both in the discussions of the Vienna Circle and, later, in the first article mentioned, to demand consistently the rejection of formulations in terms of mental experience [...] the comparison between statements and 'reality'. [...] By distinguishing between the 'formal' and the 'material' modes [of speech], rejecting the pseudo-questions which use of the latter provokes, proving the universality, and in the consistent application of the formal mode to the construction of syntax

51 Carnap 1932b:42; see also his 1932c:166.
52 Carnap 1932b:44/5; the statements here separated by a double slash appeared in parallel columns expressing talk in the correct formal and the potentially misleading material mode respectively. Compare again Carnap 1932c:166.
[...] I have arrived at results which wholly confirm Neurath's views. Moreover, the demonstration [...] that the protocol language can be included in the physical language disposes of our previous difference of opinion on this point (the question of the 'phenomenal language') which is mentioned in Neurath's article. Neurath's suggestions which have often met with opposition, have thus shown themselves fruitful in all [in original added: essential] respects.53

It is obvious, however, that Carnap's retention of a phenomenalistic protocol language with indubitable statements stood in sharp contrast to and did not "wholly confirm Neurath's views". In this section I propose a partial solution of this mystery. Carnap thought the dispute with Neurath ended because he himself had given up the idea that the protocol language spoke only about the phenomenal given. Carnap's new position was that the sentences of the phenomenal language were translatable into sentences of the physical language (namely those describing a bodily state of the speaker, either his/her behaviour or nervous condition). This new position of Carnap's was not sufficient, however, to establish agreement with Neurath. True, Carnap accepted Neurath's suggestion for the analysis of introspective psychological terms as speaking of bodily events54; Carnap also accepted Neurath's rejection of talk of truth as correspondence. Yet Carnap retained the tenet of the indubitability of the statements of the protocol language and the belief that they were formulated in the phenomenal language.55

53 Carnap 1932b:74. What is the "first article mentioned"? The German original reads "in the article mentioned"; there Carnap's note is preceded by a reference to Neurath's "Soziologie im Physicalismus" (1932a), dated "in this issue", and to Neurath's Empirical Sociology (1931c), dated to 1931. (That's no help.) The revised English translation mentions in addition Neurath's "Physikalismus" (1931b), also dated to 1931. (Suggestive.)


55 It would lead too far to pursue the intriguing question of how Carnap's new position was related to Wittgenstein's then current one as rendered in his conversation with Schlick and Waismann in December 1929. According to Waismann's reports (1967: ) Wittgenstein then envisaged an ego-centered version of the everyday natural language as basic to epistemological considerations. His position then would seem to be one midway between that of his Tractatus and his Philosophical Investigations.
7.3.2 Carnap's Private Language Argument

What allowed Carnap to resist Neurath's charge and retain a "separate protocol language"? Carnap presented his reasons in published form first in paragraph 6 of his paper on the physical language as the universal language of science, and then in his Warsaw-lecture-turned-Erkenntnis-paper "Psychologie in physikalischer Sprache" where he advanced the phsyicalisation of the language of psychology as a confirming instance of the unity of science thesis. On both occasion he too presented a private language argument. (It will be noted that he did not really respond to Neurath's, however.)

Carnap's private language argument aimed to eliminate the possibility of raising Neider's kind of objection to the "original" private protocol language. Given Carnap's acceptance of psycho-physical parallelism in the Aufbau, the physicalistic and the phenomenalistic languages had distinct domains; since the latter referred to private givens, so Neider, it could not be intersubjectively verified. Carnap now argued that the phenomenal and the physicalistic language shared the same extensional domain. As usual for Carnap, meaning theoretic questions were handled by the supposedly syntactic device of translation. The question of whether the reference of the phenomenalistic protocol language was compatible with that of the physicalistic language of science thus became whether "statements in the protocol language, e.g. the ('original') protocol statements can be translated into the physical language".56

According to Carnap, all sentences speaking of psychological processes spoke of physical processes. Imagine the sentences of psychology to form a theoretical system. Then the thesis of physicalism amounts to the following.

To every sentence of the system language there corresponds some sentence of the physical language such that the two sentences are intertranslatable. [...] The translatability of all the sentences of [a language] L1 into a (completely or partially) different language L2 is assured if, for every

56 Carnap 1932b:76, translation amended.
expression of L1, a definition is presented which directly or indirectly (i.e. with the help of other definitions) derives that expression from expressions of L2. Our thesis thus states that a definition may be constructed for every psychological concept (i.e. expression) which directly or indirectly derives that concept from physical concepts.\textsuperscript{57}

Carnap proceeded to prove this thesis along tracks well worn since the Aufbau: the test procedures for other mind talk preclude other than physicalistic interpretations of the (hetero-) psychological language. If such talk is meaningful, then it must be verifiable; it must be taken to speak of physical events.\textsuperscript{58} No problems arose from this physicalist conception because, so Carnap, a physicalistic reading of psychological sentences already underlay psychological practice. Psychological concepts were to be understood as expressing a physical property, "defined as a disposition to behave (or "respond) in a specific manner under specific circumstances (or stimuli)."\textsuperscript{59}

Such a view of course encountered the objection that the meaning of psychological concepts was not captured in such an analysis. Against this view Carnap argued as follows. First, assume the verifiability condition of meaningfulness and see whether other mind talk so interpreted agrees with the conception forwarded by the objector. Second, assume the objector's view and see whether verification is possible even in principle.\textsuperscript{60} Here is the first case. Imagine a psychological system sentence P1 whose content went beyond that of a non-psychological system sentence P2 and a protocol sentence p2 which was associated with P2 and stated its verifiable content. Given the verification condition of meaningfulness P1 could, if founded at all, at best be founded on p2.

A sentence says no more than what is testable about it. If the testing of P1 consisted in the deduction of protocol sentence p2, these two sentences

\textsuperscript{57}Carnap 1932c:166/7.
\textsuperscript{58}In the Aufbau, of course, this reduction terminated with the auto-psychological language. Here in 1932b and 1932c Carnap dropped the second part of the reduction, namely from the physical to the auto-psychological language. He could do so because he was concerned with the materialistic derivation of concepts.
\textsuperscript{59}Ibid.:170.
\textsuperscript{60}The arguments of Carnap 1932b:78-82 and 1932c:173-7 are the same in content.
would have the same content. But we have already seen that this is impossible [according to the objector].

In that case, however, it was for Carnap entirely unclear on what evidential base these non-translatable psychological sentences were asserted. Here is the second case:

The two analogous sentences, "The first box contains matches" and "The second box contains matches" are both logically and epistemologically of the same sort. This is why the analogy holds here. The case is different with "I am angry" and "That person is angry." We consider the former of the two sentences to be meaningful and the latter (if its physical interpretation is rejected) to be meaningless. [...] For being a sentence about other minds, not to be physically interpreted, it is in principle not testable.

Given his verificationist assumptions, the analogy broke down if mind talk was to be interpreted non-physicalistically. Carnap concluded:

Our (fictitious) supposition that the protocol language and the physicalistic language speak of completely different facts cannot be reconciled with the fact that the physical descriptions can be verified empirically.

Carnap's private language argument was a straightforward argument from the verifiability condition of meaningfulness for the behaviourist interpretation of other mind talk.

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61 Carnap 1932c:174.
63 Carnap 1932b:E455.
64 To put the matter in the terms of Carnap's representational theory of mind, outlined in Scheinprobleme: Given verificationism, the meaning, which allegedly differentiated between sentences about other minds and sentences about their behavioural indicators, was relegated to the realm of non-theoretical accompanying representations which a rational reconstruction of the logical content of scientific statements could discard as only psychological. Compared to the original representational theory of Scheinprobleme, however, must note the following change in Carnap's position. Originally, it was the non-logical, epistemic "sense" associated with expressions of the physical language which was involved in actual cognition concerning physical and hetero-psychological states of affairs. According to Carnap's 1932 position, however, it was the non-logical epistemic "sense" associated with the auto-psychological language which accounted for the actual cognition concerning one's own and other minds. Carnap continued to assign to non-logical sense--the "accompanying object representations"--whatever his favoured model of language could not accommodate, actual cognition. What was changed between 1928 and 1932 was his determination of which language's non-logical sense was invoked to carry what was unaccounted for otherwise: the only apparent domain of discourse was redetermined--in 1928 the physical, in 1932 the phenomenal--so as to meet objections to his failed psychological sketch of the given. (See Carnap 1932b:90/1; for the prehistory of this change see above appendix to chapter 5.)
To summarise Carnap's private language argument. Neider's argument (during stage one) had shown the insufficiency of a language with an auto-psychological base for the practice of science; Carnap responded with a careful application of Occam's razor. Carnap took Neider to provide an argument that the "primitive" or "original" protocol language needed a physicalistic extension. The view that its referents were only correlated with those of the physical language had shown itself untenable. If the terms of the protocol language did not refer to public objects, then they do not partake of the intersubjective language at all. Carnap's private language argument said that there could not be a language which spoke of essentially private objects: the given did not specify a domain distinct from the physical.

7.3.3 Carnap's Private Language Argument and the Language of Epistemology

Let me now ask how Carnap's private language argument related to his epistemological position. Carnap held that the distinction between sentences about other minds and and one's own mind "cannot be made among the sentences of intersubjective science." All sentences of psychology as sentences of a theoretical system of unified science were sentences about minds in general, other minds. The distinction between other minds and one's own remained a significant one, however, for epistemology, the only type of inquiry which considered first-person statements as such. Carnap required two distinct derivations of concepts, one for science, one for epistemology. For the phenomenalistic protocol sentences a physicalistic interpretation was needed so that others could use and refer to my own in the course scientific investigations.

Recall that these protocol statements were to be indubitable, and so unrevisable. How then, if at all, did the fundamental sentences of the materialistic derivation of concepts

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65 Carnap 1932c:170.
differ from the fundamental sentences of the phenomenalistic derivation, when the latter were translated into the physicalistic language? When these psychological sentences about the given were translated into the physical language they became descriptions of the state of the body of the protocolist.\textsuperscript{66} The protocolist in turn was considered as a detecting device for the presence of a certain physical property in the environment.\textsuperscript{67} (it was good enough for Carnap that the deductions required for such a translation were possible in principle, and that, in the meantime, dispositional definitions (in terms of speech behaviour) could give "necessary and sufficient conditions" for application of, for example, the expression "seeing red now".\textsuperscript{68}) So informed, we can answer the most puzzling question which Carnap's position raises: how could the protocol language still need no justification or confirmation and yet be physicalistic? If the protocol language spoke of physical events, how was it possible that it retained its privileged position within unified science? Carnap's conception of original or primitive protocols presupposed a "sharp (theoretical) distinction between the raw material of scientific investigation and its organisation."\textsuperscript{69} It would seem then that, with the latter translated, there existed a subclass of physical statements with regard to which an individual was epistemically privileged. These sentences concerned the given, considered to be (type-) identical with the stimulation state of the body of the protocolist.\textsuperscript{70} Carnap's position thus would seem to flat-out contradict Neurath's that all physicalistic statements were revisable.

This, however, was not Carnap's position. Carnap held that one had to distinguish between sentences within the physical system language and sentences outside of it, yet prior to it, the "original" protocol sentences. The former kind of sentences were dubitable,

\textsuperscript{66}Ibid.:85 and Carnap 1932b:86/7.
\textsuperscript{67}Carnap 1932c:184/5.
\textsuperscript{68}Ibid.:86. (The explicit assertion that necessary and sufficient conditions were captured was not yet contained in German original)
\textsuperscript{69}Carnap 1932b:43.
\textsuperscript{70}"The facts of the given, the experiences, are physical facts, i.e. spatio-temporal events." (Carnap 1932b:76; translation amended.) Type-identity followed from the behaviourist reduction.
the latter were indubitable.\textsuperscript{71} Carnap took the position that protocol sentences remained in just this way outside of the physical system language. His point was that it required a translation between the protocol sentences in the phenomenal language and their equivalents in the physical language: what was dubitable, so Carnap, was whether the correct translation of these protocol sentences fulfilling this signal function had been found. Unlike the protocol sentences themselves, the physical sentences remained therefore dubitable.\textsuperscript{72} Whereas under the first interpretation given there would exist a class of the sentences of science which were not revisable to the same degree as others (with respect to them one individual, namely the protocollist in question, retained the right to veto any rejection), according to Carnap's position no such discontinuity obtained.

\textbf{7.4 Conclusion to Stage Two Round One: Two Different Physicalistic Readings of Protocols}

The following contrast obtained after the first (published) round of stage two. One difference between Carnap and Neurath lay in what their private language arguments took to be at issue: the understanding of another (Carnap) or the constancy of an individual's language use (Neurath). Another difference was that whereas Neurath argued that the protocol language had to be physicalistic and could not be phenomenalistic, Carnap accepted that protocol sentences spoke about physical things, yet insisted that his methodological solipsism was not incompatible with this. Note that, and how, these differences were related to another, that of what physical things they took the protocols to be speaking of: for Carnap, states of the bodies of protocollists, for Neurath, the states of bodies of protocollists in their relation to their environment.

\textsuperscript{71}Carnap 1932b:43, 1932e.
\textsuperscript{72}Carnap 1932b:48-50 implicitly notes the need for translations but only his 1932e spells out this conception in detail; see section 7.6 below.
Neurath’s first stage-two proposal for the form of protocol sentences was the following:

If someone says: "I see blue", this is coordinated as a 'statement about reality', if one accepts the statement as being about spatio-temporal changes that have taken place outside the man, or as an 'hallucinatory' statement, if certain changes are assumed to be only within the human body, that is, in certain areas of perception within the brain, whatever their delimitations are.\(^{73}\)

Elsewhere he defined "observation statements" (often used interchangeably with "protocol statements") as "statements in which [names or descriptions of] perceiving persons and stimulus-producing things occur''.\(^{74}\) Short of specifying his difference with Carnap over the breadth of the physicalist reading of protocols sentences, these remarks indicate by what kind of physicalistic protocol Neurath wanted to replace the phenomenalistic protocol language. They specified both the bodily state of the protocollist and states of the environment. Though Neurath’s proposal is admittely not crystal clear on this point, this is what he (better) meant. Here’s a circumstantial argument for why Neurath’s physicalist conception of protocols is "relational", as opposed Carnap's "narrow" one.\(^{75}\) Carnap could conceivably retain the privileged position of the protocollist for his own protocol because he, unlike others, had direct access to their subject matter. Even though the given was ultimately physical, its indicators for others were sufficiently complex and inaccessible to render any protocollist’s veto against the rejection of his protocols pretty impervious to rational criticism. Neurath, on the other hand, stressed that there remained no such first person veto power. Now clearly, Neurath could not claim to have better access to the givens of others than Carnap had. He could claim the universal revisability of protocols because they dealt with intersubjectively accessible physical facts, observable physical states of interaction between an epistemic subject and its environment. Neurath's

\(^{73}\)Neurath 1931b:55; see also 1932a:67.
\(^{74}\)Neurath 1932a:62.
\(^{75}\)For on these matters, see below, sect. 7.5.2.3 (further cross-references there).
relational, as opposed to Carnap’s narrow construal of the physicalist reference of protocol sentences was in keeping with his conception of meaning as correlated information, and the thrust of his private language argument.

Despite these differences, however, Carnap can hardly be said to have engaged with Neurath’s stage two argument so far. Yet I argued that already during the first substage of stage two, Neurath did not only argue against the Carnap of the Aufbau or his unadornedly compatibilist position of 1930, but also against Carnap’s then contemporaneous position. Apart from the hermeneutic sense I’ve managed to make of Neurath’s remarks, what argument is there for this conclusion? That already during round one of stage two Neurath took account of Carnap’s 1931 views is shown by the following remark which addressed Carnap’s then still unpublished view—though in a highly oblique way, to say the least:

Though we cannot confront the ‘ego’ with the ‘world’ nor with ‘thinking’, we can still differentiate within physicalism between statements concerning the ‘physicalistically described cube’ and statements concerning the ‘physicalistically described person’, and then can, under certain circumstances, extract ‘observation statements’ and thereby create a substitute for ‘phenomenal language’; but careful investigation will probably show that the bulk of observation statements are part of the bulk of physical statements.

What Neurath here most obscurely talked about was nothing but Carnap’s conception of a physicalised phenomenal language. Statements about a physicalistically described person substituted for statements in the phenomenalistic language and constituted ‘observation statements’. (Note Neurath’s scare quotes!) Against this narrow physicalist

76 I noted earlier that act two of the protocol sentence debate began (at the latest) during the February-March 1931 lecture series in the Verein Ernst Mach. (See above appendix to chapter 2.) The first indication was Neurath’s endorsement there of the Neurath principle as valid for all scientific statements; this position went against Carnap’s position up to late 1932. (Neurath 1932f:312). We are now in a position to see that at least in broad outlines the published papers present the same position as the talks from which these papers sprang. In one of his March 1931 lectures Carnap had stated that there obtained “[n]o separation between ‘psychical’ and ‘physical’ objects.” (Carnap 1932f:311.) That, of course, was the position Carnap endorsed in his intermediate response to Neurath’s charge.

77 Neurath 1932a:65; translation amended.
interpretation, Neurath argued that the observation statements used in science (note the absence of scare quotes around "observation statements" the second time around!) concerned not stimulation states of observers, but spatio-temporal states and events outside of the observer. Neurath did, however obscurely, specify the differences between his own and Carnap's physicalistic interpretation of protocol sentences!

Clearly, however, this nudge of Neurath's did not effect any change in Carnap's thinking. Carnap's insistence to have established agreement with Neurath during round 1 of stage 2 remains puzzling. Carnap may well have found the passage last cited too obscure. It could hardly have escaped him, however, that the physical states to which Neurath's protocol statements referred to were different from the ones his own protocol sentences referred to. Did he think it a "non-essential" point of Neurath's otherwise fruitful suggestions? But how could he do so when in his response to Neurath's charge in round 2 of stage 2 he declared Neurath's complaints (which had remained the same) to be not as ephemeral as Neurath himself portrayed them (for the sake of public Circle unity), but as "central to epistemology"? Does this perhaps indicate that, for once at least, Carnap was sloppy here?

7.5 Neurath's Rejoinder: "Protokollsätze"

Neurath made one further attempt to convince Carnap of his point. In the meantime, sensing Carnap's reticence to give up recourse to an even only apparently phenomenalist, but nevertheless indubitable protocol language, Neurath started to

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78Carnap 1932b:74; see above sect. 7.3.1. Admittedly, Carnap 1932b was published directly following Neurath 1932a in the same issue of Erkenntnis 2, #4/5 (presumably to give pride of place to Neurath as initiator of physicalism). But quite apart from what may have been their personal communication at the time, being a co-editor of Erkenntnis, it does not seem unlikely that Carnap saw Neurath 1932a before publication and could have made amends in his 1932b. What is more, Carnap 1932c, which still retained the position of his 1932b, was published only at the very end of 1932.

79See quotation of Carnap 1932b:74n. in section 7.3.1 above.

distinguish his own physicalist position from Carnap's by the addition "radical" (and did so still before the next and best-known installment of their debate).

The predictions formulated in the universal language are confronted with the protocol sentences which are formulated from the start in the universal language (the radical physicalism rejects the assumption of a separate protocol language).\textsuperscript{81}

It had became clear from Carnap's then most recent contribution that he did not agree with Neurath's private language argument. Not surprisingly then Neurath restated his own in his rejoinder, his famous "Protokollsaetze".\textsuperscript{82}

7.5.1 The Anti-Carnapian Argument of "Protokollsaetze"

Neurath's "Protokollsaetze" ("Protocol Statements") was dedicated to convince Carnap of the one point he so far had shown himself unwilling to accept: the protocol sentences of science were to be formulated not in some phenomenal language but already in the physicalist language of science itself - and there fell under the Neurath-principle.

Neurath provided two kinds of reasons for rejecting Carnap's claim that protocol statements need no justification, considerations of a socio-historical meta-theoretic kind, and meta-linguistic considerations. The first, meta-scientific kind of reason concerned the historical nature of scientific theories and the lessons of scientific practice and issued in his pronouncement that all scientific statements are subject to revision.\textsuperscript{83} The second meta-linguistic kind of reason derived from a restatement of his private language argument and issued in his pronouncement that "[t]he 'methodological' solipsism and 'methodological'

\textsuperscript{81}Neurath 1932c:564; see also his 1932g
\textsuperscript{82}Neurath 1932d. This paper was published in the last days of 1932 in Erkenntnis vol. 3 nos. 2/3. Without declaring it so, this double-issue was virtually dedicated to the protocol sentence debate, containing six articles on the topic which by then, I suppose, had accumulated in the editor's offices. Its first article was Carnap 1932c (a version of one of his two talks of March 1931), followed by Zilsel's and Duncker's criticisms and Carnap's response (1932d), followed by Neurath 1932d and Carnap's response to it (1932e). I count Carnap 1932c and 1932d into round one of the second stage of the debate because their position, unlike that of his 1932e, is the same as his 1932b.
\textsuperscript{83}Neurath 1932d:94.
positivism [...] do not become more usable by the addition of the word
'methodological". Since Neurath partly mixed his argument against Carnap's view
with the presentation of what follows from his own alternative conception, I shall here
reconstruct Neurath's rejoinder to Carnap in a more systematic form than that given in his
paper itself, and change the order in which his points were made for, I hope, greater
perspicuity.

7.5.1.1 The Private Language Argument Restated

First off, in steps one and two of my reconstruction, come the meta-linguistic
considerations. Step one was the restatement of Neurath's private language argument.85

The universal jargon, in the sense explained above [the physicalistically
cleansed language of everyday], is the same for the child as it is for the
adult. It is the same for a Robinson Crusoe as for a human society. If
Robinson wants to join what is in his protocol of yesterday with what is in
his protocol today, that is, if he wants to make use of a language at all, he
must make use of the 'intersubjective' language. [...] every language as
such is 'intersubjective'; it must be possible to incorporate the protocols of
one moment into the protocols of the next moment, just as the protocols of
A can be incorporated into the protocols of B. Therefore it makes no sense
to speak of monologising languages, as Carnap does, nor of different
protocol languages that are later related to each other.86

Neurath's reasoning here is the same as in his previous two papers in his dispute with
Carnap, with this difference only: Carnap's modified position of recognising distinct
personal protocol languages and relating them to each other via his elaboration of his long-
held intertranslatability thesis of phenomenal and physical languages was now clearly noted
and dismissed.

84Ibid.:97.
85In the actual course of the paper this restatement of the private language argument followed what I
reconstruct as steps two and three: their ultimate persuasiveness depends on the demonstration of the
untenability of Carnap's alternative by the private language argument.
86Ibid.:96/7.
Now, if this argument is to work against Carnap it is important, that it is construed as I construed its earlier expressions above, namely, as turning on the question of what made for the constancy of an individual's language use. (Carnap's private language argument, as we saw, did not consider this matter.) Unless this was the right interpretation of Neurath's argument it is hard to see how Neurath could have used it, essentially unchanged, to renew his dispute with Carnap.

7.5.1.2 The Inescapable Consequences of Using Vague Expressions

The second step of my reconstruction of Neurath's argument consists in noting that, if the protocol sentences were formulated in the intersubjective physical language and referred to stimulation states and their relation to the environment with specifications of the circumstances of their assertion, then there was no way to overcome impreciseness (and the limitations it imposes) even in the language of science.

The fiction of an ideal language composed of neat atomic statements is as metaphysical as the fiction of Laplace's spirit. Scientific language, with its ever growing equipment of systematic formulations, can by no means be regarded as an approximation to such an ideal language.\(^\text{87}\)

The idea of an ideal fully precise language overlooked the fact that highly scientific languages are available only for certain sciences, and even there they did not cover the entirety: it was necessary to recur to the physicalistically cleansed ordinary language for the formulation of protocol sentences which retained many vague terms.\(^\text{88}\) The universal language of unified science thus could not be but a "universal jargon", a language mixing terms of both, the ordinary and the mathematical scientific language.

Neurath had explicitly criticised Carnap's recourse to an "ideal" language during stage one of their debate.\(^\text{89}\) Here he renewed his criticism with reference to his contention

\(^{87}\text{Neurath 1932d:91.}\)

\(^{88}\text{Ibid.:93.}\)

\(^{89}\text{Neurath 1928b:296, 1930a:42; see sections 5.2.1 and 5.3.1 above.}\)
that protocol sentences were "assertions with physicalistic concepts" which included specifications of time and place.\textsuperscript{90} Neurath placed particular emphasis on the vagueness engendered by the use of proper names.\textsuperscript{91}

'Otto' is itself in many respects an imprecise term; the statement, 'Otto observes', can be replaced by the statement, 'The man whose carefully taken photo is no. 16 in the file, observes'; but the terms 'photo no. 16 in the file' has not yet been replaced by a system of mathematical formulas that is unambiguously coordinated to another system of mathematical formulas that takes the place of 'Otto' [...] etc.\textsuperscript{92}

Neurath's point stands in need of clarification. A number of points could have been made here. First, that spatio-temporal objects cannot be located at a precise point in space and time.\textsuperscript{93} Second, that the question of objects' identity through time is a problematical one. Third, that the relation between scientific terms of the ordinary language ('liquid', 'solid') and in scientific theories proper is a problematic one. Fourth, that it is impossible to ground talk of spatio-temporal objects in phenomenal experience. Which one of these points did Neurath have in mind? Neurath was concerned with points three and four.

First, point three: Neurath here stated that the specification of the protocolist did not measure up to the rigors of the highly scientific language. Neurath's remarks were addressed to the general project of providing an exhaustive characterisation of scientific concepts by implicit definitions in the Hilbert-Schlick tradition as continued by Carnap in the \textit{Aufbau} with definite structure descriptions as his tool.\textsuperscript{94} Neurath's remarks made

\textsuperscript{90}Neurath 1932a:G539; this requirement was now restated: 1932d:93.
\textsuperscript{91}Neurath apparently also had similar qualms about perception terms but did not expand on them: "a protocol is not 'primitive' in Carnap's sense, because it cannot get around the 'Otto' and the 'perceiving'". (Neurath 1932d:96.)
\textsuperscript{92}Ibid.:91.
\textsuperscript{93}This point was made, for example, in Hahn 1933:75.
\textsuperscript{94}See above section 3.2.2. To briskly recapitulate. This project sought to establish the possibility of objective knowledge and show how to justify scientific knowledge by a complete structuralisation or formalisation of empirical knowledge. This project required the elimination of all only intuitively descriptive terms which introduced vagueness and uncertainty. (Schlick 1918/25.) During the syntactic "stage of Carnap's theorising presently at issue the project of structuralisation was pursued not only by using logico-mathematical expressions but also by using descriptive of linguistic structures (what Carnap called "descriptive syntax" as opposed to "pure syntax").
implicit reference Carnap's proposal for integrating ostensive definitions into the translation rules of descriptive logical syntax. Neurath's point was that, if ostensive definitions were included as "translations" in the category of structural descriptions, then these structural descriptions did not help the theorist to escape the vagaries of the natural language they were a description of. In the absence of a phenomenalist reduction of the expressions of the physical language to "founded" basic relation extensions (the Aufbau project) only a system of explicit mathematical formulae could provide the required non-intuitive structural cleanliness. It was impossible then to arrive at "clean" protocol sentences, not only in the physical everyday language, but also in the language of science. The imprecision due to the lack of complete mathematical formalisation was here to stay.

Point four followed from the foregoing. In the absence of recourse to a phenomenalist reduction (rendered inservicable by his private language argument) protocol statements had to make reference to relational physical states so as to allow for intersubjectivity and constancy of language use. The intersubjective accessibility of protocol statements required the specification of the protocollist; but such a specification could only be given in the physicalist language, it was not recoverable in Carnap's phenomenal language.

Carnap tries to introduce a kind of 'atomic protocol' by demanding that "a strict distinction be made between the making of a protocol statements and the processing in the scientific procedure" [...] in so far as personal names appear in protocol statements, 'processing' must always have taken place.95

Carnap's reconstructive ideal of a clean protocol language had to be abandoned. The physicalist language to which we had to recur simply could not be as "clean" or "atomic" as Carnap envisaged, for the reasons given above. From the ordinary language we start at the beginning of science and to the ordinary language we must return to integrate our specific results into the totality of our knowledge.

95Neurath 1932d:96. Carnap made this distinction at 1932b:43.
7.5.1.3 The Lessons of Scientific Practice

Now that it was established that it was impossible in principle to build up scientific theories from clean atomic statements, there remained no reason for rejecting considerations from the what went on in actual scientific practice as irrelevant for rational reconstruction. The third step of Neurath's argument thus turned to his meta-scientific considerations. First, Neurath pointed to evidence from the history of science, namely, the changes occurring in theories in the course of the historical progress of science.

The process of change in the sciences is like this. Sentences that were used at a certain age drop out at a later age and are often replaced by others. Sometimes the wording remains, but the definitions are changed. Each law and each physicalistic sentence of unified science or of one of its factual sciences is subject to such change. The same is true for every protocol sentence.⁹⁶

According to Neurath, the history of science shows that even sentences once held to provide the evidential base of theories were rejected by later theories. Neurath complemented his handwaving towards examples from the history of science here⁹⁷ with an argument that turned on considerations of the nature of scientific theories as such, considered in abstraction from any historical actualities. Neurath once again affirmed his, the Neurath principle.

In unified science we try [...] to create a consistent system of protocol statements and non-protocol statements (including laws). When a new statement is presented to us we compare it with the system at our disposal and check whether the new statement is in contradiction with the system or not.⁹⁸

To show that even protocol sentences were no exception, Neurath produced the thought experiment of the scholar Kalon who wrote down simultaneously two contradictory protocol sentences. What would we do here?

⁹⁶Neurath 1932d:94, his italics; compare also his 1936c:G714.
⁹⁷He did not refer to his 1915 and 1916, nor gave examples, as he did in his 1935b:129.
⁹⁸Neurath 1932d:94, his italics.
Two conflicting protocol statements cannot be used in the system of unified science. Though we cannot say which of the two statements is to be excluded, or whether both are to be excluded, we can be sure that not both can be 'verified', that is, it is not the case that both statements can be incorporated into the system. If in such a case, a protocol statement has to be given up, why not also sometimes when, only after long chains of logical argumentation, contradictions appear between the protocol statements on the one hand and a system of protocol statements and non-protocol statements (laws, etc.) on the other hand? According to Carnap we could only be forced to change non-protocol statements and laws. But in our view the cancelling of protocol statements is a possibility as well. It is part of the definition of a statement that it requires verification and therefore can be cancelled.99

Neurath's general argument for the revisability of protocol statements then was this: To hold a statement unrevisable is to exempt it from the scientific requirement that it be controllable and rejected, if found wanting in support; it is thus to place it outside of science; on pain of building science upon meaningless vocalisations protocol statements thus better be rejectable. Neurath argued here from entirely general considerations: what it meant for a sentence to belong to the scientific language. (Neurath's private language argument had taken care of Carnap's efforts to devise a language that was both intersubjectively verifiable in principle and private in practice.)

Clearly, in "Protokollsätze", Neurath did not adduce any new arguments against Carnap's position other than his examples of inescapable vagaries even in the language of science, his pointing towards examples from the history of science, and his thought experiment. Basic to these points was, of course, his private language argument which established the need for a physicalistic language even for purposes of (what remained of) epistemology. That no other new arguments were presented and his dispute with Carnap was yet renewed can only mean that Neurath thought Carnap to have missed the point of his arguments all along.

99Ibid.:95.
7.5.1.4 The Boat

Neurath summed up the morals of his position in his famous metaphor.

There is no way to establish fully secured, neat protocol statements as starting points of the sciences. There is no tabula rasa. We are like sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry-dock and reconstruct it from the best components. Only metaphysics can disappear without trace. Imprecise 'verbal clusters' ['Ballungen'] are somehow always part of the ship. If imprecision is diminished at one place, it may well re-appear at another place to a stronger degree.\(^{100}\)

As in previous occurrences of the Boat, Neurath's metaphor points us to the socio-historical background conditions of scientific theorising, to what scientists share with others and cannot call into question in its entirety.\(^{101}\) In the case of this, its third appearance in Neurath's writings, it points first of all to the fact that the physicalist language of advanced science could not be sharply divorced from the ordinary language from which it had emerged. Given the argument as I laid it out, furthermore, that this conclusion could not be evaded by recourse to some ideal language depended on Neurath's prior argumentative step which established the practical impossibility of a solipsistic language. Neurath's third Boat thus also points to the social nature of cognition as shown up in his private language argument.

I should also add that Neurath's arguments must not be taken as a disparagement of the pursuit for special language forms that were cleansed of the inadequacies of the traditional natural languages, or as betokening a generally anti-formalist tendency: from the very beginning of his career Neurath had actively advocated such ventures!\(^{102}\) What Neurath urged here was merely the recognition of the limits of this enterprise of formal structuralisation of knowledge. This points to the fact that Neurath's conception of the

\(^{100}\)Neurath 1932d:92.
\(^{101}\)Neurath 1913a:457, 1921:199.
\(^{102}\)E.g. Neurath 1911.
nature of the theory of knowledge within unified science was different from Carnap's (and, of course, Schlick's). Scientific knowledge was not to be justified, and its objectivity defended, by rendering it impervious to vagueness through its complete formalisation. Rather, "the possibility of science" was to be rendered "apparent in science itself".103

7.5.2 Neurath's Alternative

I now turn to Neurath's alternative conception of scientific philosophy in the slightly greater detail in which it was presented here. Neurath did not find the time to elaborate much further his earlier suggestion for the naturalisation of epistemology in this installment of his debate with Carnap, but he did explore its consequences by considering the form of the scientific language, and of the protocol sentences in greater detail than before. I will consider both below. But already the broad direction into which Neurath's proposal pointed can be indicated by comparing his anti-Carnapian argument with Schlick's argument against Carnap. Did Neurath urge that the theory of knowledge could not avoid recourse to intuitive, "experienced" meaning? The answer is "no". Neurath made the point that the existence and availability of language presupposed a lot of things which were not expressed in language itself. But instead of adverting to intuitive meaning, Neurath pointed to the contextual aspects of these linguistic structures in use. To return to Neurath's example: which person could be meant by "Otto", with whom was the speaker acquainted and which relationship to another would explain the speaker's assertion of this statement? Neurath's definition of the sameness of statements in terms of speech behaviour could disambiguate assertions better than any formal method under such conditions. (Thus Neurath brought the notion of function into the equation of form and content.104)

103 Neurath 1932a:61.
104 This is further explored below; see fn. 110 for cross-references.
7.5.2.1 The Language of Science: A Universal Jargon

While Neurath did not explore his epistemological alternative here, he did specify his related alternative conception of the form of the scientific language. In Neurath's view the language of science emerged from the everyday language and remained inescapably tainted by the latter's lack of precision. (Protocol sentences, for example, were allowed to contain the vague terms of the physicalistically cleansed everyday language, if there were grounds to think them at least in principle replaceable by (future) terms of the highly scientific language.\textsuperscript{105}) Neurath distinguished the three kinds of languages at our disposal and indicated their relation to one another.

What is first given to us is our historical ordinary language with a multitude of imprecise unanalysed terms ["Ballungen"]. We start by purifying this ordinary language of metaphysical components and thus arrive at the physicalist ordinary language. [...] In addition, there is the physicalist language of advanced science that we can design to be free of metaphysics from the very start. It is at our disposal only for special sciences, indeed only parts of science. If we want to embrace the entire unified science of our age, we must combine terms of ordinary and advanced scientific languages, since in practice, the terms of both languages overlap. There are certain terms that are used only in ordinary language, others that occur only in scientific language, and finally terms that appear in both. In a scientific treatise that touches upon the whole range of unified science, only a 'jargon' that contains terms of both languages will do. We expect that it will be possible to replace each word of the physicalist ordinary language by terms of the scientific language - just as it is also possible to formulate the terms of the scientific language with the help of terms of ordinary language.\textsuperscript{106}

For a change, what Neurath said here is pretty self-explanatory. I need only note that, with the conception of scientific languages as adumbrated here, Neurath took a decisive step away from the idea that the language of science could be reconstructed by a(n inversely) pyramidal system of constitution system with clean observational terms--even physicalistically understood--at the bottom. Given the vagueness of the terms of the everyday physicalistic language their replacement by terms of the highly scientific language

\textsuperscript{105}Neurath 1932h:233.
\textsuperscript{106}Neurath 1932d:91/2.
was at least marked by the latitude of choice in how they were to be precisified. This step of Neurath's was completed when he took his encyclopedic turn in 1934/5.\textsuperscript{107}

7.5.2.2 Neurath's Protocol Sentences

Finally, Neurath also first presented in this paper his fully-fledged proposal that the protocol sentences be of a certain form.

A complete protocol statement might for example be worded like this: "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)]."

This factual statement is so constructed that, after 'deletion of brackets', further factual statements appear, which, however, are not protocol statements: "Otto's speech-thinking was at 3:16 o'clock: (at 3:15 o'clock there was table in the room perceived by Otto)" and further: "At 3:15 o'clock there was a table in the room perceived by Otto". [...] It is essential for a complete protocol sentences that the name of a person occur in it. [...] for the protocol sentence to be complete, the expression within the innermost bracket is a sentence that again features a personal name and a term that belongs to the sphere of perception terms.\textsuperscript{108}

Neurath's proposal for the form of protocol sentences provided for endless wonderment amongst philosophers and is generally held to be either unintelligible or ill-formed.\textsuperscript{109} I cannot here provide a full explication of Neurath's proposal; for now, I can only motivate the one aspect of his proposal which bears most directly on his argument against Carnap's methodological solipsism.\textsuperscript{110}

Neurath's talk of "'deleting brackets'" indicates that protocol sentences as envisaged would be decomposable into parts. One of these parts is the sentence in innermost brackets: "At 3:15 o'clock there was a table in the room perceived by Otto."

Neurath stressed of this sentence again that it must not contain indexicals. This

\textsuperscript{107}See Neurath 1935a.
\textsuperscript{108}Neurath 1932d:93/4, his italics.
\textsuperscript{109}For one recent expression of wonderment see Creath 1987b:474. To date, no one has yet provided a closer interpretation than Quine 1969a:85, namely as "reports of relations between perceiving and external things".
\textsuperscript{110}And while a full explication is indeed beyond the scope of this dissertation, more hints towards Neurath's theory of protocol sentences will be investigated--below sect. 8.2.2, ch. 9, sects. 10.4, 10.5, 11.2--to give the probably most detailed explication of his proposals yet in sect. 11.3.
requirement ensures that the innermost sentence represents what Frege would call a complete thought: it is publically accessible without requiring recourse to contextual features of its assertion. (Within reason: as noted above, not all contextual dependence could be avoided, but reliance on indexicals could.) For Neurath, furthermore, the innermost sentence of a complete protocol sentence reported not merely an observable state of affairs, but the actual observation of this state of affairs by the specified protocolist. The requirement that the innermost sentence express a complete thought meant that the subject as well as the object of the observation had to be so denoted that they were publically accessible. Both had to appear in straight forward physicalist descriptions; the physicalist description of the protocolist alone was not enough. (It was not sufficient that some state of being-appeared-to was ascribed to the protocolist.) Here then Neurath's relational physicalist interpretation of protocol sentences (as opposed to Carnap's narrow one) found clear expression in the very form the protocol sentences had to take according to his proposal.

It was a consequence of this aspect of Neurath's proposal for the form of the protocol sentences that it really did not matter whose protocols were at issue when scientists set about testing theories. Neurath considered it "without any fundamental significance"\(^\text{111}\) that people tended to hold on to their own protocols more tenaciously than to those of another. Since the protocol sentences specified the protocol taker in objective terms another's protocols could be just as servicable as one's own. To illustrate this process of the acceptance of protocol sentences, Neurath imagined a sorting machine, programmed with all previously accepted sentences of the science under consideration. This machine sorted through newly proposed protocol sentences that were fed into it and

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\(^{111}\text{Neurath 1932d:97.}\)
sounded alarm when a contradiction obtained. Whose protocols they were was irrelevant.\textsuperscript{112}

7.6 Carnap's Considered Response: "Über Protokollsätze"

We are now reaching the end of stage two of the protocol sentence debate, Carnap's second reply to Neurath. What was Carnap's reply this time? Carnap responded as Carnap would: "My opinion here is that this is a question not of two mutually inconsistent views, but rather two different methods for structuring the language of science which are both possible and legitimate."\textsuperscript{113} For Carnap, the two methods consisted of thinking of protocol statements as themselves as belonging to a language separate from that of science or as already belonging to it. It was a question to be answered by a conventional determination, "not by an assertion", as he (implicitly) rebuked Neurath.\textsuperscript{114} In this ecumenical constructionist spirit Carnap set about evaluating the different ways of conceiving of protocol sentences and their relative merits.

7.6.1 Carnap's Defense of the Phenomenalistic Protocol Language

In the system form where protocol sentences remained outside of the system language they were considered as "signals" for which rules of translation were required. The protocol sentences bore upon the system of scientific statements only in their translation and were considered for their consistency with it in this form only.

Corroboration (confirmation, verification) of a system sentence means therefore agreement with the protocol sentences; corroboration of the protocol sentences cannot be required (although this is the case for the aforementioned system sentence which asserted the signal process did in fact take place). If we encounter a contradiction in the system then we make

\textsuperscript{112}See Davidson 1982a for an argument why these sentences to be thrown into the machine should be viewed as beliefs. Talk of protocol sentences was talk in the formal mode--and so disguised the fact that these were sentences which spoke of the protocolist's experiences. Neurath 1934a:161 would suggest as much: the question which his anti-phenomenalism raises is simply how these experiences are conceived of by Neurath. (See below sect. 10.5.)

\textsuperscript{113}Carnap 1932e:457.

\textsuperscript{114}Ibid.:458, 464.

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a change either in the hypothetically established sentences or in the translation rules.\textsuperscript{115}

This system form (which did not include the protocol sentences themselves) allowed the protocol sentences to retain the characteristics Neurath found so objectionable: all that could be done about them was to change their translation into the scientific system language. They could not be rejected: that revisions of their translations were allowed in cases of contradiction with the system followed not from acceptance of their fallibility but from the presumption of their indubitable veridicality. To demonstrate the need to consider protocol sentences revisable Neurath had given the imaginary example of a scholar who simultaneously produced two contradictory protocol sentences (one with each hand)\textsuperscript{116}: should such a case occur then, so Carnap responded, "we conclude that we have erred in the interpretation of the signals."\textsuperscript{117} Clearly, Carnap simply ruled out the possibility that the case envisaged by could occur.\textsuperscript{118} The reason was that he held on to his conception of direct and indubitable reports of the given in a phenomenalist language.

7.6.2 Carnap's Preference for a Physicalistic Protocol Language

The second system form conceived of protocol sentences as part of the system language. Here Carnap saw two possibilities in turn: (A) determine the protocol sentences to be of a certain form, say that suggested by Neurath, and (B) to leave their form open such that every singular sentence could serve as a protocol sentence. Route (B) had been suggested to him in conversation by Karl Popper and went as follows.\textsuperscript{119}

Every concrete sentence of the physicalistic system language can serve under certain circumstances as a protocol sentence. [...] it is a matter of decision which sentences one wants to use as [...] protocol sentences. As soon as one wants - should doubts appear or if one wishes to lay a more secure foundation for scientific theses - one can take take the sentences

\begin{itemize}
\item \textsuperscript{115}Ibid.:463.
\item \textsuperscript{116}Neurath 1932d:95.
\item \textsuperscript{117}Carnap 1932:461.
\item \textsuperscript{118}It may be noted here that recent empirical findings of the effects of brain bisections would force Carnap to hold that there were two persons in one body.
\item \textsuperscript{119}For more on Popper's role in the protocol sentence debate, see appendix to chapter 8.
\end{itemize}
previously regarded as endpoints and reduce them in turn to other sentences which are interpreted as endpoints by choice. In every case the reduction - engaged in for purpose of control - has to stop somewhere. In no case, however, is one forced to stop at any specified place. From any sentence one can reduce still further; there are no absolute initial sentences for the construction of science. 

In this second system form, Carnap noted, "we have the choice either of modifying this protocol sentence, or the group of other concrete sentences in question, or to modify the group of laws which helped derive these sentences." 

Alternative (A) allowed Neurath's proposal to be followed, but Carnap noted a formal objection. This objection was that "it has the defect, from the point of view of syntax, that a sentence which refers to another contains the other as a clause" but Carnap did not spell it out further. Whatever the right interpretation of Carnap's 1932 objection may be, it must be noted that Carnap did not uphold it. When he discussed the matter of protocol sentences again later his objection against Neurath's proposal was an entirely different one. His own proposal raises a yet different criticism. Carnap suggested that the "formulation of the methodological requirement concerning basic sentences" (namely, that there exist a "certain connection between [them] and our perceptions") be formulated as rules about the scientific language, namely, about which sentences of the physicalistic language were to count as basic sentences. Unlike Neurath, Carnap did not want his protocol sentences to say of themselves that they were protocol sentences. Carnap apparently objected to Neurath's proposal that the rule, namely, the requirement of

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120 Carnap 1932e:465/6; his italics restored, translation amended.
121 ibid.:467.
122 Ibid.:465. I find it difficult to make sense of this objection, given Carnap's own conception of the syntactic meta-language according to which sentences could talk about themselves. (E.g. Carnap 1934b:12, 24.) It would seem that only a minor repair of Neurath's proposal was required, say, turning it into a conjunction of paratactically related sentences.
123 According to his 1936/7:9-13 protocol sentences were to be formulated entirely in terms of the "primitive" predicates of the physicalistic language, i.e. in terms which were both confirmable and observable thing predicates. That in Neurath's protocols the innermost sentence reported an observation in terms of its object and subject required the use of perception terms. For Carnap this procedure had the "disadvantage" that the primitive basis of the physicalistic language would include terms "intersubjectively confirmable but only subjectively observable", namely perception terms.
observationality, find its direct reflection in the form (and content) of the basic sentences themselves.\textsuperscript{124}

Carnap thus went with Popper's proposal. Doing so meant for him that, again, he followed Neurath, only that this time he advanced "a step further: in [Popper's] testing procedure there is no last sentence" for not even the form of protocol sentences was here predetermined.\textsuperscript{125} In evaluating the two broad system forms--protocol sentences outside of or within the physicalistic system language--he preferred the second in version (B) as "the most suitable among the forms of the language of science which are currently described".\textsuperscript{126} What made it so suitable was not that it was a truer "theory of the structure of cognition" but that it was a more convenient proposal for a language form: it dispensed with the need for rules of translation between extra-system and system sentences. (Carnap did not declare any of the three methods contemplated to be impossible.)

7.7. The Outcome of Stage Two: Three Remaining Disagreements

What then was the result of the second stage of the protocol sentence debate? While on the question of what makes for the empirical basis of theories Carnap's position had moved considerably closer to Neurath's during the course of stage two there still remained two open disagreements between them. Furthermore, there lay yet deeper disagreements behind these open ones. Here I can only highlight the two surface disagreements and one of the underlying ones. (Further disagreements will be unearthed in the analytical chapters 9 through 11. The role of Popper is evaluated in the Appendix to chapter 8.)

\textsuperscript{124}Carnap 1936/7:13.
\textsuperscript{125}Carnap 1932e:469.
\textsuperscript{126}Ibid.:470.
7.7.1 Two Surface Disagreements

The first remaining open disagreement was the following. Carnap had conceded Neurath's point that protocol sentences could be formulated in the intersubjective physicalistic language of science. Carnap's adoption of (Popper's) method (B) marked his apparent acceptance, of Neurath's arguments against the idea of building up science from "clean atomic elements". In truth, however, Carnap did not accept Neurath's argument against the phenomenalistic language (but merely accommodated Neider's). Carnap did not come to agree with Neurath that protocol sentences had to be formulated in the physicalistic language. Neurath, on the other hand, continued to argue that any protocol languages, whose sentences needed no justification, were to be rejected as "non-realisable". This formulation clearly takes account of, and rejects as irrelevant, Carnap's proof of the logical possibility of an auto-psychological language, namely its construction in the abstract. A private language was a practical impossibility.

The second remaining open disagreement was that Neurath continued to advertise his own proposal for the form of protocol sentences, unimpressed by Carnap's advocacy of the Popperian form (B). In response to Neurath's prodding, Carnap had abandoned his narrow conception of the physicalist interpretation of protocol sentences. Yet he did not accept Neurath's relational physicalist conception either. Instead, he adopted Popper's conception of protocol statements as straightforward statements of fact. This conception was "wide" in the opposite sense of his earlier "narrow" interpretation: now only the physical facts observed were to be stated, any mention of the process of observation itself was dropped. At no time then did Carnap accept Neurath's view that protocol sentences reported both the physical facts and an observer's relation to them.

127 Neurath 1933:G591.
Yet Neurath did not launch further attacks on Carnap. As is already apparent from my reference to one of his further publications, this does not mean that Neurath came to agree with Carnap. What is known of their private communications underscores this conclusion. In a 1934 letter to Carnap, Neurath called his advocacy of Popper's proposal for the form of protocol sentences an "abandonment of empiricism"!129 One can only presume that Neurath thought further public discussions too divisive--for the Vienna Circle's physicalist wing in particular which was about to become the object of counterattack for Schlick and his followers.

Behind these clearly apparent disagreements about what Neurath claimed was the inevitability of a physicalistic protocol language, and about what the form of protocol sentences should be, lie yet deeper disagreements. I shall lead into the first of these by briefly reviewing the one foundationalist interlude of stage two of the protocol sentence debate, Zilsel's query to Carnap. (The others require a broader overview and will be discussed in my concluding chapters 8 and 9.)

7.7.2 Zilsel's Query To Carnap

Zilsel's query of Carnap's physicalism as presented in his intermediate stage two position took issue with the project of the structuralisation of empirical knowledge. Carnap's syntactic turn had transformed the project from the structuralisation of the given to the structuralisation of language. Long ago, Schlick had raised the the problem that, if complete structuralisation of the meaning of concepts could be achieved, then knowledge so reconstructed would be wholly divorced from experience. Thus Zilsel raised the question how Carnap's syntactic physicalism was to distinguish the system of scientific statements from all the possible systems of scientific statements.

At this one point the formal mode of speech fails, for the connection of one of the structures of sentences to the real "material" experiences cannot be captured in words and sentences. To do empirical science thus means to apply what can be said to what cannot be spoken of.\textsuperscript{130}

Zilsel asked: If explicit recourse to the data of experience by reliance on the relation of correspondence between statement and fact was renounced, was Carnap not committed to an implicit return to intuition with whose intimations one of these theory forms was to be filled out and thus distinguished "as real"? Did physicalism not rely on the existence of that which cannot be spoken of?

Carnap's response stated that no uniquely determinative criterion could be given for what the system of scientific statements was that alone applied to the data. There was no need to bring the unspeakable into the logic of science:

There is no other distinguishing criterion for "our" science than the historical one that it is the science of our cultural circle; more precisely: the science which with such and such hypothetical suppositions - appearing there and there in the historical development - was built up following these and these scientific methods and is controlled by the protocol sentences of the scientists of our cultural circle.\textsuperscript{131}

Carnap's response to the question which of the many possible in themselves coherent systematisations of experience was to be considered the truly scientific one was that it was that which the majority of scientists agreed on.

For Carnap, it was not the business of the materialist derivation of the concepts of science to ensure that a given language of science captured the actual data of experience. Within the physicalistic scheme of things the only questions vaguely related to epistemological ones was how protocol statements came to be made. This was a question of conditioned behaviour---speech behaviour\textsuperscript{132}---and had to be answered by empirical

\textsuperscript{130}Zilsel 1932:153, his italics.
\textsuperscript{131}Carnap 1932d:180. This point, which went back to Neurath 1932a:61,67, was later picked up by Hempel in his continuation of the debate between Schlick and Neurath; see below sections 8.2, 9.3.2, 10.2.5, 10.5.3, 11.2 and 11.3.
\textsuperscript{132}Carnap 1932d:181/2.
science, behaviourist psychology. It is important to note that Carnap here responded in
defense of the claim that the physicalist language was the universal language of science:
nothing was amiss if the physicalist language of science could not formulate what
distinguished the system of science as a symbol systems applicable to the data. What
Carnap did not say, but was committed to, was the view that the defense of the empirical
nature of scientific knowledge claims was a matter for epistemology. It was, of course, for
the purposes of epistemology that Carnap had defended the possibly of a phenomenalist
protocol language against Neurath. Here I come to an interesting consequence arising from
the stand-off at the end of stage two.

7.7.3 Neurath's and Carnap's Different Defenses of Empiricism

Let me note a problem closely related to the question raised by Zilsel. Carnap
continued to draw a distinction between protocol sentences and non-protocol sentences.
The question thus arises how the two classes of sentence were to be distinguished.
Consider the passage widely read to indicate Carnap's capitulation to Neurath's holism:

Not only laws, however, but also concrete sentences are formulated as
hypotheses [...] such as a sentence about an unobserved process by which
certain observed processes can be explained. There is in the strict sense no
refutation (falsification) of an hypothesis, for even when it proves L-
incompatible with certain protocol sentences, there always exists the
possibility of maintaining the hypothesis and renouncing the
acknowledgement of the protocol sentences. Still less is there in the strict
sense a complete confirmation (verification) of an hypothesis. [...] Further,
it is, in general, impossible to test even a single hypothetical sentence. In
the case of a single sentence of this kind, there are in general no suitable L-
consequences of the form of protocol sentences; hence for the deduction of
sentences having the form of protocol sentences the remaining hypotheses
must also be used. Thus the test of applies, at bottom, not to a single
hypothesis but to the whole system of physics as a system of hypotheses
(Duhem, Poincare).133

That Carnap noted the need to draw a distinction between protocol statements and other
factual statements is indicated by the fact that he did draw the distinction between

133 Carnap 1934/7:318.
hypotheses and protocols. Hypotheses were statements, universal or singular, which led to observable consequences only in conjunction with other sentences; protocols, by contrast, bore their empirical content on their sleeve.

Carnap clearly acknowledged that protocol sentences could be rejected. But what then distinguished protocol sentences from other factual sentences like hypotheses? Since protocol sentences could also be rejected the distinction between observational and theoretical statements could no longer be drawn in terms of their revisability. Since Carnap put no formal constraints on the physical sentences counting as protocol sentences—unlike Neurath who required them to be of a particularly complicated structure—he also could not distinguish them in terms of their syntactic surface structure. Given, furthermore, the rejection of the correspondence theory of truth implicit in his officially non-semantic syntactic approach, Carnap was unable to do as he was to in 1935 after his semantic turn: put this condition on protocol sentences that they should feature only observable predicates, and declare that the coherentist justification of protocol sentences was an additional and only supplementary way of testing them. The notion of observationality invoked here presupposed the notion of "direct testing" and the return of the correspondence theory.\textsuperscript{134}

How then could Carnap have distinguished the two classes of statements? The answer is that he simply did not do so in a principled way. (Hempel, who followed Carnap's Popperian strategy, in fact declared there to be no difference at all.\textsuperscript{135}) Protocol sentences were simply those sentences that were treated as temporarily the last instance in the process of testing factual sentences. That, however, raised the very question of what still ensured that the system at issue was composed of empirical sentences. What controls did remain to save the empirical nature of science, as Neurath challenged Carnap in his 1934 letter quoted above? The only control lay, so it seemed, in what the community of

\textsuperscript{134}See sections 8.3.2 and 8.3.3 below.

\textsuperscript{135}Hempel 1935b; see sections 8.2.3 und 9.4 below.
scientists decided to accept as a protocol sentence in particular and an empirical sentence in general.

But that was not the whole story. Recall that Carnap required two derivations of concepts, a materialistic and a phenomenalistic one. What still "saved" empiricism for Carnap then was this. The defense of empiricism was not to be expected from science itself. The concepts of empirical science had to gain the legitimation through epistemological analysis: could they be ohenomenalistically reduced? Carnap's continued conception of all scientific terms as verifiable in just the sense attacked by Neurath, namely as reducible to the given, stood in the service of defending empiricism. Carnap sought to distinguish the two classes of empirically acceptable and empirically unacceptable statements by means of distinguishing the types of terms they contained. Now Carnap did not say so explicitly, but an anecdote of his about an encounter with Neurath points in this direction. 136 Carnap once admonished a reading group organised by Neurath who sought to translate passages from Freud's psychoanalytic writings into physicalist discourse sentence-by-sentence. Against this approach Carnap urged that it was single terms that needed to be translated. 137 The moral to be drawn from this is that it suggests Carnap's approach to the problem of differentiating empirical from non-empirical statements: Empirical statements employed only concepts which stood in a relation of reducibility or translatability to the phenomenal protocol language. Carnap's defense of empiricism at the height of his syntactical period--in the face of his acceptance of Duhemian holism--thus turned on his continued belief into the possibility of an "original" protocol language. Since for Neurath, the phenomenalist reduction was a non-starter, his 1934 accusation that

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136The following construal is consistent with his refusal to abandon hopes for a phenomenological reduction as late as 1961 (Carnap 1961:v, vii).
137Carnap 1963a:58. This interests of Neurath's in Jahoda's memoir (1982). Note that Else Fraenkel-Brunswick called her 1954 "a return to an assignment suggested to the writer some twenty years ago by the late Otto Neurath, to clarify the standing of psycholanalysis in the framework of the unity of Science movement inaugurated by him at the time." (ibid.:273). (Neider 1977 recalls a number of unspecified Circlists to have been in analysis.) For some brief remarks of Neurath's on psychoanalysis see 1932a:80 and 1933:G607/8.
Carnap abandoned empiricism now becomes intelligible. (Against the mere choice for a different form of protocol sentences, that would seem to be an unduly harsh criticism.) Since, by his lights, methodological solipsism as a residue of idealist metaphysics, Neurath could hardly react otherwise to Carnap's defense of empiricism.

6.8 Conclusion

To conclude. My reconstruction of stage two of the protocol sentence debate shows that the dispute between Carnap and Neurath left unresolved three theoretical conflicts which differentiate "physicalism" from "radical physicalism". Apart from what I called their two "surface" disagreements, the physicalists Carnap and Neurath also possessed conflicting defenses of empiricism. Neurath drew the distinction between protocol sentences and non-protocol sentences in terms of their syntactic surface structure. Already due to their elaborate form Neurath's protocol sentences were clearly distinguishable from non-protocol sentences. Both Carnap and Neurath then preserved the empirical nature of science but did so differently. Carnap identified the class of scientific sentences as a whole in terms of the potential reducibility of their terms to a phenomenalist language. Neurath identified them in terms of their relation to protocol sentences and these in terms of their surface structure and ultimately in terms of speakers' behaviours. Unlike Carnap, Neurath sought to defend science from within.

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138 On the defense of Neurath's view against Schlick's and Popper's complaints that he had given up empiricism see sections noted in fn. 131 above.
Chapter 8

STAGE THREE:

CHALLENGES TO RADICAL PHYSICALISM

Abstract: Overview of stage three and stage four (8.1). Schlick's challenge to the physicalism of Neurath and Carnap and responses by Neurath and Hempel (8.2). Carnap's Paris congress attempt at a reconciliation of the warring factions which amounted to the renunciation of the position he held at the end of stage two (8.3). Inconclusive ending of the debate between Schlick, Carnap and Neurath and their stand-off (8.4). Appendix: Evaluate Popper's often overemphasised role in the debate.

I conclude my reconstruction of the protocol sentence debate with consideration of stage three; stage four can be, for reasons indicated below, mostly neglected.

8.1 Overview of Stages Three and Four

First, an overview of the remaining stages of the debate. Stage three is mostly taken up by Schlick's return to the debate from which he kept his public distance since the announcement of his obliquely anti-formalist conception of epistemology in the first issue of Erkenntnis.¹ As was to be expected from this earlier experiential turn, Schlick did not find the fallibilist position on which Carnap and Neurath came to agree by late 1932 to his liking. The discussions in the Vienna Circle continued to center around the question of the form, content and status of protocol sentences and in 1934 finally Schlick published a sharp criticism of the physicalist position. Schlick's response engendered rebuttals by Neurath and Hempel, the latter a rejoinder by Schlick, another rebuttal by Hempel, and a final coda by Schlick. At the Paris Congress in 1935, Carnap sought in vain to reconcile

¹See chapter 4.
the warring factions by presenting a new conception of his own; this I take to signal the conclusion of stage three. It ended with Schlick’s retention of his foundationalist outlook, Carnap’s shift to a yet different position with his semantic turn, and Neurath’s shoring up of his own version of the soon generally abandoned position of non-semantic physicalism under the name of "encyclopedism".

Stage four consisted mainly of further attacks on physicalism from a variety of quarters, often containing criticisms already made. Insofar as they were generally directed against Carnap’s final syntacticist position they had become irrelevant against Carnap after the Paris congress; insofar as they were directed against Neurath’s radical physicalism they universally failed to come to grips with Neurath’s coherentist theory of justification. The interest of this fourth stage of the debate consists, apart from chronicling the universal misunderstanding Neurath’s views were subjected to, only in Neurath’s return engagements which furnished further characterisations of his proposed naturalisation of epistemology. Since the latter are best dealt with on the occasion of the full reconstruction of his positive theory of knowledge, not undertaken here, I shall not follow the debate into its fourth stage.

8.2. Schlick’s Foundationalist Challenge To Physicalism

The argumentative line pursued by Schlick’s challenge to physicalism had previously been partly explored by Zilsel in 1932 who, in effect, first applied some of Schlick’s criticisms of the Aufbau to his syntactic theorising. In the defense of empiricism, Schlick was not willing to put up with what he took to be the physicalists’ irresponsible handwaving towards merely contingent facts, the agreement of the the scientific

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community. Moreover, there was Carnap's claim that what Schlick claimed to do could not in fact be done, namely, exhibit the correspondence of statement and fact. Schlick held to his Tractarian views throughout the protocol sentence debate: if logical form could not be described, but had to disclose itself to the cognising consciousness as meaning, then acquaintance with the very material of cognition was required. (It was not required, mind you, to spell out a meaning that was non-structural, but to provide experiential access to the medium through which the logical form could disclose itself as meaning.) While Schlick agreed that all meaning was a matter of structure he nevertheless insisted that the inexpressible intuitive material of cognition be taken recourse to in meta-scientific inquiry, and that it could only be recovered in the experiential turn of his own epistemology. Schlick's concern, in other words, echoed Zilsel's, still further steeped in Tractarian presuppositions. Schlick was saved the trouble of arguing his Tractarian point against Carnap by the fact that the other physicalist, Neurath, even denied the kind of phenomenalist defense of empiricism Schlick (and Carnap) required. It was against this denial that Schlick's 1934 critique of physicalism argued. Thus, while Zilsel's query focussed upon Carnap's version of physicalism, Schlick's critique was directed mostly against Neurath's radical stance. It was only due to Hempel's intervention that Schlick was forced to discuss his Tractarian presupposition.

8.2.1 Schlick's Rejection of Radical Physicalism

As the title—"Ueber das Fundament der Erkenntnis" ("On the Foundation of Knowledge")—and his introductory passages indicate, Schlick found nothing wrong with the "wish for absolute certainty" in epistemology. Indeed, the problem of the form and content of protocol sentences was, for him, "the latest form in which [...] the decisive empiricism of our day clothes the problem of the ultimate ground of knowledge." Protocol

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3See above chapter 4.
4Schlick 1934:209.
sentences were to provide this ground; Schlick thought of them as "statements which express the facts with absolute simplicity, without any moulding, alteration or addition."⁵ They were to be distinguished by certainty; such sentences expressing the facts without any adornment stood "at the beginning of science."⁶ For Schlick, the question of the protocol sentences was the question "of the criterion of truth."⁷ Because Carnap and Neurath had considered the question of the foundation of knowledge only in the sense of the temporal and not logical priority, their conception of protocol sentences resulted, so Schlick, in a "peculiar relativism"⁸ according to which truth could only consist in a mutual agreement of sentences.

Neurath was singled out particularly here. Schlick read him as putting forward a coherence theory of truth and suggested that his view resulted from presupposing, and at the same time denying, "correspondences with reality":

The astounding error of the "coherence theory" can be explained by the fact that its defenders and expositors were thinking only of such statements as actually occur in science, and took them as their only examples. Under these conditions the relation of non-contradiction was in fact sufficient but only because these statements are of a special character. They have [...] their 'origin' in observation statements.⁹

Schlick argued that, if one wanted to reject arbitrary but coherent fairy tales, then something had to be added to the requirement of coherence. Schlick next considered the view that this criterion be the economy principle: "we are to choose those as basic statements whose retention requires a minimum of alteration in the whole system of statements in order to rid it of all contradictions."¹⁰ Even that would not distinguish basic sentences for once and all. Furthermore, one would have to know to which sentences the criterion of

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⁵Ibid..
⁷Ibid.:213.
⁸Ibid.:212.
⁹Ibid.:215.
¹⁰Ibid.:216.
economy is to be applied. Schlick concluded that the additional condition needed above coherence would be the criterion of truth. "So we find ourselves once more referred to the question of their origin."\textsuperscript{11} Once sentences were ordered in this respect "there is no place left for the application of the principle of economy (apart from certain very special cases in still unfinished areas of science)."\textsuperscript{12} Schlick clearly presumed that an ordering of sentences according their origin, the causal history of their production, would turn up a certain class which we would not challenge because of this origin or causal history.\textsuperscript{13}

It is important to note that Schlick agreed with Carnap and Neurath in this respect: protocol sentences, once expressed by means of physical marks and intersubjectively intelligible, were "anything but incontrovertible".\textsuperscript{14} They may be eliminated and declared the result of some error, and even our own protocols were not exempted. All this, however, only showed, so Schlick, that the question of the protocol sentences was as yet falsely conceived, and so failed to yield the foundation of knowledge.

Its essential deficiency lies in ignoring the different rank of statements which expresses itself most clearly in the fact that for the system of science which one takes to be the "right" one, one's own statements play in the end the only decisive role.\textsuperscript{15}

Schlick's own solution was that the basic sentences providing the foundation of knowledge would be in the first-person present-tense form "expressing the immediately observed".\textsuperscript{16} These "Konstatierungen" (translated variously as "affirmations" or "constatations") provided the temporal starting point of scientific inquiry and its endpoint for they also serve in the verification of hypotheses. If a predicted experience occurs

we obtain a feeling of fulfillment, i.e. characteristic satisfaction. It is obtained in the very moment in which the confirmation takes place [...] thus

\textsuperscript{11}Ibid.:217/8.
\textsuperscript{12}Ibid.:218.
\textsuperscript{13}Ibid.:220.
\textsuperscript{14}Ibid.:212.
\textsuperscript{15}Ibid.:219.
\textsuperscript{16}Ibid.:220.
the function of the statements about the immediate given lies itself in the immediate present.  

In this latter function basic sentences show themselves to constitute "an absolute end" and thus to be of logical priority as the final arbiter in the construction of science.

Schlick declared affirmations to be synthetic statements but to be like analytic ones in respect of their indubitability.

I grasp their meaning as soon as I grasp their truth. In the case of a confirmation it makes as little sense to ask whether I might be deceived regarding its truth as in the case of a tautology. [...] they are the only [synthetic] sentences that are not hypotheses.

What needs to be noted is at what price the unrevisability of affirmations was bought: they have so to speak no duration, [...] the moment they are gone one has at one's disposal in their place inscriptions or memory traces, that can play only the role of hypotheses and thereby lack ultimate certainty.

Not only did these affirmations contain demonstratives (which may not be replaced by spatio-temporal coordinates), but no perception terms (they would thus look like "Here now blue"); they were, furthermore, fleeting to the point of inexpressability: "[a] genuine constatation cannot be written down, for as soon as I inscribe the demonstratives 'here', 'now', they lose their meaning." It was this inexpressability of Schlick's constatations which disqualified them in the opinion of the physicalists and placed them in the company of Wittgenstein's rejected 'elucidations'.

8.2.4 Neurath's Rejoinder to Schlick

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17 Ibid.: 222.
19 Ibid.: 222.
20 This is noted in Hempel 1982: 5 and Hilpinen 1982: 72.
21 Ibid.: 226.
Neurath's response, "Radikaler Physikalismus und 'wirkliche Welt'" ("Radical Physicalism and the 'Real World'"), consisted in specifying and defending four points of disagreement with Schlick. They were 1) that radical physicalism lacked the firm ground of absolute certainty, that 2) it lacked an unambiguous criterion of truth, that 3) it did not speak of agreement between knowledge and reality, and that 4) it did not acknowledge unrecordable affirmations after Schlick's fashion. Only with respect of 4) did Neurath return Schlick's criticism with equal sarcasm and pointed out that constatations which could not be recorded remained "alien to science"\textsuperscript{22}, for science did not deal in the inexpressible. With respect to the remaining points Neurath was concerned as much with clarifying his own position as he was with criticising Schlick's.

As was to be expected, Neurath reaffirmed, against 1), the Neurath-principle: all statements of science were selected by decision and could in principle be revised. Science did not provide certainty. Against 2), Neurath remained unrepentant: no unique criterion of truth could be given. He reasserted his own understanding of how the notion of truth was to be used.

We shall call a statement 'false' if we cannot establish conformity between it and the whole structure of science; we can also reject a protocol sentence unless we prefer to alter the structure of science and thus make it into a 'true' statement.\textsuperscript{23}

Nevertheless, Neurath claimed to have been unjustly placed with coherence theorists of truth.\textsuperscript{24} In an aside, Neurath referred to Schlick's early work on truth as a correct refutation of idealist conceptions of truth. Here Neurath could have (but did not) pointed out that Schlick himself stated that

\textsuperscript{22}Neurath 1934a:113.
\textsuperscript{24}Neurath 1934a:101. His added comment that his position "could at least be classed" with the economy standpoint should not be taken as indicating acquiescence into the conception criticised by Schlick: it must be remembered that Neurath aimed for the reinterpretation of truth talk that substituted acceptance conditions for correspondence.
The verification proper always comes down in the last resort to a recognition of the identity of two judgements, one of which is a judgement of perception.25

Was this so different from Neurath's view that statements are always only compared with statements? What did Neurath do, but turn Schlick's early talk of "judgement" into the "formal" mode? Schlick, of course, already in 1910 had added the qualification that the judgement of perception is one "whereby an actual experience is immediately expressed", and developed the criterion of truth as the unique correspondence of judgement and fact.26 Both of these additions Neurath rejected, of course, and therein lay their dispute.

Was Neurath committed then to accepting consistent fairy tales because he rejected talk of "conformity with reality"? In response to 3), Neurath reasserted his rejection of the possibility of such comparisons, rejected Schlick's allegation and undertook to specify further his own empirical orientation.

Certainly we too have a court to appeal to, one that is formed by the protocol statements accepted by us; but it is not finally fixed. We do not renounce the judge but he is replacable. "All factual statements can be traced back to a certain section of the totality of statements, namely the protocol statements. Within the system of statements the protocol statements are the last to which one refers back." [...] We find the consistent application of the principle to always compare one statement with another logically also where, instead of protocol statements, other accepted statements are used as control statements.27

Both Neurath's own and Popper's proposal as accepted by Carnap (and, as we shall see, Hempel) led to the rejection of Schlick's talk of comparing sentences and reality.

Neurath's own method (unlike Popper's, he may have added) specified what the last instance of control should be: linguistic forms of a particular type, namely, observation sentences "carefully worded" as protocol sentences.28 Furthermore, not only did theories

25Schlick 1910:78.
26Ibid.:94.
27Neurath 1934a:107; the quote is from his 1933:G539. I have changed the translation from "content statement" to "factual statement".
have to conform to with a particular class of sentences, but they had to conform also to the requirement that this set be the maximal set of acceptable protocol sentences.

Thus our striving after knowledge of reality is reduced to striving to establish agreement between the statements of science and as many protocol statements as possible. But this is very much; in this rests empiricism. For if, by our "resolution", we grant so much weight to the protocol statements that in the last resort they decide the validity of a theory, our 'new scientism', in spite of the stress it lays on on logic, does not deviate from the old program of empiricism, which is even reinforced by pointing out that protocol statements too can be physicalistic statements.29

To give up empiricism, as he was accused of doing--even after his response to Schlick by Popper, Russell and Ayer, for example30--was clearly the last thing Neurath had in mind:

I do not believe that Neo-Platonism can be represented as a consistent system whose predictions can be verified by protocol statements accepted by us. [...] The renunciation of confronting all predictions in the last resort with protocol statements can probably not be maintained without endangering the fundamental empirical standpoint.31

Note then that Neurath clearly affirmed that he was able to give a criterion for the empirical conduct of science: the confrontation of hypotheses with a special class of statements, protocol statements. Unlike for Carnap, not any factual sentence could serve in this capacity for Neurath. These statements had to state both the stimulation state of a person and his relation to the environment.32

Neurath accepted, of course, what Schlick found unacceptable. At issue here was not the thought that more than one system of higher-level hypotheses could agree with accepted protocols: Schlick agreed with Neurath on this point.33 What remained at issue between him and Schlick was, so Neurath, basically only whether the evidential base of scientific theories could be characterised uniquely. Here we return to Neurath's point

29Ibid.:109, my italics.
30Popper 1935:95f., Russell 1940:148, Ayer 1940; see section.10.2 below.
31Neurath 1934a:106 and ibid.:107/8, his italics.
32See above sections 7.4 and 7.5 2.3, and chapter 9, and sections 10.5, 11.2 and 11.3 below.
33Schlick 1917.
against Schlick's demand for certain foundations. Against Schlick—as against Carnap—Neurath held that the immediate precise and untheoretical "given" was a fiction.

Though we can coordinate precise mathematical formulations with our imprecise observation statements, the assumption that one would have to arrive at precise elementary statements if only one would have sufficient intelligence at one's disposal leads to a fiction that resembles Laplace's spirit—a perfectly metaphysical notion. Here too we have a metaphysical endeavour to put the unambiguity of 'atomic statements' or 'constatations' as the eternal unambiguous reality against the fluctuations of humanly paltry science.34

It is clear that "metaphysical" does not merely mean "ontologically objectionable" to Neurath. Anything which "absolutises" the means of human cognition into a, or particularly the, complete system also counted as metaphysics; for Neurath, any conception which made believe that we were not "at sea" epistemologically was "metaphysical"35: "Never is even only one sphere of science completed, as Schlick thinks; all are connected with all others and thus somehow participate in the general incompleteness and uncertainty."36

Is it true that are all scientific statements uncertain to some degree? Schlick had likened his constatations to analytic sentences in respect of their certainty. Neurath made the prima facie startling claim that "[t]he statements of logic and mathematics are also not 'certain'."37 Here, finally, Neurath seems to have overdone it—the sentences of logic and mathematics uncertain?—but did he? Before passing this judgement, note that even analytic sentences were, following Carnap, valid only relative to a previously conventionally chosen linguistic framework, and thus were formally true, and thus certain, only conditionally. But still apart from this point, it must be noted that Neurath here argued (as

34Neurath 1934a:1067.
35As Menger recollected, while other members of the Circle usually called objectionable doctrines "meaningless" Neurath preferred to call them "metaphysical" (Menger 1979b.) This wide Neurathian notion of "metaphysics" would seem to be equivalent to his term 'pseudo-rationalism' and like the latter date back, in spirit if not in letter, to his first Boat of 1913.
36Neurath 1934a:110.
37Ibid.:104.
always) from the point of scientific practice. "'Certainty' is a term to be defined within the doctrine of human 'behaviour'"\textsuperscript{38}; for Neurath, it was not a logical term.

If Schlick thinks that I have only understood a statement when I know whether it is analytic or synthetic, what about the case when I declare a statement analytic today and reach another opinion tomorrow, declaring that I had been wrong and not understood the statement yesterday. I have no means available at all to reach a final verdict about whether a statement was understood by me or not - this is a typical pseudo-formulation.\textsuperscript{39}

What Neurath was concerned about was our knowledge of formal systems, and there he pointed out that it was no better insulated against error in than empirical knowledge. Thus he quoted Karl Menger's view that even the "activity of mathematicians is not in general contradiction-proof"\textsuperscript{40} If challenged, even mathematicians referred and deferred to a sort of protocol sentences - namely those stating results of computations which, as all human activities, were fallible. Neurath did not then challenge the analytic-synthetic distinction as such.\textsuperscript{41} What he did challenge was that this distinction provided a fixed point for human thought. Neurath challenged the belief that philosophical solutions to problems in the theory of knowledge bought for the price of idealising abstractions from the practice of were not diminished in their explication claim. It was, after all, on the presupposition of the analytic-synthetic distinction that Schlick's conception of philosophical inquiry--and even Carnap's physicalism--was built.

Neurath was not entirely alone with this contention. Frank had shown in 1932 that the analytic-synthetic distinction proceeded from the assumption that the naming of things could be fully isolated from any other events in the world, and that this assumption was not one that commanded inevitable agreement. Frank had noted an important point about the determination of tautologies.

\textsuperscript{38}Ibid.: 110.
\textsuperscript{39}Neurath 1934a: 104; compare also his 1936a: 766.
\textsuperscript{40}Menger 1933a: 104.
\textsuperscript{41}Compare Neurath 1932a: 60.
Tautological sentences do not say anything about the world of experience but only about the manner in which we designate experiences; they concern the relations between different designations of the same experience. But there can be no doubt that the designation of an experience is a process in the real world and that one cannot rule out from the start that the way of designation has an influence on the process in the real world.42

Drawing the distinction between tautological and non-tautological sentences of the scientific symbol system "presupposes" that the process of changing the designations of signs can be "isolated from all other processes". Frank's point was obviously not lost on Neurath: knowledge of meaning was no better off than knowledge of the natural world, Neurath concluded: neither could lay claim to indubitability.

8.2.5 The Hempel-Schlick Exchange

Neurath's response to Schlick was not the only one from the camp of more or less radical physicalists. It seems to me that if Neurath's rejoinder had been the only one then Schlick would have let matter rest. As Schlick noted, he did not take seriously Neurath's allegation to have joined the metaphysicians and poets. What led him to defend his views was rather Hempel's "clever article"43 "On the Logical Positivists' Theory of Truth".

Just as Neurath pointed out that talk of protocol sentences proceeded in what Carnap called the "formal mode of speech" (talk of "experience sentences" instead of talk of "personal experience" etc.) in order to "avoid traditional pseudo-problems"44, so Hempel stressed that

the concept of truth may be characterised in this formal mode of speech, namely in a crude formulation, as a sufficient agreement between the system of acknowledged protocol statements and the logical consequences which may be deduced from this statement and other statements which are already accepted.45

42 Frank 1932:28.
44 Neurath 1934a:101/2.
45 Hempel 1935a:54.
Like Neurath, Hempel stressed that "nowhere in science will one find a criterion of absolute unquestionable truth". And similarly to Neurath, Hempel argued that, given that constataions cannot be written down

it is impossible to understand how a "Konstatierung" may be compared with an ordinary scientific statement. And such a comparison would be necessary, as Prof. Schlick assumes that every empirical statement is in the end tested by "Konstatierungen".

I believe that Schlick's dismissal of Neurath's rejoinder can only be explained by the somewhat strained personal relation between Schlick and Neurath. (The standard objection against Neurath's vagueness is, in this case, pedestrian: Schlick was not doing too well on this score himself.) Hempel's points against Schlick—though not Hempel's own views—were essentially the same as Neurath's.

Schlick responded to Hempel by defending his talk of comparing facts and propositions by discussing as an example the case where he checked a statement in his Baedeker travel guide against reality.

[What on earth could statements express but facts?] I am saying that certain black marks in my Baedeker express the fact that a certain cathedral has two spires is a perfectly legitimate empirical assertion.

Schlick attempted to dismiss the anticipated counter-suggestion that under analysis this amounted to nothing but a comparison between "propositions" by noting that

we can distinguish between cases in which a written, printed or spoken sentence is compared with some other written, printed or spoken sentence, and cases like our example where a sentence is compared with the thing of which it speaks.

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46Ibid.:55.
48See Neider 1973, 1977, but also compare Stadler 1982c.
49Schlick 1935a:67.
50Schlick in this response in English had switched to talk of propositions as equivalent to talk of "statements" ("Aussagen") or "sentences" ("Saetze"). Neither of the Vienna Criclists paid much attention to the distinction between sentences, statements and propositions in their writing.
51Ibid.:66.
Schlick rejected Hempel's claim that "the structure of facts" cannot be ascertained: "Since you do not deny the existence of facts [...] why should you deny that we can know their structure?"\textsuperscript{52} Clearly Schlick did not think the formal mode of speech "much more correct"\textsuperscript{53}: he rejected the idea that talk of meaning as correspondence (however established) was illegitimate.

Schlick's first response to Hempel, then, consisted in declaring their disagreement to be a terminological one, and reserving for himself the right to speak as he did. On a more constructive note, however, he offered this clarification. Whereas his 1934 article stated that he was concerned with the question of the logical priority of the sentences by which scientific statements were tested\textsuperscript{54} he now stated that he sought to answer a "psychological question", namely, "why I myself (the individual observer) accept [the system of science] as true."\textsuperscript{55} As a response to this psychological kind of question, he reaffirmed his conception of "those simple experiences" he spoke of as "constatations".\textsuperscript{56}

Hempel's rejoinder consisted in reaffirming two of his earlier points. First, that Schlick's example of the comparison between facts and what his travel guide said did after all amount to a comparison of statements only, namely those of the travel guide with those expressing "the result (not the act!) of his counting the spires."\textsuperscript{57} These two statements were compared logically by establishing their compatibility by purely syntactic means. Second, Hempel pointed out that talk of comparison with facts in this sense was dependent upon the rules of the language adopted. Such a comparison might lead to a positive result in one linguistic framework and to a negative one in another. Talk of statements "expressing facts" was accordingly

\textsuperscript{52}Ibid.:68.
\textsuperscript{53}Ibid.:67.
\textsuperscript{54}Schlick 1934:212.
\textsuperscript{55}Schlick 1935a:69.
\textsuperscript{56}Ibid.:70.
\textsuperscript{57}Hempel 1935b:94.
not quite harmless (though, of course, not false either) [...] for this term indicates something which is forever fixed with all its characteristics, whilst it is essential for the system of scientific statements that it may always be changed again, that no proposition is adopted once and for all, and, in addition, that the adoption has, after all, the character of a convention [...] And the character of statements which are adopted by a convention evidently does not admit of such questions as: Are there statements which express facts adequately, which are absolutely true [...]?

Hempel was not persuaded that, even after clarifying his psychological concern with the foundation of knowledge, Schlick had cleared up all divergences of opinion as misunderstandings.

To show how Schlick's clarification easily appears inadequate, let us read Schlick's talk of affirmations as a concession to the formal mode of speech. With affirmations "disrobed" of their formal mode clothing, Schlick argued for the need to grant that primacy to experience which he thought simple empiricism required. What was to ground science by fixing unique correspondences was pure experience. For this to ground scientific theories, it was required that, in some kind of unexplained process, experience ("erleben") was turned into cognition ("erkennen"). This was problematical, not only if Schlick was to remain consistent with his earlier views, but also because this process remained wholly mysterious. (Just that, of course, had been Neurath's point.)

8.2.6 Schlick's Coda

Schlick's coda to the discussion, the introduction and part three of "Sur le Fondament de la Connaissance" ("On the Foundation of Knowledge"), made clear that the disagreement, in the end, was over whether Carnap's syntactic program of the logic of science adequately encompassed all that the theory of science and epistemology took for its task. Hempel thought epistemological questions to be adequately treatable in Carnap's fashion, but Schlick did not think so.

58Ibid.:95/6.
Schlick approached this topic by offering an alternative conception of the language of science as a subject matter of philosophical inquiry. "[B]y 'statement' we mean neither the mere signs, nor the thoughts or facts corresponding to them, but (with Wittgenstein) the rules of use conventionally coupled with the sentence [...]"\textsuperscript{59} With 'sentence' understood to express a statement in this sense, Hempel's last quoted return-objection obviously did not apply anymore, for it traded on a definition of 'statement' as a pattern of written or spoken symbols alone. But Schlick went yet further: for him, the rules "do not merely relate, as in logic, to combinations of linguistic signs, but also to the employment of language in life and in the activity of research (where ostensive definitions are utilised)."\textsuperscript{60} What Schlick meant here was that the syntactic rules specified in the axiomatisation of Carnap's linguistic framework failed to comprehend all uses of statements. They only specified uses of statements within the logical system of scientific propositions.

It was here that Schlick's talk of the rules of language use gained its bite against the physicalists. Like Carnap at an earlier stage, Schlick believed that the protocol statements were not part of this system.\textsuperscript{61}

Even if it be true that all propositions occurring within the system of science must be viewed as hypotheses, this fact does not entitle us to declare all statements whatsoever to be hypothetical; there are many propositions which undoubtedly deserve the name of 'statements', but are possessed of properties quite different from those of hypotheses, and hence should not be so called. They do not occur within science itself, and can neither be derived from scientific propositions, nor the latter from them; they are therefore ignored by those who are interested only in logical deductions, the internal rational concerns of science. Yet they play the most important role of all in the psychological issue concerning the foundation of all knowledge.\textsuperscript{62}

Not being part of the system of scientific propositions, the use of these fundamental statements required rules not specified by Carnap's frameworks.

\textsuperscript{59}Schlick 1935d:408.
\textsuperscript{60}Ibid..
\textsuperscript{61}Noted by Hilpinen 1982:76n.
\textsuperscript{62}Schlick 1935c:406-7.
All statements having the character of hypotheses can be false on two and only two grounds: whether an error is present, or a lie. But a false affirmation is always a lie, that is simply the logical rule which applies to it.\(^63\)

What he previously had characterised as a pseudo-analytical trait of affirmations, their certainty and unrevisability, Schlick now spelt out as special rules for statements outside of the system of science. In addition, Schlick argued that the rules governing the use of constatations must forever lie beyond the reach of Carnap's method of explicating the meaning of statements: they "cannot be wholly written down, because they ultimately lead to ostensive definitions which are not replaceable by any description."\(^64\)

It turns out, then, that Schlick's experiential turn of 1929 and his attack on coherentist physicalism in 1934 and 1935 did not violate his own earlier strictures on making a strict separation between knowledge and experience: the meaning which disclosed itself to consciousness in the experience of a given fact was, after all, expressible in a statement, albeit not one which obeyed the rules of the language of science. Schlick thus remained consistent in his rejection of intuition as an instance of knowledge. With this clarification of his understanding of "affirmations", and why they cannot be written down (namely, because the rules governing them can never be fully specified), the battle lines of the debate also became clearer. Against Carnap's formalist approach, Schlick's argument urged that the syntactic method could not presume to have incorporated the appeal to experience necessary to uphold empiricism. Such appeal lay "logically" prior to the making of a scientific statement within the system of scientific propositions in this sense: affirmations were the psychological "occasions for framing"\(^65\) the latter.

\(^{63}\)Schlick 1935d:410.

\(^{64}\)Ibid.:413.

\(^{65}\)Ibid.:412.
Schlick's clarification of his views did not establish the success of his views either, however. If an affirmation was not a lie, it had to be true. All scientific statements, as hypotheses, could be false. Now, a protocol statement, as a statement of the system which allowed for the deduction of predictions etc., also could be false. In what sense then did the constatations--being indubitable in Schlick's sense they could only attest to how one was being appeared--lend certainty to scientific knowledge claims? Schlick's realm of certainty remained divorced from that of science! In the course of the development of his 1929 position, and especially in the course of his clarifications subsequent to his 1934 paper, it became apparent that Schlick's original concern to show science well founded was unanswerable with the means at his disposal.

8.3 Carnap's Attempt at Reconciliation

Carnap reentered the protocol sentence debate once more at the Paris Congress in the fall of 1935. Carnap noted with relief Schlick's clarification of the psychological nature of his concern, and urged care to keep logical inquiries separated from psychological ones henceforth. His own logical proposals sought to reconcile the apparently inevitably opposed positions of Schlick and his followers and the more or less radical physicalists.

8.3.1 The Discovery of Tarski's Theory of Truth

Carnap's reentry into the debate followed his acceptance of Tarski's definition of the concept of truth. The concept of truth, Tarski proposed, was a meta-linguistic predicate which, on pain of paradox, could only speak about sentences of an object language, not sentences of the language it itself belonged to. So understood, the concept of truth was adequately captured by determining the extension of "is true" in the object-language in a mathematically precise fashion. Tarski's example was "'Snow is white' is true if and only

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66 Carnap 1936c:367. As in his 1936a where he sought to dispell yet another confusion (see 8.3.2 below) Carnap did not mention the culprit's name, here: Schlick's, there: the physicalists' presumably.
if snow is white"; here the same sentence, "Snow is white", was first mentioned and then used. Tarski suggested that the two occurrences of the apparently same sentence (type) constituted in fact two occurrences of sentences of distinct languages, an object and a meta-language respectively: the sentence of the object language was mentioned and the sentence of the meta-language was used. Tarski proved that once the distinction between object and meta-language had been drawn, the predicate 'is true' could be defined by determining its extension for the object language with the help of the translation or disquotation of sentences of the object-language into the meta-language. In general, for the determination of the truth predicate of an object language, one needed to use the truth predicate of the meta-language: the predicate 'is true' for the meta-language in Tarski's example was not itself defined; this could only be done in a meta-meta-language etc.

The importance of this mathematical discovery for Carnap was the following. In his Logical Syntax Carnap held that 'true', unlike 'analytic', was not a logical, namely, syntactical predicate. Since meta-logic (and thus, by his lights, the philosophy of science and epistemology) were confined to purely formal investigations, Carnap thought any talk of truth other than in its purely formal variety, analyticity, to be illegitimate: as the liar paradox showed, unrestricted talk of truth led to contradictions and, failing a fool-proof way of restricting such talk, had to be banished from the "logic of science". Tarski's discovery convinced Carnap that such a way of restricting talk of truth had been found. The notion of truth had become scientifically respectable. Furthermore, the "logic of science" needed no longer be restricted to the investigation of the syntax of language, now it could explore the newly opened field of semantics: linguistic frameworks could be stipulated not only in terms of their vocabulary and formal rules of formation and transformation, but also in terms of meaning rules. The reference or designation relations of terms could now be defined. What previously had to count as talk in the misleading

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67 Carnap 1934/7:216.
material mode of speech (and had to be substituted for by hook or crook in the formal mode) now became a bona fide object of the logic of science.

8.3.2 The Distinction between Truth and Confirmation

It did not escape Carnap that Tarski's result had consequences for the ongoing dispute about the form, content and status of scientific protocols. As Schlick's remarks showed, much of the debate turned on specifying the conditions of the verification or falsification of scientific hypotheses by test of their predictions. The formal mode prescribed in Logical Syntax forbade talk of meaning as such, namely, as what an expression stood for or referred to, and thus talk of truth as correspondence. Clearly then, Tarski's logically impeccable notion of truth for formalised languages (an important qualification for many of Tarski's and Carnap's early addressees) was just the ticket to defuse an apparently interminable, basically terminological dispute. So Carnap thought.

Thus Carnap declared in his Paris lecture "Wahrheit und Beweisung":

The difference between the two concepts 'true' and 'confirmed' ('verified', 'scientifically accepted') is important and yet frequently not sufficiently recognised. 'True' in its customary meaning is a time-independent term; i.e. it is employed without a temporal specification. For example, one cannot say that "such and such a statement is true today (was true yesterday; will be true tomorrow)" but only "the statement is true". 'Confirmed', however, is time-dependent. When we say "such and such a statement is confirmed to a high degree by observations" then we must add: "at such and such a time".68

Carnap noted that the earlier logical misgivings against the unrestricted use of true "brought it about that the term 'true' was used in the sense of the entirely different concept 'confirmed'." Since Tarski's discovery provided for the required restriction of truth talk

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68Carnap 1936a:18/1949:119; this paper, combined with his 1946 "Remarks on Induction and Truth" was combined and translated by Feigl in 1949 under the translated title of the original "Truth and Confirmation" (Carnap 1949). Words dropped from the English translation restored between strokes.
the term 'true' should no longer be used in the sense of 'confirmed'. We must not expect the definition of truth to furnish a criterion of confirmation such as is thought in epistemological analyses.⁶⁹

In so many words then, Carnap here declared that neither he nor Neurath had intended to provide a coherence theory of truth. Their talk was, in fact, confirmation talk.

In the confirmation of statements Carnap distinguished the direct and the indirect testing, the latter proceeding with the help of another statement which in turn was directly tested and was entailed by the statement in question. With the concept of truth as correspondence explicated by Tarski, Carnap also distinguished within the process of confirmation two components, the process of the "confrontation of a statement with observation" and the process of the "confrontation of a statement with previously accepted statements". Of these two operations

[The first one is more important. Without it there could be nothing like confirmation. The second one is |only| an auxiliary operation. Its function is mostly negative or regulative: it serves in the |subsequent| elimination of incongruous elements from the system of statements in science.⁷⁰

Clearly, Carnap was prepared to redeem talk of "comparing statements with facts".

If 'comparison of [a] statement with fact' means the procedure which we called the first operation then it must be admitted that this procedure is not only possible, but even indispensable for scientific testing. Yet it must be remarked that the formulation 'comparison of statement with fact' is not unobjectionable. [...] Confrontation is understood to consist in finding out whether one object (the statement in this case) fits the other (the fact); i.e. as to whether the fact is such as it is described in the statement, or, to express it differently, whether the statement is true to the fact.⁷¹

'Comparison' would be the wrong term, so Carnap, because, first, statement and fact are not compared in respect of a property they share, and because, secondly:

⁶⁹Ibid.:19/ibid.:120.
⁷⁰Ibid.:21/ibid.:125.
⁷¹Ibid.:21/2/ibid.:125.
The answer to a question concerning 'reality' however depends not only upon that 'reality', or upon the 'facts' but also upon the structure (and the set of concepts) of the language used for the description.\footnote{\textit{Ibid.}:22/ibid.:126.}

Thus Carnap did not only support two of Hempel's (and Neurath's) criticisms of Schlick's formulation of his position, but also Schlick's view of the primacy of the observational confrontation in a fashion he thought acceptable to his fellow physicalists. Clearly, Carnap believed that if the warring factions were to follow his own semantic turn their disagreement would evaporate.

\textbf{8.3.3 The Liberalisation of Empiricism}

To complete the review of the 1935 innovations in Carnap's logic of science we must register also another change in the conception of the scientific language first announced at the Paris Congress.\footnote{Carnap 1936b.} This change concerned his claim, in \textit{Logical Syntax}, that "the various possibilities of translating a thing-sentence into an equipollent sentence are obviously not incompatible with one another."\footnote{Carnap 1934/7:301.} The two possibilities Carnap had had in mind there were languages containing only sense-data designations and languages containing only space-time coordinates and physical descriptive functors. Both languages were held to be fully intertranslatable. In 1935, Carnap had to withdraw the claim that the physical and the phenomenal language were equipollent, i.e., fully translatable.

Carnap had come to recognise that disposition terms present a difficulty not taken account of heretofore. In 1932, for example, psychological terms (including the terms of the phenomenal and auto-psychological language) were defined as dispositional terms and their integration into the scientific language was held to unproblematical, once the disposition was specified in terms of stimulus and response.\footnote{Compare, e.g., Carnap 1932c:E131.} The problem was that the envisaged
definition in terms of a conditional was false. The envisaged definition was: 'an object/organism \( q \) has the dispositional property \( p \) if and only if when subjected condition/stimulus \( s \), \( q \) exhibits behaviour \( r' \). The problem was that, given a false antecedent, the definition would apply, but clearly miss "the intended meaning".\(^7\) In 1935, by contrast, disposition terms were admitted into the scientific language as long as the experiential or operational conditions by which the obtainment of the dispositional property was tested were defined in fully observational terms. Instead of the explicit definition of theoretical terms, Carnap now only required so-called "reduction sentences". They were of the form: 'if an object/organism \( q \) is subjected to condition/stimulation \( s \), then \( q \) possesses the dispositional property \( p \) if and only if \( q \) exhibits behaviour \( r' \). (This Carnapian usage of "reduction" does not, of course, conform to modern usage where it does denote eliminability.) Thus Carnap came to admit into the scientific language also terms which were not eliminatively definable by its basic predicates, be they physicalistic or phenomenalistic.\(^7\) Together with the admission of laws into the scientific language proper--already effected in Logical Syntax\(^7\) --this move of Carnap's was later recalled as the "liberalisation empiricism".\(^7\)

Neurath's reaction to this turn of Carnap's thinking was positive. In his Congress report he wrote:

Carnap developed the thought that one can make do with the language of physicalism by showing that it is insufficient to use only definitions which allow the definiendum to be replaced by the definiens but that the program of physicalism can only be realised once the concept of "reduction" is

\(^{76}\)Carnap 1936/7:440.

\(^{77}\)In his soon to follow "Testability and Meaning" Carnap presented his proposals more precisely (and popularised them more widely): there he distinguished between synthetic reduction pairs and analytic bilateral reduction sentences whereas he had only noted the need for the latter at the Paris congress. For a full discussion of these moves of Carnap's and further refinements see especially Hempel 1963.

\(^{78}\)Carnap 1934/7:321. In his 1932b:E450 Carnap had still considered laws as general formulae for the derivation of predictions, thus following Schlick's Wittgensteinian 1931 lead of considering laws to be outside of the scientific language on account of their unverifiability. Neurath also underwent a conversion similar to Carnap's: compare his 1931b:53 and 1932a:62 to his 1933:G390.

\(^{79}\)Carnap 1963a:56.
introduced. This provides for the possibility to reduce one concept to another without allowing replacement. Thus the symmetry between sentences of the "phenomenal" and the "physical" language—up to now regarded by many with suspicion—disappears.\textsuperscript{80}

It must be noted that that the conclusion drawn by Neurath was not the conclusion which Carnap drew! In his Congress talk, Carnap instead still spoke of a "complete formal analogy" between the theses of positivism (phenomenalism) and physicalism.\textsuperscript{81} Carnap only pointed out that, due to the recognition of the mere reducibility of disposition terms, sentences of the phenomenal and the physical languages were no longer to be viewed as straightforwardly intertranslatable.\textsuperscript{82} Carnap did not declare this to speak for physicalism and against phenomenalism. Neurath jumped the gun here: Carnap declared the relation between the phenomenal and the physical language to be only "more complicated" than envisaged by the intertranslatability thesis.\textsuperscript{83} Carnap still believed possible that thing predicates be "reducible" by what he later called "introductive chains" of reduction sentences\textsuperscript{84} to perception terms. In principle then, the statements of empirical science could still be expressed in the phenomenal language. Carnap's introduction of reduction sentences did not render methodological solipsism any more impossible in principle than before, but only more cumbersome in practice.

Nevertheless, there is one aspect of Carnap's Paris congress innovations which did lessen his reliance on his residual epistemological phenomenalism. With the acceptance of Tarski's theory of truth, and the rehabilitation of the confrontation of statement and fact, Carnap was now able to distinguish protocol sentences from non-protocol sentences by the condition that they contain only "observable" physical predicates.\textsuperscript{85} (Statements containing observable predicates were "directly testable" by the confrontation with facts.) Unlike still

\textsuperscript{80}Neurath 1935c:662.
\textsuperscript{81}Carnap 1936b:65.
\textsuperscript{82}Ibid.:67; see also his more detailed treatment of in his 1936/7:463-8.
\textsuperscript{83}Carnap 1936b:67.
\textsuperscript{84}Carnap 1936/7:446.
\textsuperscript{85}Ibid.:13.
in 1934, Carnap did not have to abandon the principled distinction between protocol sentences and non-protocol sentences within the physicalistic language, and instead rely for the defense of empiricism on the epistemological phenomenalistic reduction.\textsuperscript{86} (That, as I noted, had been the consequence of the adoption of Popper's conception of basic sentences outside of the Popperian and within the Carnapian syntactical framework. This consequence had been noted explicitly by Hempel and perhaps before by Schlick, prompting him to insinuate that the physicalists generally--and not just Neurath--had fallen into idealism.)

8.4 Conclusion to Stage Three: Stand-Offs Between the Participants

To conclude. Schlick's challenge to the physicalists failed to convince them of the supposed error of their ways. It did, however, occasion clarifications of Neurath's radical physicalism and Schlick's own method of meaning determination. Carnap's own subsequent change of position though was rather prompted by the dynamics and tensions internal to his own project. (For how Popper played into them, see appendix to this chapter.)

Carnap's attempted reconciliation was highly influential on subsequent analytical philosophy in the view it presented of the protocol sentence debate, namely, as exemplifying partly the confusion of logical and psychological questions and partly the confusion of truth and confirmation talk.\textsuperscript{87} Carnap was less successful in bringing the other two main contenders to agree with him, or with each other.\textsuperscript{88} And while Carnap's

\textsuperscript{86}See above section 7.7.3.

\textsuperscript{87}See Hempel 1951 for an elaboration of Carnap's portrayal in terms of the distinction between absolute and relative confirmation.

\textsuperscript{88}See, e.g., Joergensen's comment about the still unresolved debate in his welcome address at the Second Congress for Scientific Philosophy in Copenhagen, 1936:283. Schlick still heaped scorn on Neurath's idea of a "Index Verborum Prohibitorum" in the last of his papers published during his lifetime and even extolled the continuity of Vienna Circle philosophy with the philosophical tradition (Schlick 1937:392, 397). This did not escape Neurath (Neurath 1937b:208n.7) who continued the battle for unified science in his anti-philosophical vein. Neurath criticised Carnap in his 1941a:222. Carnap noted the ongoing dispute with Neurath in fn.9 of the expanded English translation 1949:122/3.
views on the matter of the distinction of truth and confirmation became decisive for the developing Received View of scientific theories in the Anglo-Americanisation of Vienna Circle, Schlick's, and particular Neurath's views, fell by the wayside. The protocol sentence debate thus "ended" (discounting its inconsequential fourth act\(^9\)) with the disputant still clearly at odds with each other.

Consider, for example, the stand-off between Schlick and the physicalists (who were also at odds with each other). Did Schlick's clarifications remove all conflict with the physicalists? Clearly not. Schlick obviously retained his view (i) of truth as unique correspondence of statement and fact, (ii) of the possibility of meaningful statements outside of the language of science, (iii) of the basic statements of science being indubitable. As is obvious by now, Schlick's position was totally opposed by Neurath.\(^9\)

Carnap, of course, drew somewhat closer to Schlick's position (i) with the advent of semantics.\(^9\) (Tarski seemed to have redeemed something like the old Schlickean idea of truth as unique correspondence with the concept of the satisfaction of a formula by a sequence.) Carnap also did not doubt the possibility of (ii) or (iii). Yet he too had no sympathy for the Wittgensteinian strictures, now that meaning could finally be openly theorised about. As for the relation between Neurath and Carnap, not much changed between the end of stage two and stage three, but Carnap's position: if anything, their positions were now in even sharper conflict, given Carnap's acceptance of (i).

It may seem strange that, with all of Schlick, Carnap, and Neurath convinced of the need for scientific philosophy, they ended up (more or less) accusing each other of having abandoned empiricism. There were, however, good reasons for their apparently unresolvable conflict. These reasons concerned the conflict of their underlying

\(^9\)See section 8.1 above.
\(^9\)That is one thing the Vienna Circle literature agrees about.
\(^9\)Compare Davidson 1982:480 and Lauener 82:130.
philosophical conceptions, their desiderata for scientific philosophy. I will turn to this matter in the next chapter.
Appendix to Chapter 8: Popper and the Protocol Sentence Debate

I must now fill a lacuna in my review of the protocol sentence debate and provide a discussion of the role of Karl Popper in it. Even though completeness is not my aim, this matter needs to be addressed, for Popper's influence on Carnap is often seen to overshadow Neurath's. In view of the fact that Neurath's radical challenge to Carnap went mostly unmet, this claim has some plausibility. Yet precisely wherein did Popper's contribution consist?

Popper's reception of Vienna Circle theorising in general, and of the physicalists' views in particular, is a peculiar one. Popper was not happy to have had his conception adopted by Carnap, for by his lights the problem of validating knowledge had to be solved in an entirely different fashion: Popper retained the foundationalist ideal. Finding this ideal to be contradicted by the uncertainties incurred by the application of the inductive method, he discarded induction as a route to knowledge and, instead, developed a deductivist model of scientific knowledge which provided for knowledge by the standards of old. What Popper ended up with, however, was an inversion, as it were, of the traditional conception of knowledge, for only negative knowledge was provided for - only once we falsified a hypothesis did we gain knowledge, knowledge that a claim was false.

Despite this disagreement, one contribution of Popper's to the Vienna Circle philosophy stands out already: his proposal for the form of scientific protocols which Carnap accepted in late 1932 as his preferred method (B). Yet Popper is also famous for his claim to have "killed Logical Positivism". Clearly, however, Popper's proposal that any singular physicalistic sentence could serve as a protocol sentence hardly amounted to such a killing: Carnap did not, for example, abandon his belief in the possibility of an

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"original" protocol language when he accepted it. What then, if anything, did the killing, if such there was?

An argument attributed to Popper by Carnap says that the universals of the intersubjective language were not reconstitutable in the autopsychological language; on the basis of the latter, the intersubjective concepts of science remained out of reach.\(^{93}\) Popper dated his objection to Carnap's auto-psychological language to his first meeting with Feigl.\(^{94}\) Apparently Feigl communicated Popper's misgivings to Carnap who then invited Popper discuss these matters in the summer of 1932 when Carnap read the still unpublished first version of what came to be Popper's *Logik der Forschung*.\(^{95}\) From this it is sometimes concluded that Carnap's turn towards physicalism was due to Popper's argument, and Neurath's efforts are discounted as "enthusiastic though logically defective".\(^{96}\) Carnap himself, however, assessed the importance of Popper's arguments as having "helped in clarifying and strengthening the physicalistic conception I had developed together with Neurath".\(^{97}\) To what extent then did Popper have a hand in the protocol sentence debate?

To assess these matters, we must try to determine what may have been Popper's argument against Carnap's auto-psychological language. The obvious candidate here is the argument in Popper's *Logik der Forschung* (Logic of Scientific Discovery) against Carnap's *Aufbau*. Its highly compressed form is the following.

[W]e can utter no scientific statement that does not go far beyond what can be known with certainty 'on the basis of immediate experience'. [...] Every description uses *universal* names (or symbols or ideas); every statement has

\(^{93}\) Carnap 1963a:32.

\(^{94}\) Popper 1963:194 dated it as "either 1930 or 1931" whereas Feigl 1969a:67---reported it as 1929 and Popper 1974:82 as "1929 or 1930" noting his inclination to follow the dating by Feigl (who had left Vienna for the US in the summer of 1930).

\(^{95}\) Popper 1974:85, 89; Carnap 1963a:31. Parts of this draft were since published as Popper 1979.

\(^{96}\) Feigl 1963:229/30n.

\(^{97}\) Carnap 1963a:32; his own 1932e:223 only mentions talks with Popper; the latter's [1935] is first announced in Popper 1933:427n.5.
the character of a theory or hypothesis. The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the universals which appear in it cannot be correlated with any specific sense-experience. (An 'immediate experience' is only once 'immediately given'; it is unique.) By the word 'glass' for example, we denote physical bodies which exhibit a certain law-like behaviour, and the same holds for the word 'water'. Universals cannot be reduced to classes of experiences; they cannot be 'constituted'.

Now, what is the argument here? As it stands it is seriously underspecified. At least three arguments are hidden here.

According to the first candidate argument, it is essential for Popper's argument that the distinction between universal concepts and individual concepts be observed. Universal concepts are concepts whose extension comprises an infinite class of elements; individual concepts are concepts whose extension comprises a limited class of elements. Universal concepts cannot be defined by individual concepts: the definition of the latter uses proper names essentially, whereas the definition of the former does not do so. Popper argued then that science uses universal concepts, whereas the concepts defined in the Aufbau always remained individual concepts. Carnap's Aufbau program failed because "we can utter no scientific statement that does not go far beyond what can be known with certainty 'on the basis of experience'." Carnap falsely tried to constitute the universal concepts of science by individual concepts, namely concepts for particular experiences.

A second candidate for Popper's "killer" argument turns on the fact that Carnap's phenomenal language referred to experiences: objects of the physical domain (which, as Carnap stressed, was the only one to exhibit thoroughgoing lawfulness) cannot be reduced to objects of the auto-psychological domain (or even classes of such phenomenal objects). In this reading, what brought Carnap's reconstruction to a fall was its reliance on a

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98Popper 1935:94/5; compare ibid.:45n..
99Ibid.:95.
phenomenalist basis: Popper made the point that science requires intersubjective accessibility to the evidence.

A yet different third candidate argument is presented in Popper's own rendering of the criticism he first put to Feigl: according to the Aufbau, all statements were either analytically true or contradictory, given Carnap's ultimately "enumerative" definition of concepts there via the pair list of the relata of the basic relation extension.\textsuperscript{100} This criticism flatly discounts Carnap's own distinction between analytic and synthetic theorems, the theorems which define higher-order terms by simpler ones and thus constitute the genealogy of concepts and the theorems by which particular empirical claims are shown to be justified.\textsuperscript{101} Because this criticism did not apply, and Carnap did not recognise it\textsuperscript{102}, the third candidate argument cannot have had an influence on Carnap.

Discounting candidate three for the reason mentioned, what could have been of importance to the protocol sentence debate were the candidate arguments one and two, which Carnap combined in retrospective. To evaluate Popper's claim to have killed logical positivism we must recall Carnap's "liberalisation of empiricism": not all terms of the language of science needed to be eliminably definable in terms of some basic observational, particularly, phenomenalistic vocabulary.\textsuperscript{103} If anywhere, it is here that Popper's claim to have "killed" logical positivism could find what justification it possesses. How then was Popper's "killing" related to Carnap's "liberalisation" of empiricism?

The first candidate argument may well have been the criticism which in the long run roused Carnap to replace the presumed eliminative definability of dispositional concepts by non-eliminative "reduction sentences" (as first reported at the Paris congress in 1935).\textsuperscript{104}

\textsuperscript{100}Popper 1963:194.
\textsuperscript{101}See above section 3.2.1.
\textsuperscript{102}Compare his reaction to Goodman's criticism in his 1961 and 1963b:944-7.
\textsuperscript{103}See above sect. 8.3.3.
\textsuperscript{104}Carnap used the example 'Here is a stone' in his 1936b:62-7; see also his 1936/7.
This reconstruction reads Popper's argument as saying that the law-like behaviour of the objects of science cannot be fully captured in phenomenal terms. What Carnap did, accordingly, was reflect on Popper's objection and find that disposition terms did not eliminatively reduce to physical observational terms and that physical observational terms ("thing predicates") did not eliminatively reduce to phenomenal terms. Carnap came to this conclusion because he stumbled over the problem that reduction by simple conditionals failed in cases where the antecedent was, as a matter of fact, false. Popper's argument thus provided the impetus, but not the reason for Carnap's change of position. If this conjecture is true, then Popper's criticism did indeed contribute to the "liberalisation" (as Carnap put what Popper thinks the "killing") of logical positivism.

That said, it must be noted that it is yet another question whether Popper's first argument, so reconstructed, was solely responsible for this change in Carnap's theory. It is one, furthermore, that is likely to be answered negatively. The first point to note here is that such criticism of Popper's against the simple reduction-by-conditional was not directed against it any more explicitly in the 1930-33 manuscript for what was to become the Logik der Forschung (parts of which were read by Carnap in the summer of 1932) than in the brief passage cited from Logik der Forschung above. Unless the relevant passage was lost or dropped from the lately published edition of this manuscript105, Carnap must have learnt of this argument from Popper in conversation—if indeed he got it from him. The second point to note here is that just this particular criticism of the simple-minded and faulty reduction of disposition concepts was made very explicitly, and so directed against the Vienna Circle physicalists, by L.J. Russell in his contribution to an Aristotelian Society symposion with Susan Stebbing, held in July 1934.106 Three months later, in October 1934 Carnap gave his London lectures later published as Philosophy and Logical Syntax.

It is more than likely, I believe, that L.R Russell's criticism came or was brought to Carnap's attention then at the latest.\(^{107}\)

What role then did Popper's second candidate argument play in the protocol sentence debate? Already in 1930 Carnap conceded part of Neider's and Neurath's criticism of the epistemological approach taken in the *Aufbau* which they put to him during stage one of the debate: Carnap noted that there was a need for a materialistic derivation of concepts so as to underwrite the practical testability and intersubjectivity of scientific statements. In a further revision of his views published in early 1932 Carnap reinterpreted the "original" protocol language as also speaking of physical objects. When at the end of stage two of the protocol sentence debate, therefore, Carnap neglected to note Popper's argument for the intersubjectivity of the protocol language (while at the same time noting and accepting his conception of protocol sentences), he did so because these points had been not at all new to him. As we saw, Neurath and Neider had badgered him on this point since 1929. Neurath not only drew far more radical conclusions from the anti-phenomenalist argument than Popper whose falsificationist deductivism sought to provide the (at least negative) certainties which verificationism could not provide, but Neurath's physicalist arguments were developed against, and affected, Carnap's position independently of, and prior to, Popper's intersubjectivity argument. Neurath was long familiar with the intersubjectivity argument first put to Carnap by Neider and incorporated his own materialist conclusion into his 1929 address, and had his own radical private language argument since 1931. The importance of Neurath's physicalist arguments against Carnap's retention of a phenomenalist protocol language is thus underlined.

\(^{107}\)It may also be added that whereas Popper is fond of claiming that his *Logik der Forschung* came out not, as its imprint indicates, in 1935 but in 1934. Haller furnished a yet more precise date: December 1934 (Haller & Kreuzer 1982:74).
I conclude that none of the candidate arguments hidden in *Logik der Forschung*, or the by now published manuscript for it, amount to a "killer" argument. To be sure, Popper's longer-term contribution to the protocol sentence debate as represented by his first candidate anti-reductionist argument, however vague, must not be discounted: the revision of Carnap's conception of the structure of the language of science apparently in part prompted by it was soon widely accepted, Neurath included. Furthermore, the dissatisfaction with Carnap's response to partly Popper's challenge spurred on the critics of what soon came to be known as the Received View of scientific theories and contributed to its overcoming by critics working both inside and outside of the tradition of Logical Empiricism. But the "real" protocol sentence debate was not thereby affected, nor was the radicality of Neurath's physicalist challenge matched. (That Popper's anti-phenomenalist argument, as reconstructed, was a good one, is quite irrelevant: it only put into formal dress of universal and individual concepts Neider's verificationist point.)

So it must be considered one of the ironies of the history of analytical philosophy that one of Neurath's "pseudo-rationalist" opponents, no less, lays claim to effectively overthrowing Viennese scientific philosophy, while Neurath's more radical pioneering efforts from within the Circle have been mostly forgotten. Therein perhaps the "killer" reputation of Popper's argument finds its justification.

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108 Just in case it is thought that Neurath derived his view that science uses a universal jargon (in which terms of the physicalistically cleansed everyday language figure) from Popper's view that empirical terms cannot be defined (Popper 1979:366/7) it must be noted that Neurath himself alluded to the idea of a universal jargon already in his 1932a:62-8 passim.

109 What Popper did not mention in his memoirs was that Neurath himself answered with a delightfully devastating critique of Popper's *Logik der Forschung* (Neurath 1935b, briefly referred to above). The consideration of Neurath's points would lead too far into (or would require too much of) the reconstruction of the "encyclopedic" Neurath to be attempted here. Suffice it to say that Popper's falsificationism was noted to trade on an underlying presumption of certainty--namely, certainty of falsification--which rendered Popper's "physicalism" as unusable as Schlick's. See below sect. 11.2.1 for refutation of Popper's criticism of Neurath.
Chapter 9

INTERNAL SYSTEMATIC ANALYSIS:
THE PROTOCOL SENTENCE DEBATE AS THE ARENA OF THE
STRUGGLE FOR THE TRUE SCIENTIFIC PHILOSOPHY

Abstract: Summary of my reconstruction of the protocol sentence debate in terms of categories and questions internal to it, thus filling out the provisional sketch of the three perspectives on scientific philosophy provided in the introduction. Schlick, Carnap and Neurath were not naive scientific thinkers (9.1). Comparative analysis of their answers to the questions about protocol sentences which were explicitly discussed in the debate (9.2), of their considered answers to questions less explicitly discussed (9.3), and finally, of their answers to questions which remained entirely implicit (9.4). Specification of the three Viennese conceptions of knowledge and epistemology (9.5).

The present chapter summarises my interpretation of the protocol sentence debate as a whole by presenting a systematic exposition of the positions taken by the disputants on ten related but progressively more obscure epistemological questions. In methodology this chapter diverges from the preceding chapters. Here I do not engage in detailed historical reconstructions of the expression and development of the three perspectives of scientific philosophy in the debate, but present my results in a systematic fashion.1 In doing so, I pick up the large-scale questions I surveyed in my introduction. To repeat, my basic thesis is that the Vienna Circle's protocol sentence debate was not a string of blunders committed by a monolithic movement of philosophical levellers but rather a clash between three quite different answers to the fundamental and non-naive question of how empiricism was to be

1Since the present chapter is a summary of the results of the preceding review I shall now, for the most part, dispense with the scholarly apparatus of quotes and citations. These are assembled in the preceding chapters 3-8 of detailed analyses and in my introduction chapter 2.
defended in the light of the counter-intuitive advances of science. My specific thesis is that Neurath's answer to the foregoing was that epistemology had to be naturalised.

9.1 "Critical" Viennese Scientific Philosophy

Before specifying the differences between the three conceptions of scientific philosophy with reference to the positions taken by Neurath, Carnap and Schlick in the protocol sentence debate in a systematic, I must stress their common "critical" outlook (in the Kantian sense of the term). This point is basic to what follows. I will then catalogue the philosophical questions which they dealt with "critically" and which will be considered in the subsequent sections of this chapter.

9.1.1 "Critical" Scientific Philosophy

In stressing the Circle's "critical" approach to the problem of knowledge, I reaffirm the warning, given in my introduction, that the Vienna Circle did not embrace a naive scientific outlook, as alleged, for example, from different perspectives by the early Popper and by Critical Theorists like Horkheimer and Habermas. It is essential for the understanding of Neurath's philosophy that this point be clearly understood. Given his contrapuntal style of discussing philosophical matters and his at times brusque, if not unsophisticated sounding, turns of phrase, Neurath's revolutionary proposals have all too

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2 It is most likely the failure to appreciate the Vienna Circle's "critical" dimension which leads Passmore to pass the still widely accepted judgement that Logical Empiricism is "as dead as a philosophical movement ever becomes" (Passmore 1967:56). The new life given particularly to Carnap by the new canon of course crucially turns on the recognition of that "critical" dimension. So does, as I show in this dissertation, the new life of Neurath: seeing Neurath respond to Carnap's neo-Kantianism breaks the hermeneutic lock on his own naturalism.

3 For the tone of the Critical Rationalist criticism of Vienna Circle scientism, note Popper's allegation of "psychologism" (Popper 1935:94f) and his recollection of his early opinion that the Vienna Circle proceeded more or less in ignorance of Kant (Popper 1974:82/3). The tone for the Frankfurt reception was set by Horkheimer 1937 and summed up by Habermas: "That we deny reflection is positivism." (1968:preface.) For a fuller discussion of the literature see above sect. 2.3 and the Appendix to the Bibliography.
often been misunderstood. Seeing the sophisticated context within which he worked is a necessary first step towards their better appreciation.

No mere "levellers" were at work in the protocol sentence debate: the debate was over what empiricists should do given the untenability of naive empiricism. What I call "naive empiricism" has it that immediate sense experience is by itself sufficient to provide the foundations for knowledge. The Vienna Circle's abandonment of Kantian metaphysics as a science of the synthetic a priori did not amount to a relapse into naive empiricism for two reasons, reasons yet over and above their Humean anti-metaphysical stance. The first reason such reason was their familiarity with the quandaries of the then contemporary theory of science. Positivism had rejected Kant's metaphysical grounding of science as unnecessary. With the conventionalist critique of Mach, however, metaphysically ungrounded habitual inference came to be seen as incapable of providing experiential support for the general principles and high-level hypotheses of science. Philip Frank thus described the theme of the Vienna Circle as follows:

Our whole group understood and fully agreed that the human mind is partly responsible for the content of scientific propositions and theories. [...] We admitted that the gap between the descriptions of facts and the general principles of science was not fully bridged by Mach, but we could not agree with Kant, who built this bridge by forms or patterns of experience that could not change with the advance of science.4

In short, the Vienna Circle was concerned with the important role of conventions in science. The second reason for the Circle's sophistication was its familiarity with the problem of objectivity. As Schlick had in effect noted in his early Allgemeine Erkenntnislehre, the objectivity of experience was again called into question once Kant's transcendental foundations were rejected. Experience was subjective and private whereas knowledge claims aspired to objectivity: how then could the former support the latter?

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4 Frank 1949:7/8. Frank talked specifically about the "first" Vienna Circle but his narrative implied the thematic continuity between it and the Vienna Circle proper.
Thus, for all of the members of the Vienna Circle, the private indubitable materials of experience were not, by themselves, sufficient for the justification of knowledge claims.

Combining the two post-Kantian problems of accounting for the role of conventions in science and reconciling the subjectivity of experience with the objectivity of knowledge claims, the aim of Vienna Circle philosophy may be described as follows: to accommodate the creativity of scientific theorising without accommodating idealism. It is important to note that all three Viennese perspectives on scientific philosophy grappled with this accommodation! Carnap, Schlick, and Neurath sought to capture the creativity of scientific theorising in the logical frameworks of the language(s) of science. What they differed on was the nature of that logico-linguistic reconstruction and the precise type of strategy to so comprehend the creativity of science--not that this comprehension constituted the epistemological goal.

The central question of the protocol sentence debate was "What justifies scientific knowledge claims?" The basic answer was: the statements expressing them stand in appropriate logical relations to statements formulating proven evidential conditions. In the protocol sentence debate the question of the nature of the logical relation was discussed relatively little. It was assumed that the statement expressing the knowledge claim had to entail a tested and confirmed evidence statement. The main topic of the debate was rather the nature of these evidence statements. The common Viennese concern with the forms of representation in general found application in the task of establishing the ground of objectivity of these evidence statements.

The reductionism for which the Vienna Circle is famous obscures the fact that the Circle's concern with the forms of linguistic representation was a sophisticated attempt to
come to terms with the problematic post-Kantian legacy. To understand the protocol sentence debate, then, one must see that it was a debate concerning the approach, as well as the solution, to a problem conceived by all in its post-Kantian complexity. That there is this "critical" Kantian dimension to Vienna Circle philosophy is, of course, the lesson taught by the new canon of Vienna Circle scholarship. My investigation bears out its plaint as follows: failure to note the Kantian dimension might easily result in the failure to appreciate the complexity and importance of the various positions and the constructive aspects of the debate.

It is equally important, however, to appreciate the differences in the philosophical perspectives taken by Schlick, Carnap, and Neurath. Noting only their shared "critical" sophistication would not explain their sharp disagreements in the protocol sentence debate. It is the task of this chapter to provide this differentiation in reasonable detail. I shall argue below that a different diagnosis of the perceived problem of knowledge underlies each of the three perspectives on scientific philosophy, adopted by Schlick, Carnap and Neurath. The success or failure of any of these different conceptions of scientific

5Even when a phenomenalist reductionist strategy was pursued, as it was by Schlick and Carnap, it was assumed that the conception of form and structure unearthed by the tool of modern logic was broad enough to comprehend the problematic creativity of scientific theorising by the rational reconstruction of the conventional elements in science. Consider Carnap. In the Aufbau, Carnap assumed that the constructional language explicates the apriori of cognition in its formal structure; his Logical Syntax picture was that the structure of arbitrary formal systems made for possible a priori frameworks of knowledge. Or consider Schlick. Schlick assumed that the structure tool of modern logic delimited a precise class of conceptual operations ("affirmations") which were self-justifying. (Stage three Schlick, like stage two Carnap, had to recognise the relative isolation of these basic statements from those of science.)

6See above sections 2.3 and 2.5.

7Passmore 1967:55/6 is after all the only traditional historian who clearly discerned three basic conceptions—though not their depth.

8Thus note that—alone of the new canon—Romanos 1983:28 notes Carnap's retention of phenomenalism for epistemological purposes (see below sect. 9.3) but still fails to clearly distinguish three basic Viennese conceptions and to note Neurath's naturalism.

philosophy is dependent partly on how radical the sceptical challenge to the knowledge claim of science was understood to be. It is not at all surprising, therefore, that the three principals of the debate did not come to agreement. In the defense of empiricism, that is, in the response to the radical sceptical challenge as they perceived it, epistemologies defined themselves and knowledge in the themes they set themselves to explore.

9.1.2 The Issues of the "Critical" Protocol Sentence Debate

To reconstruct their own conceptions of scientific philosophy in this chapter, I present a systematic analysis of Schlick's, Carnap's and Neurath's different positions in the protocol sentence debate. These positions are fixed by their answers to the three groups of questions and problems which figured in the debate, though not always explicitly so. The epistemological taxonomy developed here is thus an "internal" one. (For an "external" analysis see the next chapter.)

First, there are the three concrete questions concerning (i) the content or meaning scientific evidence statements, (ii) their logico-linguistic form and (iii) their epistemological status. (Note that throughout I shall use the terms 'evidential statements' and 'protocol sentences' or 'protocol statements' interchangeably.) The participants' answers on these questions is clearly documented in my review above and summarised below. This is the level of the debate which serves as the data base for the required reconstruction. The answers to these three concrete questions coalesce in their answer to the leading problem which I call (iv) the "basis problem", namely what forms the basis of scientific knowledge claims.

Second, there are the underlying problems. These are (v) the structuralisation problem and (vi) the problem of the adequacy condition of epistemology. The structuralisation problem concerns the question whether the defense of scientific knowledge
claims can be undertaken by an investigation of purely formal or structural aspects of scientific theories, evidence statements and their relation. The adequacy problem concerns the question when an epistemological analysis is to be considered successful. I call them "underlying problems" because, while the basis problem (iv) was prominently discussed in the protocol sentence debate, problem (v) already was less openly discussed and problem (vi) least of all. With (vi), in fact, we touch upon the distinctive features of the disputants' perspectives on scientific philosophy; we must work our way up to it. With respect to problems (v) and (vi) we must also consider three further underlying problems, namely (vii) the question of the representability of logical form, (viii) the question of whether the rebuttal of the sceptic requires that knowledge be shown to be actual or only possible and (ix) the question of what the disputants' new conceptions of meaning were.

I shall start my analysis by summarising the protocol sentence debate along the lines of questions (i) through (iii) and problem (iv). This procedure recommends itself because this way we start from facts, namely, the participants' documented positions in the debate. Here we gain the entry into the murkier domains of the protocol sentence debate, what I called the underlying problems (attacked through the medium of discourse about protocol sentences). Different positions on (i) through (iii) expressed answers to (iv) and followed from different positions on (v) through (ix). A survey of the candidates' answers to (i) through (iv) shows their commitment to particular answers to (v) through (ix); their answers to (i) through (iv) thus indicate obliquely what their positions were on the topics less prominently addressed in their public exchanges. Of these I will leave consideration of (vi) and (ix) to last and then proceed to the last group of questions.

This third and last group of ideas discussed in this chapter is that of (x) the three conceptions of scientific philosophy. This is the least explicitly argued about level of the debate; here a great amount of reconstruction is required of interpreters. The different
conceptions of scientific philosophy reflect the different perceptions of the sceptical challenge to be met. This means that they reflect the disputants' conceptions of what can be assumed in the rebuttal of the sceptical challenge to the knowledge claim of science.

9.2 The Data: The Positions on the Content, Form and Status of the Evidential Statements of Science

Before I start, a comment about my representation of the disputants' positions. In the preceding six chapters I reviewed the protocol sentence debate from 1928 to 1935, from its early background in Carnap's *Aufbau* and its later background in his *Logical Syntax*, across its first three stages, and into its final fourth stage. To represent my results accurately, I split Carnap's position into four stages, corresponding to his positions during the the four stages of the debate. I call them "Carnap-1", "Carnap-2" etc.

"Carnap-1" is the *Aufbau* position, "Carnap-2" that of 1930-1932 (up to Carnap's "Ueber Protokollsaezete"), "Carnap-3" that of 1932-1935 (that of "Ueber Protokollsaezete") and "Carnap-4" is his post-Paris congress semantic position. The Carnap-1 position is somewhat generalised because the nature of evidence statements was not explicitly commented on. Furthermore, due to the ambiguity in Carnap-3, that position is subdivided into two distinct positions. This ambiguity will be resolved at the end of the present section. No such distinctions are required in the cases of Schlick and Neurath. Their positions remained sufficiently constant throughout the debate to be represented by one gloss.

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10 For an overview of the themes and stages of the debate see above, section 2.4; for a defense of my dating of the stages see above Appendix to Chapter 2.

11 That during stage one Schlick perhaps hoped to achieve with his position what at the end of stage three he had to admit could not be achieved, namely, full foundations for scientific knowledge, did not result in a major change of position on any of his questions in play. See below, sects.9.4.3 and 9.6.1.
9.2.1 The Content of Evidential Statements

The candidates' answers to the question of the content of protocol sentences were as follows. By "content" I mean referential meaning. (Note thus that what Schlick called "content", the intuitive private material of consciousness, does not figure at all in the present terminology.)

**Schlick:** the evidential statements concerned the immediate given of conscious experience; that there was a physicalistic interpretation for them was irrelevant to their sense.

**Carnap-1:** the evidential statements (of the epistemological reduction) formally explicated similarity relations between phenomenal givens; it was assumed that the two schemes of physicalistic and phenomenal contents were correlated.

**Carnap-2:** the evidential statements were basic statements about the phenomenal given; these statements possessed a physicalistic interpretation in terms of stimulation states of the organism.

**Carnap-3a:** the evidential statements were basic statements about the phenomenal given; these statements possessed a physicalistic interpretation in terms of stimulation states of the organism.

**Carnap-3b:** the evidential statements were about physical states of affairs.

**Carnap-4:** the evidential statements were about observable physical states of the environment

**Neurath:** the evidential statements were about the stimulation state of an organism and its relation to the environment.
Note that while the coarse distinction here is between phenomenal and physical contents, there were in fact four different positions. The content of protocol sentences was construed as either phenomenal (Schlick, Carnap-1), narrowly physical (Carnap-2-3a), widely physical (Carnap-3b-4) and relationally physical (Neurath).

9.2.2 The Form of Evidential Statements

The candidates's answers on the question of the form of protocol sentences, that is, the nature of the language in which they were formulated, were as follows.

**Schlick:** the evidential sentences lay outside of the scientific system language; different rules of formation and use attached to them.

**Carnap-1:** the evidential sentences were expressed in the constructional language of the Aufbau in a separate, epistemological system language, not in the scientific language.

**Carnap-2:** the evidential sentences were basic sentences in private Aufbau-type languages outside the physicalistic system language but inter-translatable with it.

**Carnap-3a:** the evidential sentences were basic sentences in private Aufbau-type languages outside the physicalistic system language but inter-translatable with it.

**Carnap-3b:** the evidential sentences were any physical system statements that were accepted into positions of serving the testing of other physical statements.

**Carnap-4:** the evidential sentences were physical system statements employing only observational predicates.

**Neurath:** the evidential statements were physical system statements with a particular surface-syntactic form.
Note that there were two questions at issue here. First, whether the language in which the evidential statements for scientific theories were formulated was, or could be, the same as that in which the scientific theories themselves were formulated. The two answers were that protocol sentences do belong to the physical system language (Neurath, Carnap-3b-4) and that they do not belong to it (Schlick, Carnap-1-3a). Second, there was at issue the question how the logical form of the language in question was to be conceived of. On this second question again two positions were taken, namely, that the logical form at issue was to be explicated in syntactical terms (Carnap-1-3b, Neurath, Schlick) or in semantical terms (Carnap-4).

I should warn the reader that the agreement on this second question during stages one through three of the debate must not be taken to signal agreement about the nature of "syntactic" theorising. Whether or not such syntactic theorising could itself count as properly meaningful discourse is a separate issue which is dealt with by the structuralisation question. It is similarly a separate issue how the individuation of syntactic categories was thought of; that is dealt with partly by the meaning question and partly by the adequacy problem.

9.2.3 The Epistemological Status of Evidential Statements

The question of the epistemological status of protocol sentences concerned the question whether the evidential statements had to be unrevisable, and thus certain, or whether they themselves could be revised, and thus uncertain. The candidates' answers were as follows.

Schlick: the evidential statements were certain and unrevisable.
Carnap-1: the evidential statements were certain and un revisable under the condition that the reconstructional language in which they were expressed correctly represented the connections between the concepts involved.

Carnap-2: the evidential statements were certain and un revisable.

Carnap-3a: the evidential statements were certain and un revisable.

Carnap-3b: the evidential statements were uncertain and revisable.

Carnap-4: the evidential statements possessed an overwhelming presumption in their favour but were, in principle, uncertain and revisable.

Neurath: the Neurath-principle: all statements, and thus also the evidential statements, were revisable and uncertain.

Note that there were two basic positions of claiming (Schlick, Carnap-1-2-3a) and denying (Neurath, Carnap-3b-4) certainty to protocol statements. Within each basic position, however, somewhat less clear-cut stances must be recognised. There was that of conditional unrevisability (Carnap-1) as opposed to unconditional unrevisability (Schlick, Carnap-2-3a) and that of more (Neurath, Carnap-3b) or less (Carnap-4) probable revisability.

9.3 The Leading Problem of the Basis Question

I began this chapter by reiterating my warning that the Vienna Circlists were not naive scientistic thinkers, unaware of the problematic Kantian legacy, but that their efforts must be viewed as attempts to come to terms with this legacy. We can see their historical awareness reflected in their answer to the basis question, the question of what is the nature of the statements upon which scientific theories rest.
9.3.1 Viennese Answers to the Basis Question: Received Interpretation and Historical Fact

The Received View of matters Viennese has it that the issues addressed in the concrete questions of the protocol sentence debate were these: whether the contents of protocol sentences were phenomenal or physical states of affairs and whether they were revisable or not. The Received View of the debate contrasts the position that protocol sentences were unrevisable and concern phenomenal states of affairs with the position that they were revisable and concerned with physical states of affairs. According to the Received View, Schlick and Carnap-1 would be foundationalists and Neurath and Carnap-2-3-4 would be anti-foundationalists. (Needless to say, perhaps, this view does not note the ambiguity of Carnap's stage three position and takes his position to be represented by Carnap-3b.) The Received View can be, but need not be, of a kind with the view that the Vienna Circle embraced naive scientism. The Received View of the protocol sentence debate is endorsed, for example, by self-consciously "scientistic" thinker like Quine:

[B]y 1932, in Carnap's 'Psychologie in physikalischer Sprache', physicalism had prevailed. The phenomenalism that Carnap imbibed from Russell had had its day."\(^{12}\)

My detailed investigation, by contrast, shows that the various positions adopted lead to important and subtle further differences about the nature of the evidential statements for scientific knowledge claims, and thus about foundationalism, about the vulnerability of empiricism to scepticism, and about epistemology in general.

Consider the solutions offered to the basis problem. The candidates' solutions to the basis problem fell out of their answer to questions (i) through (iii), of the content, form,

\(^{12}\)Quine 1984:327.
and the status of protocol sentences. Their answers to the question: What forms the basis for the defense of scientific knowledge claims? were these.

**Schlick**: incorrigible statements about the immediate given (phenomenalistically understood).

**Carnap-1**: incorrigible statements about the immediate given (phenomenalistically understood).

**Carnap-2**: incorrigible statements about the immediate given (physicalistically understood).

**Carnap-3a**: incorrigible statements about the immediate given (physicalistically understood).

**Carnap-3b**: corrigeble statements about physical states.

**Carnap-4**: corrigeable statements about observable physical states.

**Neurath**: corrigeable statements about the relation between states of the environment and stimulation states of an organism.

First of all, recall the disputants' sophisticated "critical" approach to the question of justification. Then we find that Schlick cannot count as a straight-forward foundationalist. (What made even the foundationalistically oriented Circlists non-naive foundationalists was, again, that it was not the acquaintance with the intuitive materials of consciousness that made for the meaning of the basic statements but, like that of any other statements, their logical form.) Second, we must take into account the different physicalistic interpretations of protocol sentences, the different conceptions of the certainty they afforded, and consider whether they were formulated in the system language of science or outside of it. Then we find that Carnap-2 cannot count as an anti-foundationalist, and,
furthermore, that there is reason to believe that Carnap-3 cannot count as an anti-foundationalist either.

9.3.2 Carnap's Foundationalism

What the received view overlooks is that the opposition phenomenalism-incorrigibility vs. physicalism-revisability captures the opposition between Schlick and Neurath, but fails to do justice to Carnap's subtle changes. What precisely does it mean to say that "physicalism had prevailed"? My reconstruction shows that Carnap-2 endorsed the combination of physicalism-incorrigibility with his conception of a narrowly physicalistical interpretation of statements about the given. Carnap-3 further supposed the compatibility of this incorrigible physicalist protocol language for purposes of epistemology with the adoption of a corrigeble physicalist protocol language for science.

That Carnap-2 remained foundationalist despite his official endorsement of of the physicalist protocol statements, is a result I established in chapter 7. Carnap always distinguished the tasks of the scientist from that of the epistemologist, and the task of the language of science from the language of epistemology. (This is indicated by Carnap's remark that epistemology was the only type of inquiry which, could not do with third-person statements alone, but recognised and reverted to first-person statements.) What distinguished the physicalist Carnap-2 from the Aufbau's Carnap-1 was the recognition that the intersubjectivity requirement for scientific evidence brought physicalism in its train. Thus Carnap-2 combined physicalism with incorrigibility! By 1932, in fact since 1930, Carnap-2 had given up the ontological phenomenalism which the Aufbau's acceptance of the "working hypothesis" of psycho-physical parallelism had required--

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13See sects. 7.3 and 7.7.  
14Carnap 1932c.  
15The trick was to hold original protocols to be outside the language of science and to require translation of them before they can be integrated into science. See above sect. 7.3.3.  
16See above, sect. 5.6.
however "methodological" his solipsism was meant to be--but Carnap not only retained at least the option of, but even still required recourse to epistemological phenomenalism. No longer did Carnap require non-physical "givens", but he still required phenomenalistically immediate "givens". No longer then did Carnap hold to "the phenomenalism he had imbibed from Russell", but neither did he accept Neurath's radical physicalism which required the renouncement of epistemological phenomenalism as well.

Given these facts about Carnap-2, Carnap could have been an anti-foundationalist at best since stage 3. What does require some further discussion is my claim that there is reason to harbour doubts of Carnap's anti-foundationism even during stage 3. I noted the difficulty by flagging the ambiguity of Carnap's position at stage three. Here the question is: Were the protocol statements of the sort recommended by Popper and accepted by Carnap indeed such that it was with reference to them that the knowledge claim of science could be defended against the sceptic? This question amounts to asking what Carnap's stage 3 physicalism amounted to in terms of epistemological concern.

How reasonable would it be to hold that Carnap failed to note that with his acceptance of Popper's basic sentences as the preferred form of physicalistic protocol statements (which marked the end of stage two), the notion of protocol statements as a distinguished class of statements had become superfluous. This was later admitted by Hempel. Against this, Schlick and Popper argued that physicalism failed to safeguard empiricism. The question is how Carnap could defend empiricism, given his embrace of the thesis of theoretical holism, that scientific tests apply, "at bottom, not to a single hypothesis but to the whole system of physics as a system of hypotheses (Duhem, Poincare)."¹⁷ Carnap required a stronger defense of the principle of empiricism, and of the demarcation of science from non-science, than that afforded by his claim that

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¹⁷Carnap 1934/7:318.
[t]here is no other distinguishing criterion for "our" science than the historical one that it is the science of our Cultural circle; more precisely: the science which [...] is controlled by the protocol sentences of the scientists of our cultural circle. 18

No safeguard was afforded by Popper's basic sentences (Carnap–3b), as Schlick correctly noted. I argued that Carnap noted this too. I pointed out that Carnap never repudiated the possibility of a phenomenalist reduction, and thus never barred the path of his preferred epistemological reconstruction of knowledge claims. It is without inconsistency then, that Carnap could have held on to the physicalistically interpretable yet ostensibly phenomenalistic protocols he had endorsed throughout stage two for the distinct purposes of epistemology.

I argued in my review above that during stage three Carnap officially recognised the convenience of straightforwardly physicalist protocols and accepted their use in science (see Carnap–3b), but still required a phenomenalistic reduction for the purposes of epistemology. (see Carnap–3a) 19 Carnap–3 held the view that science better do without incorrigibility. Carnap–3 accepted this conclusion for the language of science, but never for the language of epistemology. Thus Carnap–3 could hold two positions at the same time which seemed incompatible. Carnap–3 held both the physicalist-revisability and the physicalist-incorrigibility view of the basis of scientific knowledge. Carnap held the former in his capacity of constructor of scientific languages, and the latter in his capacity of epistemologist. (Carnap the epistemologist never accepted Neurath's contention that the epistemological conception of methodological solipsism made unjustified assumptions which barred it from doing service in the theory of knowledge.)

As I see it, it was only the later availability of the notion of the "observability" of predicates which allowed Carnap to shed the need for phenomenalistic reductions for

18Carnap 1932d:180; for an earlier formulation see Neurath 1932a:61, 67, for a later one Hempel 1935b.
19See section 7.7.3 above and section 11.3 below.
epistemological purposes. (Carnap never renounced the possibility of employing such reductions.) Carnap could only invoke the notion of observability once he embraced, at the start of stage four of the debate, the semantic interpretation of the correspondence theory of truth offered by Tarski. It was Carnap's correspondence interpretation of observational predicates, behaviouristically explicaded, that enabled his new semantic picture theory, and thus—better than ever—enabled the provision of formal-structural explicaciones of knowledge claims. Observationality gave physicalist correspondence foundations to science. (This last sentence must suffice to note the further question whether even Carnap-4 was really an anti-foundationalist.)

I am thus inclined to think of Carnap-3a as the "real" Carnap-3, that is, Carnap the epistemologist. In that case, Carnap-2 and Carnap-3 can be grouped together. Contrary to common presumption, they share their reliance on phenomenalistic reductions for epistemological purposes, and do so unlike Carnap 4. (Of course, Carnap-2-4 all recognised the insufficiency of an exclusively phenomenalistic demonstration of the reducibility of the concepts of science, unlike Carnap-1.)

The result, then, is that the common judgement about who was and who was not a foundationalist must be revised at least twice: not only Carnap-2, but also Carnap-3 was a (non-naive) foundationalists. This leaves only Neurath and, at best, Carnap-4 as anti-foundationists.²⁰

9.3.3 The Relevance of Incorrigibility

Note that the divisions between answers to the basis problem were those between corrigible and various kinds of incorrigible statements on the one hand and between

²⁰Note that Carnap-4 of course retained the option to defend empiricism as Carnap-3 still needed to do, namely, by epistemological phenomenalism, but I shall not probe further. (Compare Carnap 1961:v, vii.)
phenomenalist and various kinds of physicalist contents on the other. Presumably, what lay behind the adoption of phenomenalistic contents was the desire for an incorrigible base. In this connection, one result in particular stands out.\textsuperscript{21} Even the assumption of an incorrigible base did not provide enough to establish the certainty of the scientific knowledge to be defended: such incorrigible statements stood outside of the system of scientific statements! (Thus (ii) also played into the basis problem.) It becomes clear then that Schlick and Carnap-2-3 were foundationalists in a non-traditional sense not only because they were not naive empiricists: they also came to abandon the thought that their foundations transferred their own certainty to the theories they supposedly founded.

This was very important and leads to further questions. Carnap came to reject the phenomenalist base: thus the change from Carnap-1–2 to Carnap-4 over the ambiguous stage 3. Schlick, on the other hand, held on to the assumption of an incorrigible base. Why? Even when Schlick and Carnap agreed about the answer to the basis problem they disagreed on what it was about these evidential statements that provided for the validation of knowledge claims. They did so because they disagreed on what means were available to the epistemologist for the defense of science. This reason becomes plain in their answers to the structuralisation problem to which I turn in the next section.

Already, however, the disputants’ answers to the questions of the content, form and status of protocol sentences throw some light on their different conceptions of epistemology. It is obvious that each participant’s choice of answer to, say, the question of form or content, would not have worked for another. Schlick’s phenomenal content, or the "blue-now-here" form of protocols, for example, would not have done the job Neurath needed to have done, nor would Neurath’s relational physicalistic content, or his long-

\textsuperscript{21}I shall disregard here the complication that Carnap-1 only provided for conditional certainty. (In the \textit{Aufbau}, the epistemologically basic statements were formulated, unlike during his stages 2 and 3, in a universal constructional language. The precise applicability of whose reconstruction of scientific terms was left open. Carnap could provide only conditional certainty for epistemologically basic statements.)
winded form of protocol sentences, have done the job Schlick needed to have done. Their respective protocol sentences carried different statuses of revisability. Might it not be that their epistemologies presupposed different conceptions of knowledge? To gain the perspective required to answer these questions I now turn from the three principal concrete questions and the leading problem of the debate to what I called the "underlying problems".

9.4 The Underlying Problems of the Debate

The underlying problems of the debate were (v) the structuralisation problem and (vi) the problem of epistemological adequacy. The answers given to these problems were not independent of each other, but neither did they carry equal weight. An overview will confirm that the answers to the adequacy problem (and, ultimately their own conceptions of epistemology) were more influential on than influenced by their different solutions to the structuralisation problem and, of course, the basis problem. We shall work our way up to the adequacy problem. In doing so we shall see, as we did not yet, the influence of the disputants' answers to the questions of (vii) the representability of logical form, of (viii) the scope of the epistemological defense mounted, namely, whether it concerned the demonstration of the actuality or only the possibility of knowledge and of (ix) their underlying conceptions of meaning.

9.4.1 The Structuralisation Problem and the Question of the Representability of Logical Form

The structuralisation problem is whether epistemological questions could be dealt with in terms of the form or structure of the theories and evidential statements at issue. Was it even possible to attend only to the form or structure of scientific theories, their evidential statements and the relation between them? (The question whether this strategy was sufficient for dispensing the epistemological task is raised by the adequacy problem.)
Prominently at issue in the structuralisation problem was the idea that form is meaning. (As I argued in chapter 6, Carnap's syntactic theorising was pursued with meaning-theoretic concerns in mind.) Consider whether answering question (ii), the question of the form of protocol sentences, also answered (i) the question of their content. Schlick, Carnap and Neurath all agreed on this because, for them, (i) followed from (ii): logical form determined meaning. Note then that in some general sense all three theorists agreed that epistemologists had to take account of the formal or structural aspects of knowledge. The structuralisation problem proper arises against the background of this general agreement.

Schlick, Carnap and Neurath differed, first of all, on just what kind of epistemology the form-is-content thesis legitimated. (They also differed, secondly, on how the notion of form was to be understood; more on this below.) The different solutions offered to the structuralisation problem turn on the question what the form-is-content thesis enabled epistemologists to do. The candidates' answers to the structuralisation problem were these.

**Schlick**: the problem of the justification of scientific knowledge could not be solved by structural means: one needed to appeal to private experience.

**Carnap**: the problem of the justification of scientific knowledge could be treated by structural-formal considerations.

**Neurath**: the problem of the justification of scientific knowledge could be treated by structural-formal considerations.

Note, first, how (v), the structuralisation problem, and (vii), the question of the representability of logical form, interact. Question (v) is a generalisation of question (vii): it concerns the application of the formal method of inquiry into the conditions of
representationality in general to epistemology. Schlick's negative response to the idea that
the problem of justification could be handled purely formally found its reason in his
adherence to Wittgenstein's ideas on logical form.\textsuperscript{22} It followed from Wittgenstein's and
Schlick's negative answer to the question of the representability of logical form that Schlick
denied that Carnap could talk about logical form in meaningful sentences. Since the
validation of knowledge could not rest on pseudo-statements an altogether different
strategy of validation had to be pursued. Epistemology could not retreat into formal
considerations: the validation of knowledge claims was bestowed only in immediate
experience. (To see how this cohered with the common Viennese non-naive empiricism,
we must consider Schlick's theory of meaning and his answer to the problem of
epistemological adequacy.)

Note, second, how the solution to the structuralisation problem influenced the
solution to the basis problem. Both Schlick and Carnap at one time held to an incorrigible
evidential base for science; whereas Carnap gave it up, Schlick retained it. Schlick's
answer to the structuralisation problem shows his reason: the epistemologist did not have at
his disposal the means to proceed purely formally. It was ultimately his negative answer to
problem (vii), the problem of the representability of logical form, that precluded his
acceptance of other than an incorrigible and phenomenal base.

Note, third, that Carnap's and Neurath's answers to (v) the structuralisation
problem and (vii) the question of the representability of logical form were identical. Yet
both reached different conclusions on (i) through (iv), the concrete questions and the basis
problem. The reason for this lies in their different response to (vi) the adequacy problem of
epistemology and what it presupposed. What it presupposed was (viii) a view about the
scope of the anti-sceptical defense and, particularly, (ix) a conception of meaning such that

\textsuperscript{22}See above sect. 4.4.
the objectivity of knowledge could be defended by reference to it. Consideration of their underlying theories of meaning shows that important differences lurk behind Carnap's and Neurath's agreement on their answer to the structuralisation problem and their understanding of the operative concept of structure.

9.4.2 Three Conceptions of Representation and Meaning

To the disputants' theories of meaning then. At issue in the adequacy problem—whether a formal approach was sufficient for epistemology—was what needs to be explicated or explained so that knowledge claims could be justified. Note that, with the linguistic turn taken, knowledge was no longer thought of primarily in the psychologistic fashion of old, that is, in terms of particularly distinguished mental states. Rather, knowledge was thought to be something constituted by representations, typically sentences or statements. The task of epistemology thus became that of determining what it was about representations that made for knowledge and when the conditions for such were met in principle or in fact.

Carnap's concept of what it was about scientific representation that made for knowledge was: the structure of representation systems. Schlick's was that the structural correspondence expressed by scientific representations made for knowledge. From this, their guiding conceptions of what made for meaning emerges.23 For Carnap, logical form constituted meaning, for Schlick, logical form represented meaning. Consequently, Carnap pursued epistemology as a constitution theory of meaning, whereas Schlick pursued it as a theory of the mediation process by which structural universality was integrated in and gave order to particular experience. Where Carnap saw his task in laying bare the logical structures of representational systems, there Schlick saw his task in

23See, again, above sect. 4.4.
determining the nature of the process by which one such representational system was applied.

Neurath's idea is more obscure. What is clear, however, is that Neurath thought of meaning as correlated information.24 We cannot go astray too far, then, if we conclude that what for Neurath made for meaning was that aspect of representational systems which allowed for intersubjective agreement. Something about the form of the representational system used indicated what the correlated information was. For Neurath, form figured as the medium of communication. The form or structure at issue must be an intersubjectively available one. This meant that the structure at issue must be that of an intersubjective language. Form made for meaning which, in turn, was neither constitutional nor representational in Carnap's or Schlick's sense: meaning was neither imminent in form (as with Carnap's constitution theory) nor transcendent (as with Schlick's mediation theory). Meaning arose from form in context; it was, ultimately, a function of behaviour. What accordingly allowed for the objectivity of knowledge was whatever it was that made for intersubjectively intelligible representation. Thus Neurath was led to think that the kind of structure that made for meaning was a structure that could be discerned intersubjectively.

Note then that the question of the representability of logical form was not the only problem at issue in Schlick's, Carnap's and Neurath's theories of meaning. To be sure, Schlick's dissent from Carnap, namely, that logical form was unrepresentable, was motivated by his Wittgensteinian stance on logical form. But Neurath shared Carnap's belief that logical form was representable and still differed from Carnap on the general conception of meaning and form from which objectivity was to be derived. Here the disputants' different conceptions of (vi) the adequacy and aim of epistemology—and thus of

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24Neurath 1931b:55. See above section 6.4.2; see also below section 11.2.2 and 11.2.4.
(x) how to answer the sceptic—made their influence felt. The Vienna Circle's new theory of meaning were, after all, devised with an epistemological end in view.

9.3.3 The Scope of the Defense of Knowledge Claims

What played into Schlick's, Carnap's and Neurath's theories of meaning (and into their conditions of epistemological adequacy, as shown below) was also the question of what, in effect, to make of Carnap's alternative neo-Kantianism. This question arose as the question of what would be the proper scope for the defense of knowledge claims against the sceptic.

In the case of concepts and principles explicated as synthetic a priori, the question of what makes objectivity possible was, for Kant, also the answer to the question of what makes objectivity actual. His synthetic a priori did not only provide for a possible conceptual framework with reference to which knowledge claims could be justified, but also for the one cognitive framework thought to be actually shared by all humans. We were constrained to use concepts like causation in a certain way. Stripped of its synthetic nature, however, Carnap's formal a priori of cognition lacked this normative authority. As Carnap came to realise between his Aufbau and his Logical Syntax, many different analytical frameworks were conceivable. Showing the formal conditions of the justification of knowledge claims to be fulfilled provided only for the relative justification of these claims, namely, relative to the adoption of the conceptual framework employed in the justification. The question thus arose whether epistemology could rest content with merely showing the possibility of objective knowledge. In other words, was it sufficient for the rebuttal of the sceptic to demonstrate the mere possibility of objective knowledge?

It was on this point that both Schlick and Neurath dissented from Carnap's position. Carnap held that epistemology could do no more than establish the possibility of
objective knowledge; Schlick and Neurath held that the defense against scepticism had to establish the actuality of knowledge. Thus the categorisation of the disputants' positions according to their concern with the actuality (Schlick, Neurath) as opposed to the mere possibility of knowledge (Carnap) coincides with that of dissent from Carnap's theory of meaning (and his condition of epistemological adequacy). It was this difference in orientation which required, for example, Neurath to approach the form-is-content view differently from Carnap, even though he agreed with him on the answer to the structuralisation problem. It was not sufficient to exhibit merely conceivable representational systems as a priori presuppositions of possible knowledge claims; instead the structure of cognition affording objectivity had to be shown actualised, that is, instantiated. Thus Neurath sought the validation of knowledge claims in the investigation of the processes of arriving at intersubjective consensus under certain conditions, namely those obtaining in science.

9.3.4 The Problem of Epistemological Adequacy

We can see these views on the questions of (ix) meaning and (viii) the scope of the defense of science in the candidates' answer to (vi) the problem of the adequacy of epistemology. This concerned the question when, in effect, the epistemologist's job was done. Given that the ground of objectivity was sought in the form of representational systems, this question translated into: What kind of explicative treatment of representation in general was thought to be proper for epistemology? Given that the justification of scientific knowledge claims involved evidential statements, this question became: What was it about the basic sentences of science that epistemology had to explain? These were the candidates' answers to the adequacy question.

Carnap's criterion of adequacy of an account of protocol sentences lay in the formal explicability in principle of the operative concepts.
Schlick's criterion of adequacy of an account of fundamental sentences lay in establishing the applicability of a structural sign-system to reality.

Neurath's criterion of adequacy of an account of protocol sentences lay in establishing the practical controllability of their use.

Note, first, that, with their answers to the problem of epistemological adequacy, the agreement between Carnap and Neurath which was suggested by their common answer to the structuralisation question came to nought. Their formal-structural approaches towards the problem of justifying knowledge claims were really quite different. Whereas Carnap was concerned with the formal-logical properties of symbolic systems considered in abstraction from the conditions of their use, Neurath was interested in precisely those formal-logical properties of symbolic systems which allowed them to be used. Whereas Carnap placed no constraint on the abstractness of the structures from which he derived the objectivity of knowledge claims, Neurath insisted that the structures from which objectivity was to be derived be instantiated in linguistic behaviour and intersubjectively discernible. In short, Carnap and Neurath had two different programs in mind when they suggested that the justification of knowledge proceed formal-structurally. Neurath did not think Carnap's formal-structural approach sufficient to dispense of the epistemological problem.

Note, second, that Schlick rejected Carnap's formalist conception of epistemological adequacy for technical reasons (Wittgenstein's strictures) and for reasons of the scope of the anti-sceptical defense (demonstration of the actuality of knowledge). Neurath (who raised no technical objections) also rejected Carnap's adequacy condition for reasons of the scope of the anti-sceptical defense (demonstration of the actuality of knowledge). This would seem to suggest that the question of the scope of the anti-sceptical defense was the primary mover in the dissent from Carnap's conception of epistemological adequacy.
But consider this. Schlick was forced to reduce his foundationalist claim to the act of applying (part of) a symbol system. As a consequence, the regular claims of science do not participate in the certainty of affirmations. This Schlickean reduction of the scope of the anti-sceptical defense was as unacceptible to Neurath as Carnap's reduction of the scope of the anti-sceptical defense to the demonstration of the mere possibility of objectivity. Neurath agreed that what was required for the defense of science was a new theory of meaning. Unlike Schlick and Carnap, Neurath realised that, if that which was essential to representation and meaning was whatever it was that allowed for the interpretation of the statements of others and earlier selves, then no foundationally privileged domain remained for the individual epistemic agent.\(^{25}\) This in turn suggests that, for Neurath, the notion of intersubjective control of knowledge claims was at least as much of a motive for disagreeing with Carnap's conception of epistemological adequacy as the question of the scope of the anti-sceptical defense.

It may be wondered, however, whether Carnap was not in agreement with Neurath on this point: was the notion of formal explicability in principle not one which was also intersubjectively controllable, did it not import the criterion of the scientific rationality of formal science? If so, did the disagreement between Neurath and Carnap then simply boil down to a personal preference concerning how to answer the sceptical challenge, namely as pertaining to the possibility or to the actuality of knowledge? I believe that it did not. At issue here was also the question of how radical the sceptical challenge was perceived to be.\(^{26}\)
9.5 The Vienna Circle and the Problem of Scepticism

In (vi), the adequacy conditions on the epistemological account proposed by Carnap, Schlick and Neurath, we can recognise the outlines of (x), their distinct perspectives on scientific philosophy. What is expressed in the adequacy conditions of epistemology are reflections on the extent of sceptical challenge: "how much of everything" had been doubted? What, for example, remained presupposed in the candidates' accounts of representation?

9.5 1 Different Answers to Different Sceptics

Above, I characterised the critical problem with which the Vienna Circle was concerned as that of accommodating the creativity of scientific theorising without accommodating idealism. Now, accommodating the creativity of theorising raised the sceptical question whether we're not making it all up. At issue in each of the participants' answers was thus the question of how to show the sceptic that we indeed have grounds to claim knowledge of what we are talking about. The ambiguity of my formulation--"knowledge of what we are talking about"--is intentional: the scope of the knowledge claim here at issue includes knowledge of objects and knowledge of what a given discourse is about (call it "knowledge of meaning").

With the linguistic turn of the twentieth century the sceptical challenge was once more radicalised. No longer was it only the true nature of the world of which the possibility of knowledge was doubted; no longer was it either just that and, in addition, knowledge of the existence of the world that was doubted; now the possibility of knowledge of our own conceptions of things was called into question. Already the common Viennese "critical" stance recognised the difficulty of justifying objective knowledge by reference to private experience. The sceptical challenge just envisaged went
further still. The claim that we mean what we think we mean needed justification. At its most radical, the sceptical challenge even doubted the possibility of grounding our belief that we made sense. The Cartesian subject was attacked in its organising principles: clear and distinct ideas could no longer be presupposed to be available.

The disputants' responses to the problem of epistemological adequacy reflect the extent to which each recognised and/or accepted the challenge to the cognitive self-sufficiency of the Cartesian subject. It was his recognition of the sceptical challenge also in this second sense, namely against knowledge of meaning, that divided Neurath from Carnap and Schlick. The sceptical challenge was not understood in the same way by the disputants. Carnap and Schlick did not understand it to be as radical as Neurath.

I stressed earlier that Schlick and Carnap-1-3 were not traditional foundationalists as usually understood. They were at best non-traditional or non-naive foundationalists in response to sceptical challenge to knowledge claims about the external world. They remained traditional foundationalists, however, in response to--or rather in disregard of--the second, radicalised, sceptical challenge to knowledge of meaning. They renounced foundationalism with regard to knowledge of the external world, but they retained a foundationalist attitude towards first person knowledge and knowledge of meaning. They remained what can be called "internal" foundationalists for they held that first-person reports of the content of psychological states and of the meanings of assertions made were indubitable.\(^{27}\) The fact that Neurath faced the radical sceptic who challenged internal foundations had momentous consequences for his epistemological methodology.

\(^{27}\text{My usage of the term "internal fondationalism" broadly agrees with that of Romanos 1983.}\)
9.5.2 Neurath's Naturalism as a Response to the Radical Sceptic

With the radicalised sceptical challenge in play, four avenues of response to the traditional sceptical challenge to knowledge about the external world remained open. First, to ignore it as absurd and proceed as before. Second, to abandon justificatory strategies that required appeal to such internal foundations and to see what alternatives were available. One could acquiesce in the circularity of presupposing knowledge in order to explain how we come by it and simply grant that, just as there was no certainty for knowledge of the external world, there was no certainty for knowledge of one's own mental states or of the meaning of one's words. The third and fourth responses differ from the first two in engaging with the radical sceptical challenge. The third avenue of response would require showing the radical sceptic to be completely mistaken: internal foundations were available after all. The fourth, by contrast, would show the radical sceptic to be partly correct and only partly mistaken. It would show that we do have reasons to think we know what we mean but do so without reverting to internal foundations.

Schlick and Carnap responded to the radical sceptic along the lines of route 1. What was Neurath's response? Neurath accepted the radicalised sceptical challenge as showing something profound about how the traditional sceptic could not be answered. As he perceived it, the radical sceptical challenge forbade reverting to internal foundations. The epistemologist could not presuppose first-person knowledge of psychological content or of meaning as uncontroversial. Neurath thus did not respond in the fashion of avenue 1 or 3. Neurath's acceptance of the radical challenge, however, raised the question: If one accepted such radical scepticism then what was left with which the epistemologist could work? Here we must remember Neurath's naturalistic motto: "The possibility of science becomes apparent in science itself."^28 With even internal foundations challenged, the

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^28 Neurath 1932a:61. See also sects. 2.5.3, 5.3.4, 6.4.2, 7.2.3, 7.5.2, 7.7.3, and below chap. 11.
viability of working, as it were, outwards from within in response to the traditional sceptic was called into question. One might as well start in the middle of things. Let "science explain science"!

So explicated, Neurath's motto does not, of course, differentiate between responses 2 and 4. Later I shall consider further Neurath's argument along the lines of avenue 4, that not only was the naturalistic response possible and reasonable under these circumstances, but that it was the only possible response left open to the defender of scientific knowledge claims. Presently, my point is only to provide a contrastive sketch of his alternative to Schlick's and Carnap's understandings of what scientific philosophy came to. For present purposes, then, it suffices that Neurath be read as following the route of response 2. (Thus the question, whether Carnap and Neurath were divided merely by a theoretical preference for two equally possible strategies of responding to the sceptic, must wait for consideration until the next chapter.)

Regardless of whether Neurath responded a la route 2 or 4, then, Neurath's basic response to the radical sceptical challenge was to adopt the following strategy of justification: Science does give us knowledge if science itself can account for how it does so. Neurath's naturalistic strategy thus started out to explain science on the basis of the physicalism which science embodies. Science claims to provide knowledge of the natural and social world as constituted by interactions between spatio-temporally incarnate, ultimately physical elements. (Let's grant that this physicalism need not require a reduction of explanatory types.) Neurath's naturalistic strategy took this physicalistic picture of the world as given. (Neurath did not ask how we could know whether there was matter when all we were directly acquainted with was mental etc.) For the project of science explaining itself, reference to physical things, events and states of affairs was not out of order. In

29 See below sects. 10.5 and 11.5.
fact, such reference was absolutely basic. This meant that, contrary to traditional presumption, knowledge of apparently non-physical things, events and states of affairs like knowledge of other minds and knowledge of meaning, could only be invoked if it was itself physicalistically explainable.

Neurath accepted the radical sceptical challenge only in so far as it demanded that we give a justification of the challenged use of concepts. Neurath did not grant that these concepts were empty and our use of them utterly unjustifiable. Considering his agreement with the common Viennese strategy of seeking the ground of objectivity in a new theory of meaning, Neurath's naturalism amounted to requiring a physicalistic theory of meaning roughly along the lines I sketched above. For the project of justifying scientific knowledge claims this meant the following. We could account for the creativity of scientific theorising only with a conception of scientific rationality which derived objectivity from the embedding of logico-linguistic structures in a context of their use, namely, of interpersonal communication.

9.5.3 The Frame Problem of Viennese Meta-Epistemology

Even as incompletely characterised so far, Neurath's argument for the naturalisation of epistemology was clearly a meta-epistemological argument. Neurath held that the assumptions made by the epistemologist in the course of answering sceptical challenges to knowledge claims were not themselves excluded from these challenges and had themselves to be accounted for in turn. I now contrast Schlick's and Carnap's meta-epistemologies so that their three perceptions of the radicality of the sceptical challenge which they responded to are revealed.

Consider what the Viennese epistemologists worked with. To further the justification of knowledge claims, they engaged in the description of logico-linguistic
structures. The structures (variously described) were said to "make for" objectivity. But the mere descriptions of these structures did not do the epistemologists' job by themselves. Epistemologists had to give an account of what it was about these logico-linguistic structures that "made for" knowledge. The presuppositions of the preferred epistemological strategy had to be spelled out. We might say, the descriptions of these structures were to be "taken" in a certain way, they had to be "understood" against a certain background. Let me call this the "frame problem" of Viennese epistemology (for want of a better word). The frame of their epistemological reconstructions was, of course, the thesis, variously understood, that form is content.

All participants in the protocol sentence debate recognised that epistemology had to give account of itself. Consider Schlick and Carnap. Schlick renounced all philosophical and formal-theoretic propositions and, since his "Wende", declared philosophy a meaning determining activity. Carnap, in the final passages of the Aufbau and, differently, in his Logical Syntax, pointed towards what cannot be expressed and so sought to legitimise its own abstract formalistic concept of epistemology.

Note, however, that Schlick's and Carnap's conceptions of how the relevant symbolic structures "make" for knowledge remained an issue that was not formally explicable at all or only incompletely so. For Schlick, how the descriptions of structure of evidential statements were to be "taken" was a matter which could not be structurally-formally explicated. Schlick's negative answer to the structuralisation problem meant that the evidential statements had to be characterised in terms of their meaning, not form, and that their relation to the theories they served as evidence for was also not formally explicable.\[30\] For Carnap, how the descriptions of the structure of the evidential statements were to be "taken" was a matter of which linguistic frameworks they were to be seen as

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\[30\] See above sect. 8.2.4.
part of. Now, only an actually adopted framework could "make for" actual knowledge. What spoke for or against the adoption of any of these possible frameworks, however, could not be explicated in these frameworks themselves. These "practical" considerations of convenience remained informal ones. Clearly, Carnap's conception bore out his concern with only the possibility of objective knowledge. We must conclude that for both Schlick and Carnap the "frame" within which the reconstruction and justification of knowledge claims proceeded was not itself fully explicable, but had to remain "understood". Given the frame was the form-is-content thesis, this means that what had to remain "understood" for Carnap and Schlick was the grasp of the meaning of the concepts employed in the knowledge claims to be explicated. Just that, of course, was what Neurath felt could not be presupposed and left "understood". Compared to the self-styled "anti-philosopher" Neurath, Schlick and Carnap remained openly "philosophical" in their experiential and formalistic stances.

Now consider Neurath's strategy. Unlike Schlick and Carnap, Neurath held that how structures made for knowledge and how the descriptions of structure are to be "taken" must itself be fully comprehended in the epistemological theory. Neurath's naturalism demanded that epistemological accounts had to fit into a physicalistic conception of the world. This meant first of all, that the knowledge reconstructed had to be intersubjectively intelligible as such. No recourse to private phenomenal givens was possible. Secondly, this meant that the operative concepts of epistemology had to be such that they too could be fully comprehended in the scientific, the physicalistic scheme of things. Thus, the theory of meaning and concepts of like linguistic structure and logical form required a physicalistic explication. For instance, the structure or form at issue, namely that which made for objective knowledge, had to be intersubjectively recoverable. Neurath's naturalism constrained the resources of scientific meta-theory and the theory of knowledge. To partly repeat myself: If what is essential to representation and meaning is whatever it is that
allows us to interpret the statements of others and earlier selves, then no foundationally privileged domain remains for the individual epistemic agent, be that in personal experience or in meta-theoretical reasoning.

Neurath held that a scientific theory of knowledge must "explain itself" in a fashion similar to how science must make its possibility "apparent in itself". For Neurath, the modern sceptical challenge extended to philosophy itself. It did so because there were no internal foundations to which the epistemologist could recur. Neurath felt that not only did the claimant of knowledge need to be furnished with a justificatory account against the sceptic, but also that the epistemologist himself had to give an account of his own ability to provide justification. Only then, Neurath believed, could we be sure that no unjustifiable assumptions were smuggled into the justification of (first-order) knowledge claims.

9.6 The Vienna Circle's Three Guiding Conceptions of Epistemology

I can now turn to (x), the guiding conceptions of epistemology at work in the three perspectives on scientific philosophy. They express their perspectives on what kind of knowledge it really makes any sense to be talking about at all (for about the rest we must remain silent).

In my introductory chapter 2 I characterised the disputants' epistemologies in the following fashion. Carnap's was the alternative neo-Kantian focus on the possibility of knowledge; Schlick's was the non-naive foundationalist focus on the facts of knowledge; Neurath's was the naturalistic focus on the socio-historical conditions of knowledge. I can now flesh out these skeletons as follows.
9.6.1 Schlick's Guiding Epistemological Conception

Schlick's conception was, for all its sophistication, the most traditionalist picture of knowledge and epistemology. I pointed out in my review above that Schlick was in fact one of the pioneers of the structuralist approach to knowledge and meaning. That the conception of structural meaning did not, however, assume the dominance in Schlick's thinking which it assumed in Carnap's had two reasons. The first was Schlick's mistaken adherence to the Tractarian doctrine that logical form cannot be represented. The second was his traditionalist understanding of how the sceptical challenge was to be rebutted. Schlick's understanding of the sceptical challenge required him to show the actuality of scientific knowledge by providing certain foundations; at the same time, his understanding of the sceptic still allowed him access to a private realm of conceptualised experience, so that he could invoke first-person statements which were unfailingly true. Schlick remained a foundationalist in that he believed that the self-sufficient Cartesian subject could still find certainty worth finding.

Schlick restored epistemic authority to the subject by construing experience as a process of mediation. In experience, the particularity of the individual consciousness partook of the universality of structural determinations. The objectivity of abstract structures was thought to be inherited by the thoughts entertained in individual consciousness as the intuition of meaning. The comprehension of what was really nothing but complexity of form in the inexpressible experiential concreteness of ostensive presence was the ultimate foundational act in the drama of knowledge. For Schlick, knowledge was the perception of form in material. At the point of immediate ostensive application of a symbol system to the world, the abstract balm of objectivity-by-structure entered the bloodstream of individual conviction. Armed with the certainty of Schlick's "affirmations"--testifying to the communion with the legitimising abstract otherness of
unrepresentable form—the Cartesian subject resumed its work, the actual justification of knowledge claims.

What made Schlick's epistemology "critical" was his recognition of the limits of this method and the attendant change in the conception of wherein the epistemological project lay. What allowed him to think his epistemology successful was that while not providing the foundations expected of old, he nevertheless illuminated the very process at issue in the defense of empiricism: how abstract structures find application in particular reality. That scientific knowledge remained hypothetical in the end could not be helped: here even the "foundationalist" Schlick acquiesced in anti-foundationalist 20th century common sense.

9.6.2 Carnap's Guiding Epistemological Conception

My investigation supports the reading of Carnap as an alternative neo-Kantian. What I called the "new canon" of Vienna Circle scholarship arrived at this characterisation primarily through the consideration of his theory of the formal sciences. My investigation demonstrates the applicability of this interpretation of Carnap's philosophical orientation to his theorising about empirical science. Carnap's alternative neo-Kantian project was that of grounding the objectivity of knowledge claims in formal conditions on the representations employed. It involved both non-Kantian and Kantian ideas. Carnap's was an "alternative" neo-Kantian, first, in so far as his conception involved the common rejection of the synthetic a priori. The a priori framework of cognition which Carnap sought to establish consisted of purely analytic propositions and principles. Secondly, Carnap's "alternative" neo-Kantianism involved the turn away, as particularly Coffa emphasised, from concern with the constitution of the world to the constitution of

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31See above sects 2.3.3, 2.5.1, 3.3.2, 3.5, 4.3.2, 6.1.2, 6.4.1, 7.7.3, 8.3.3.
32Coffa 1986:59.
meaning. What, all the while, makes Carnap "neo-Kantian" is his continued concern with forms of representation as a priori conditions of knowledge.

In agreement with the new canon, I elevate Carnap's formalist approach to the problem of the justification of knowledge to the center point of his philosophy. The means-to-ends relation of formalism and phenomenalistic reductionism which the traditional view suggests is here inverted. The traditional view regards Carnap's logical edifices as ingenious devices for the pursuit of a traditional foundationalist end. According to the new canon, the phenomenalistic reduction of the entities bespoken by scientific theories is much less important than the general strategy to exhibit the formal aspects of knowledge claims and the meta-theoretical claim associated with it, namely, the claim that it is the form of cognition within which the claim to objectivity resides. The properly restored Carnap saw phenomenalism as instrumental to his "alternative" neo-Kantianism. Nevertheless, Carnap's phenomenalist tendencies were extremely significant. They bear out Carnap's adherence to the traditional conception of the self-sufficient epistemic subject.

Consider thus that Carnap renounced all relation to scientific practice. Note in this both the wisdom of declining to give a priori reasoning a deciding voice in practical matters and Carnap's decision to retain the domain of a priori reasoning as the proper one for epistemology. What objectivity there was lay in form alone: epistemology could do no more than articulate the logical structures of any sign-system.33 The justification of its

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33 It may be wondered what unified Carnap's prima facie very different positions. Did his alternative neo-Kantian ambition survive the apparent de-formalisation of meta-linguistic philosophical discourse, represented by the abandonment of his syntactic approach in 1935? What unified Carnap's different positions was his alternative neo-Kantianism. Carnap's assimilation of (i) to (ii)--of content to form--and thus his understanding of the affirmative answer to (v), the structuralisation problem, was conditioned by an answer to (vi), the adequacy problem, which, in the advent of formal semantics, happily accommodated the distinction of syntax and semantics. Remember his treatment of analyticity in Logical Syntax: that "analytic-in-L" could not be formally explicated in L but only in a meta-language did not prevent him from claiming analyticity to be a formal concept. Convinced by Tarski that he was really doing semantics all along, Carnap accepted semantics as yet another layer of formal determination, over and above strict syntax, wherein the objectivity of representations resided.
applicability was termed a practical matter, one outside the scope of the strict logic of
science. Carnap preserved the privileges of the Cartesian subject for the realm of a priori
inquiry: the presupposition of making sense was explicated in a variety of logically possible
linguistic frameworks and their formal rules (syntactic and semantic). Carnap's rational
reconstruction was a philosophical answer to a radically reduced philosophical problem:
his moral was that justification in principle was all that could be achieved from the basis of
what he assumed as given as data and reconstructive means.

9.6.3 Neurath's Guiding Epistemological Conception

Neurath, too, was concerned with the actuality and the possibility of knowledge,
but he considered both questions falsely conceptualised by Schlick and Carnap. Neurath
held that neither the actuality nor the possibility of knowledge could be investigated in the
fashion proposed by Schlick or Carnap. What might easily appear as Neurath's pre-critical
scientism was rather a naturalism which was, among other things, a response to Schlick's
semi-traditionalist foundationalism and to Carnap's alternative neo-Kantianism. Neurath
was the most radical and most misunderstood theorist of the Vienna Circle. He sought
develop a conception of meaning which, like Carnap's and Schlick's, required that what
meaning there is must find its embodiment in form. But Neurath did not only argue against
the academic philosophies of his time but also against Schlick's and Carnap's conceptions
of philosophy.34 His point of view rejected both the (traditionally attributed) program of
the naive marriage of phenomenalist empiricism and logicism and the (recently recovered)
program of alternative neo-Kantianism. Neurath's contrasting perspective may be roughly
put as follows. For Neurath, the rationality of science was not to be located in its ground
in private experience, as it was by Schlick, nor in the formal a priori presuppositions of

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34Already the "first" Vienna Circle to which Neurath belonged displayed a more subtle rejection of Kant
than that which the traditional view grants the Vienna Circle proper; see, e.g., the quotation from Frank
1949 in sect. 9.1.1 above.
meaning, as it was by Carnap; it was instead to be located in its practical medium of
intersubjectively accessible and thus controllable assertions.

Like Schlick, Neurath insisted that what rightfully could be called the theory of
knowledge must bear relevance to the concerns of practicing scientists. He could not rest
content with rational reconstructions of possible cognitive structures. Unlike Schlick,
however, Neurath disowned the Cartesian subject. Neurath could not find in private
experience the clarity and distinctness of ideas necessary to support knowledge claims.
What afforded clarity and distinctness of ideas was rather whatever it was that allowed for
intersubjective intelligibility: the coherence of a solitary epistemic subject could not be
presupposed. The subject matter of the epistemologist, knowledge claims, was one over
which the self-sufficient subject had lost control: to even see what it was that the
epistemologist was concerned with required empirical research. The question of what
knowledge was could no longer be answered by purely a priori reflection. Thus Neurath's
turn to the conditions of knowledge production and transmission: no longer could we
presuppose the traditional foundationalist picture, however sophisticated, of what
knowledge was, nor escape into the non-committal abstraction of structural representation.
Neurath suggested that knowledge was to be viewed as a communal project which had to
hold itself in place.

9.7 Summary

My sketch of the non-naive critical nature of all of the three Viennese scientific
philosophies speaks against some popular pictures of the Vienna Circle as proponents of
simple scientism, in particular, against Carnap as a reductionist phenomenalist, against
Schlick as a mere traditional foundationalist, and against Neurath as a naive materialistic
thinker. Most importantly, I differentiated their underlying conceptions of the theory of
knowledge and its object, of the scope of epistemological analysis, and of meaning. I also
demonstrated the extent to which they engaged with radical modern scepticism. Neither Schlick nor Carnap engaged with it to the extent that Neurath did. Further interesting questions arise from this and I shall explore some of them in this chapter. In the case of Schlick, they point in the direction of a certain philosophical dynamic Schlick shared with (and apparently did not copy from) Wittgenstein: from a structural correspondence theory of meaning to ruminations about language games. In the case of Carnap, they point, in the long run, to his debate with Quine. Now, what made Neurath's conception naturalistic was, as we saw, his demand that the terms of epistemological analysis be themselves "scientific" ones. What needs to be investigated further is his claim that Carnap's formalist project fails on this count, and the precise nature of his naturalistic alternative.
Chapter 10

EXTERNAL SYSTEMATIC ANALYSIS:
THE VIENNA CIRCLE EPISTEMOLOGIES
IN TRADITIONAL AND CONTEMPORARY TAXONOMY

Abstract: Detailed reconsideration of the epistemological problem situation: the regress argument, possible answers to it and the assumptions implicit in the sceptical challenge (10.1). The Vienna Circle epistemologies on this conceptual grid as tradition holds it to be applicable (10.2). Criticism of foundationalist wing of Circle (10.3). The expanded grid of contemporary epistemological taxonomy (10.4). Difficulties for Neurath's position (10.5).

The abstract epistemological problem situation in the light of which Neurath's Boat needs to be interpreted was only briefly described in chapter 2, before we began the descent into the historical determination of his answer. Now that we have emerged on the other side of the detailed reconstruction of the protocol sentence debate, it is worthwhile to recall this abstract problem situation in greater detail, cast the Viennese answers in the light of contemporary taxonomy, and so informed proceed to their evaluation, commonly phrased as "Didn't the Viennese project fail?" (We, of course, shall ask "Didn't the Viennese projects fail?") My interpretation of the Vienna Circle stresses that even its foundationalist wing was not "naive", but does my interpretation speak against the common picture of the Vienna Circle as embodying a failed program? My characterisations of the guiding epistemological conceptions of Schlick, Carnap and Neurath in the previous chapter do not answer the question whether the positions adopted provided successful defenses of the knowledge claim of science. I shall argue that even when the Viennese projects failed, they did so in a very interesting way. I shall thus provide the reason for my earlier claim that the
protocol sentence debate, contrary to common presumption, is not a tale of creeping rigor mortis of reductive empiricism: Neurath in particular raised questions still close to the contemporary debates in epistemology which, presumably, thinks itself freed itself from empiricist "dogma".

The view that the Viennese epistemological programs failed is perhaps most easily established by playing one party against the other, leaving each program itself in shambles. I shall try to pursue the strategy of having the Viennese contenders refute each other by trying to put their mutual criticisms, so far considered mostly in terms intrinsic to the protocol sentence debate, in terms of contemporary terminology. Thus I set the Viennese answers to the problem of justifying empirical knowledge, gained by historical reconstruction of the protocol sentence debate, on the conceptual grid of taxonomy. As the previous chapter has shown, contrary to common presumption, the deepest dividing line in the Circle set Schlick and Carnap on one side against Neurath on the other. Thus the questions directed to the plausibility of Schlick's and Carnap's epistemological conceptions will come from broadly Neurathian anti-foundationalist perspective, and those directed against Neurath from a broadly Schlick-Carnapian foundationalist perspective. We shall find not only that the traditional neglect of Neurath's naturalism inevitably leads to a shortened vision of the epistemological debate in the Circle, but also that Neurath's distinctive position and argument does not seem to "map onto" the epistemological taxonomy extended to comprehend most common contemporary responses to scepticism. This raises in a drastic fashion the question of the precise nature of Neurath's view. (This question is further considered in the next chapter.)

10.1 The Sceptical Challenge in Traditional Epistemological Taxonomy

Quite obviously, we will not get that far in adjudicating the Viennese dispute purely be means of the epistemological distinctions which the traditional view of the Vienna Circle
deems relevant to its interpretation. Yet for a first step it is not without interest to explore how the Viennese epistemologies in fact deal with some standard questions, and to note the tension that already exists in their different understanding of broadly shared positions.

10.1.1 The Problem of Justification: The Regress Argument

The sceptical challenge is easiest put in the form of a regress argument.\(^1\) This form of argument may be employed to further all kinds of sceptical challenges. There are, as I noted, many forms of scepticism; I am concerned with a defense of the knowledge claim of empirical science.\(^2\) Consequently, the regress argument must be read as concerning the basis of empirical beliefs. The regress argument says that for a belief B\(_1\) to be justified, it must be either self-justifying or be supported by a belief B\(_2\) which is justified. How is B\(_2\) justified? One answer is (1) that B\(_2\) is either itself self-justifying or supported ultimately by another belief B\(_n\) which is self-justifying. Other answers are either (2) that an infinite linear regress of justifying beliefs obtains, (3) that justification consists in a circular or holistic network of supporting but equally non-self-justifying beliefs, or (4) that justification ends with beliefs which simply are not justified. The sceptic holds that the "anti-foundationalist" strategies (2) through (4) fail to provide what can count as justification, and that the "foundationalist" strategy (1) fails for want of self-justifying beliefs. The "regressivist" answer (2) and the "contextualist" answer (4) are both highly implausible: clearly the justification is incomplete. The "coherentist" answer (3) is patently circular. The foundationalist answer (1), finally, folds before the "argument from error". This argument, familiar from Descartes, says, roughly, that since we have been mistaken about the justification of our knowledge claims in situations similar to present ones, we

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\(^1\) This form of argument goes back to Aristotle *Posterior Analytics*, ch. 2-3.

\(^2\) See above sect. 2.1. As we saw since, the question of how radical the modern sceptical challenge was understood to be was not without consequence for the protocol sentence debate (see above sect. 9.4).
cannot be sure that we are not mistaken this time around. (I thought my experience was real but I was only dreaming; how do I know I am not dreaming now?)

As scepticism grew bolder, the response became more radical as well. The traditional response to sceptical arguments is to challenge their validity. But this is not the only response possible. Even if one grants the validity of the sceptic’s argument, one is not constrained to accept the conclusion if one is prepared to challenge any one of the premises. As the sceptical challenge grew bolder, the response has taken this line increasingly often. This non-traditional response is to decline the challenge to show that we do after all possess the kind of knowledge which the sceptic says we do not possess about any given area or domain. The task is rather taken to be that of showing where the conception of knowledge goes wrong which, when properly thought through, leads to the sceptical conclusion. The two broad types of strategy correspond to the distinction between foundationalism and anti-foundationalism. The foundationalist defender of knowledge seeks to show the sceptics reasoning to be mistaken: there are foundations we can be certain of. The anti-foundationalist defender of knowledge instead focusses on the fact that sceptical standpoint is not without assumptions of its own and challenges one or more of them.

10.1.2 Basic Sceptical Assumptions

Different responses to the sceptical argument can be distinguished by their stand vis-a-vis the assumptions made by the sceptic. To distinguish foundationalism and anti-foundationalism we need only focus on three of the sceptic’s assumptions about the concept of knowledge. (For ease of subsequent discussion I shall, however, distinguish different versions of the first of these assumptions.)
The first of these is the joint assumption that the sceptical challenge itself must be taken seriously and that knowledge claims must be justified. (Sometimes it has been held that scepticism is a peculiarly philosophical kind of question which has no bearing on how we esteem our cognitive practice.) The justification requirement asks that reasons could be given, it asks for logical support: it leaves open, unless further specified, what kind of justification is to be given. To make the sceptical claim interesting, we are advised to construe the sceptic as liberal as possible. So we need not lumber the sceptic with asking for the obviously impossible. To make the strongest claim, the sceptic's assumptions better be weakest, or at least prima facie the weakest.

First, we distinguish "objective" justification from "subjective" justification, the absolute justification of a propositional claim, "given the facts", from the justification of a held belief, "given the facts from a perspective". We distinguish, in other words, whatever it is that makes a proposition true from what gives justification for thinking one true. The sceptic, arguing from error, need ask only for subjective justification. The sceptical challenge does not depend on contrasting the meager means of epistemic subjects with objective omniscience. The sceptic need only play on the potential indistinguishability of two situations from a subjective point of view. (Am I just a brain in a vat, wired up so to have the experiences I would have, were I a whole person in the environment I seemingly am interacting with?) Unable to distinguish the two, the notion of subjective justification collapses--so the sceptic argues--and so knowledge claims collapse too.

Second, we need to distinguish "having" justification from "showing" justification, that is, the possession of reasons from the ability to display these reasons in any detail. By requiring only justification of the former type it is claimed that epistemologists must provide only a reconstruction of (subjective) justification; epistemologists need not hold that the justification which can be reconstructed in defense of the knowledge claim must be
one which this subject can consciously produce at that moment. Again, note that the sceptic need only ask for the reconstruction of subjective justification.

Third, there is the distinction between the "propositional justification" of a proposition for a person and a person's "doxastic justification" of believing a proposition. A person may have subjective justification for a claim (may have propositional justification), but still have no doxastic justification in believing it for either he does not have an occurrent or dispositional belief to this effect at all, or has in fact also contradictory beliefs.³ Traditionally, it is held that the kind of epistemic justification wanted in the defense against the sceptic is doxastic justification. (We may for the moment go along with the supposition that all three Viennese epistemologies accepted this doxastic assumption; later, however, we shall see Carnap and Neurath straining against it.) The doxastic requirement limits what kinds of reconstructed subjective justifications are admissible: the legitimation of knowledge claims must essentially involve belief-states.

Note then that the sceptic need ask only for reconstructed subjective doxastic justification. Neither of these three determinations of the concept of justification are entirely uncontroversial; I will return to them below.⁴ Yet there are two more obviously controversial assumptions we must saddle the sceptic with, if his regress argument is to succeed.

The second of these large-scale sceptical assumptions is that the reconstruction of subjective justification requires that objective epistemic norms be appealed to. Call this the objectivity assumption. "Knowledge must grasp the Real." Again, in the liberal spirit, let us leave it open whether cashing this claim means that we capitalise even the definite article,
or that we de-capitalise "reality". What is important is that according to this assumption, knowledge and relativism are incompatible. Relativism, the sceptic claims, cannot count as an anti-sceptical response, a non-sceptical solution.

The other weighty assumption is that knowledge and uncertainty are incompatible: justification requires conclusive reasons. Call this the certainty assumption. The argument from error requires that fully conclusive reasons be given. So that this assumption not contradict the spirit of liberalism (making the sceptical argument as strong as possible) we should here require only certainty for the base, the basic, intrinsically self-justifying belief. It may be allowed that the relations of extrinsic justification of a belief in virtue of others ("epistemic ascent") provide for less than certain support.

10.1.3 Anti-Sceptical Responses

How do the anti-sceptical responses stand vis-a-vis these sceptical assumptions? Traditional foundationalism accepts all of the sceptical assumptions just outlined. Anti-foundationalism, most broadly construed, rejects the certainty assumption. Different anti-foundationalists are distinguished by what further assumptions of the sceptic they are challenging. Contextualism must challenge the objectivity assumption for it cannot sustain the unique justifications of systems of beliefs. No one system of claims whose justification simply breaks off is epistemically distinguished. (For the sceptic and the foundationalist, the contextualist violates two defensible assumptions, certainty and objectivity.) Regressivism must challenge the requirement that subjective justification be reconstructed. If it is potentially held beliefs that provide/possess justification, then the necessary logical incompleteness of infinite regresses will preclude justification, for that last justifying belief always elopes--but by definition of justification we need that belief to be potentially held.5

5If, by contrast, only propositional, objective justification needs to be reconstructed, then regresses may just be considered a matter of formal fact. Such seems to be Carnap's justification of mathematical
(For the sceptic and the foundationalist, the regressivist violates two defensible assumptions, certainty and concern for subjective justification.) Coherentism, as I shall show below, only needs to challenge the certainty assumption. But that is still enough to contradict the sceptic and the foundationalist.

In terms of the regress argument, the response to the sceptic turns on the question whether the demand for justification can and must come to an end and, if so, how it can do so. Contextualists agree with the foundationalist and the sceptic that justification can and must come to an end. They disagree on what is to count as a legitimate end (the objectivity and certainty objection). Coherentists and regressivists dispute the fact that justification must come to an end in the fashion imagined by the sceptic, the foundationalist, and the contextualist, namely, in a terminus of a linear chain of reasons. Coherentists and regressivists disagree about whether justification can come to an end. Regressivists hold that it cannot, but coherentists challenge regressivist's assumption--shared with the sceptic, foundationalist, but not the contextualist--that the extrinsic justification which accrues to a belief from another accrues to it only through linear reasoning. (Coherentists also disagree with regressivists over the former's assumption that subjective justification be reconstructed.)

Shelving concern with regressivism and contextualism for the moment, let us consider further the contrast between foundationalism and coherentism as the prima facie most plausible candidate for anti-foundationalism. I noted that the first sceptical assumption that justification requires the reconstruction of subjective doxastic reasons, is not uncontroversial. But as the terrain of justification is mapped out so far, the objectivity and the certainty assumption are the most controversial assumptions, if only because, in

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6 Regressivism has not been very popular at all; contextualism, however, receives attention by Wittgenstein and those inspired by his later work.
disputes between foundationalists and anti-foundationalists, the objectivity and certainty requirements are most often pointed to and declared violated by the other.

10.1.4 Foundationalism

The traditional response of foundationalism has been that an infinite regress, a circle, or a breaking-off of justification cannot be considered as a satisfactory answer to the sceptical challenge. Foundationalism holds that the justification of knowledge claims requires that we possess beliefs which cannot be doubted and which need not be supported by other beliefs. These "basic" beliefs on which justification builds must be self-justifying. The foundationalist thus owes an account of the nature of the basic beliefs, and of the relation of "epistemic ascent" from basic to justified non-basic beliefs. (Here again the sceptic may be liberal, as noted above. The relation of ascent need not be conclusive as long as the basis is certain: high enough subjective probabilities may make for subjective justification.) Foundationalism thus agrees with the sceptic in the contention that without certain foundations, knowledge claims cannot be justified. Foundationalism accepts the assumptions of the sceptic about what knowledge is, and sets out to show that, contrary to what the sceptic claims, we do possess it. (It is often paired with positivism, the reduction of knowledge claims to that which can be verified.)

10.1.5 Coherentism

Anti-foundationalism generally holds that no basic self-justifying beliefs can be found that could aid the justification of knowledge claims. (This allows for the possibility that self-justifying beliefs could be found but are held to be useless for the justification of the knowledge claims at issue.) Commonly, anti-foundationalists adopt the coherence theory of justification: what justifies a knowledge claim is the fact that it logically coheres with others we might make. What justifies a knowledge claim for the coherentist is not that
some basic beliefs imply the proposition at issue, but that the proposition is implied by other non-basic beliefs of the claimant, and does not contradict the rest. (Justification obtains because the claim logically coheres with some or all the other statements or propositions we believe, and contradicts none or as few as possible. Coherentists have considerable play in spelling out their basic concept.) Now, given the assumption that justification consist in the linear array of reasons with a terminus, circularity is easily held to be as bad a vice as regress: no terminus is found. Coherentists thus speak of the holistic support which accrues to a belief in virtue of a belief being lodged in an entire system of beliefs, such that the belief at issue coheres with this system and is, in fact, implied by some of the beliefs of this system. For the coherentist, it is simply a mistake to think that the concept of knowledge requires that the circle of justification must be broken with a belief that could not possibly be doubted. Yet coherentism faces problems of its own. When are we justified in believing that all our beliefs cohere? Coherentist must claim that the sceptic is mistaken not only in demanding self-justifying foundations but also in holding that any kind of certainty is required for justified knowledge claims.

10.1.6 The Instability of Moderate Foundationalism

It may be objected that foundationalists could similarly take recourse to a relaxation of the certainty requirement for the justification of basic beliefs. (As I noted, the foundationalist should be allowed relaxed criteria for justified epistemic ascent: otherwise no laws could ever be supported.) As opposed to the traditional foundationalist position of requiring certain foundations, the "moderate" foundationalist would thus require only prima facie justified beliefs for the defense of knowledge claims. It can be shown, however, that this modest foundationalist position is not a stable answer to the regress argument.

7Goedel's result suggests that justification cannot consist in the provability of coherence: for any self-consistent system of statements or propositions L, the predicate "provable-in-L" could only be proven in the meta-language ML and not in L itself. (Rutte 1982b.)

8E.g., Pollock 1979, following Alston.
Prima facie justification is, by definition, different from what we could call "considered" justification. Prima facie justification is defeasible and is not always justification.\(^9\) What provides full justification are beliefs to the effect that the prima facie justification holds good. Thus prima facie justified beliefs are not ultimately self-justifying. So construed, moderate foundationalism collapses into coherentism or some other version of non-foundationalism.\(^10\) (Alternatively it could be argued that, either, we do not possess such prima facie justified beliefs, and/or that it is not beliefs that possess prima facie justification but other internal states.\(^11\))

10.1.7 The Instability of Moderate Coherentism

Just as in the case of foundationalism and the certainty assumption, so in the case of anti-foundationalism, the candidate conception had better not make one obvious move to ease the burden of providing justification its way. That would be to give up on the requirement that justification is provided only by beliefs. It might seem arbitrary to construe the coherentism as holding that only beliefs can bestow justification. But if other things than beliefs can give justification, then justification is more than a matter of the coherence of beliefs. (This, again, follows from the doxastic assumption, shared by sceptics, foundationalists and coherentists alike.) To support their claim to capture the idea of justification, coherentists must insist that states other than beliefs may be causally necessary conditions of true beliefs, but that they are not relevant to epistemic justification.\(^12\)

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\(^9\)"Defeater defeaters" must surely be construed as further potential beliefs—as they are in Pollock 1987:appendix—unless we fall back into objective justification.


\(^12\)Compare Davidson 1982.
10.1.8 The Objectivity Assumption and Normativity

Yet there is still another kind of move which the coherentist might make to get his job done more easily. This obvious move would be to avoid the question whether there is a matter of fact as to whether epistemic norms are the right ones. Compare the foundationalist: he rules out relativism by a straight-forward "here it is" - the foundational beliefs anchor thought in the one and only reality. But without such foundations, what is to distinguish epistemic norms from mere cognitive habits?

Should the anti-foundationalist deny that justification requires appeal to objective norms? Coherentists hold that we do in fact break off the chain of justification at certain places, but that when we do so, we could, in principle, probe still further; yet we do not need to do so, we can let linear justification rest because of the holistic support that accrues to a belief from others of the agent in question.\(^{13}\) Coherentists face the problem of differentiating the justification possessed by incompatible knowledge claims lodged coherently into their respective systems of supporting beliefs. But having only holistic networks of beliefs to play with, how could coherentists provide such differentiation? Here sometimes a coherence theory of truth was invoked, but that is commonly held to be an unsatisfactory solution. (Truth requires more than coherence of beliefs: coherence could obtain without truth.)

So Coherentists, it seems, must abjure the claim to be providing such objective epistemic norms and instead must become descriptive because, no standard is available that would evaluate competing conceptions as untrue: all that can be stated is the incommensurability of the competing belief systems. In response, the descriptive anti-foundationalist holds that the concept of justification is simply descriptive of given

\(^{13}\text{Contextualists, by contrast, would hold that instead, when we can ask "no further", justification obtains from the sharing of communal beliefs or the participation in social practices.}
cognitive practices and thus relative to communities of justifiers; and does not prescribe an absolute standard. The descriptive anti-foundationalist simply denies that there is a fact of the matter whether a knowledge claim is really justified when the procedures governing the practice of justification in the claimant's community have been met: to the descriptivist, it makes no sense to question whether we should accept a knowledge claim on grounds over and above community standards.

This avoidance of engagement with objective epistemic norms runs the risk that the sceptic claims victory. The avoidance move consists in denying that knowledge involves objective epistemic norms. But if the epistemologist renounces objective normative claims, then the sceptic can claim victory by default: there is no ultimate question of right for knowledge claims.\textsuperscript{14} The task of the anti-foundationalist lies in providing an account of justification which carries legitimising authority. The coherentist must provide an account of epistemic justification as a principled kind of certification which is more than a descriptive account of when we do in fact stop the justificatory chain. The anti-foundationalist would be ill advised to moderate his claim and disclaim any concern with objective epistemic norms. Unless we assume that, in some sense, knowledge "grasps the real", the concept of knowledge loses its meaning: after all, it is supposed to be some kind of true belief. If the anti-foundationalist wishes to defend the notion of non-relative, objective knowledge then the task of accounting for epistemic norms over and above community standards cannot be avoided. In order that their reasoning not be seen as proving the sceptic's point, and turn into a kind of semi-scepticism, anti-foundationalists must not challenge the objectivity assumption on which true normativity turns, but some other assumption.

\textsuperscript{14}This is one example for the view that the value of scepticism lies not only in raising the question of fact, namely, do we have knowledge, but also in raising a more conceptual question, namely, what is knowledge? It does so because it raises the question of right, namely, are we justified in our knowledge claims, and so raises the question of what kind of justification is appropriate for knowledge claims.
10.2 Viennese Epistemologies in Traditional Epistemological Taxonomy

My determination to hold the foundationalist to certainty and the coherentist to normativity is not uncontroversial. Note, however, that the traditional responses to the sceptical challenge do trade on these assumptions. (Indeed, that foundationalists should challenge the certainty assumption and that anti-foundationalists should challenge the objectivity assumption, let alone the doxastic assumption or others not yet discussed, seems to be a relatively recent idea.) The taxonomy as laid out so far spells out the traditional criteria of success or failure of the various defenses of knowledge claims. The abstract epistemological argument developed so far could, of course, be continued, and will be. But we may stop here for the moment, because now we have enough to make the first differentiation between the parties of the protocol sentence debate, that between foundationalists and anti-foundationalists. Indeed, it is often thought that with the distinction foundationalist-coherentist as we have drawn it so far, the cognitive content of the Vienna Circle's protocol sentence debate is surely exhausted. We shall find otherwise. After this has become totally obvious we shall return to refine the epistemological taxonomy so far developed.

For the moment then, let's employ the taxonomy developed to capture what we have learnt about the protocol sentence debate. In the previous chapter, I differentiated the three conceptions of epistemology as Carnap's alternative neo-Kantianism as opposed by Schlick's experiential and Neurath's naturalistic turn, which are also opposed to each other. How do these positions show up in, or "map onto", the taxonomy as developed? Which of the sceptical assumptions did the Viennese defenses challenge? The sceptic makes the following a priori assumptions in the challenge to the knowledge claim of science. The sceptic assumes that (1) knowledge claims need to be justified by the reconstruction of subjective doxastic reasons, that (2) knowledge requires objective epistemic norms, that
(3) knowledge requires certainty. All in the Vienna Circle accepted assumptions (1) and (2) in some fashion. The first distinction that can be drawn between the conception of justification put forward in the Vienna Circle is that between foundationalists and anti-foundationalists. They divide on the issue of certainty.

10.2.1 On Certainty

In the previous chapter I showed that only Neurath was a true anti-foundationalist: Schlick, and Carnap until 1935, in fact required the certain immediate given for justification, and even the later Carnap thought he could appeal to it, if he liked. Most coarsely, then, the Vienna Circle divided on the certainty assumption. Schlick and Carnap-1-3 tried to uphold it and managed to retain it within the radically diminished domain swept out by what Schlick called "affirmations" and Carnap called "original protocols". Schlick and Carnap remained foundationalists because of their insistence on, and need of, a phenomenalistic protocol language. Neurath disavowed any. So, according to this first mapping, the Viennese epistemologists disagreed on the certainty assumption, but equally accepted the sceptic's assumptions that the challenge needed to be taken seriously and that knowledge needed to be justified with recourse to objective norms.16

Apart from the position it assigns to Carnap-2-3, this mapping agrees with the traditional view of the Vienna Circle. The traditional view pits foundationalists and anti-foundationalists against each other, grants their negative arguments, and beyond that holds the issue between them either confused or unresolvable. However, to see that this view lacks subtlety, to start with, consider the differences in their understanding of the justification requirement.

15See section 9.3 above.
16Assume for the moment that all met the objectivity requirement. (I stick with my argument above that the anti-foundationalist better not give up objectivity, just as the foundationalist better not give up certainty.) Whether such objectivity could in fact be provided for is a matter I shall consider below.
10.2.2 On Doxastic Justification

Did Schlick, Carnap, and Neurath all understand justification as reconstructed subjective justification? There is some tension already on the matter of what "reconstructed justification" means, and this points us towards their different understanding of the doxastic requirement.

There is the matter of Schlick's (and Neurath's) and Carnap's division over the issue of the scope of epistemological inquiry (are we aiming for de facto justification or merely justification in principle?) and its bearing on the form and means of reconstructing justification. Carnap certainly made the most of the freedom that "reconstruction" allowed; did Schlick, by contrast, challenge the delimitation for reconstructive adequacy only? Did Neurath? Note there is at least one minimal reading of the requirement for reconstructive adequacy on which they all agree: Justification need not involve only Occurrent beliefs. What they did disagree over was just how the reconstruction of the relevant subjective reasons was to go. What divided them here could be called the criterion of "ascriptibility". Was it sufficient, as Carnap held, that the reconstructed reasons be merely ascribable without violating some a priori canons of rationality? Or need one insist, as Neurath did, that they be such that a subject could in fact instantiate them as (dispositional) beliefs? Or did they have to be, as Schlick seemed to suggest, in fact dispositionally instantiated? The latter position here, of course, comes closest to the abandonment of the reconstructive approach. Yet it also did not wholly abandon it.

That no in one the Vienna Circle did abandon the reconstructive requirement, is of course the complaint of many of the critics of the Vienna Circle. The reconstruction-only

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17 See above sect. 7.7.1. (This is a first stab at interpreting Neurath's protocol sentences. More below in sect. 10.5 and ch. 11.)
18 See above sect. 4.5 and 8.2.4.
requirement has been attacked in the Viennese form of the distinction between the contexts of discovery and the contexts of justification. The reconstructive requirement held that epistemologists were only concerned with justification in the latter sense, that they were not concerned to give a "logic of discovery". Critics of Vienna Circle philosophy of science have typically pointed to the distinction of the contexts and alleged their merely reconstructive concern to be a mistaken assumption. But it is important to note the intra-Vienna Circle disputes over how the distinction of contexts was to be understood. Their conflicting criteria of ascribability map out radically different domains of reconstruction, and present radically different conceptions of the context of justification.

10.2.3 On Subjective Justification

There is also intra-Vienna Circle tension with respect to the condition that it is subjective reasons that need reconstructing. The reconstructive requirement allowed for abstraction from the particularities of experience. This leads to the question whose answers we already discerned in the understanding of the doxastic requirement: how much abstraction is defensible? The subjectivity requirement allowed that showing the grounds of justification need not duplicate the grounds of truth. It leads to the related question: how much subjectivity must be retained for reconstructed reasons to be ascribable as justifying beliefs?

As I introduced it above, the requirement for subjective justification seeks to preclude demand for the seemingly impossible. The idea was to allow that justification be less than fully conclusive: insight into the objective grounds of truth is not required to ground justification. At the same time, the requirement for subjective justification sought to tie the justification that is reconstructed to what the subject whose claims are being

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19See Hoyningen-Huene 1987 for a review of the distinction and its vicissitudes.
reconstructed could possibly have as reasons. What looks like a bonus, may not
necessarily be taken as one, however. The liberalisation of the demand from objective
justification to subjective justificiation presumes that it is subjective justification that must
be reconstructed. It might be held that that is too restrictive: if objective justification of a
claim could be given, why not use it in reconstruction? The idea here would be that, in a
Kantian fashion, the objectivity of some fundamental principles of science lies not in
picturing how what-we-cannot-know makes true what-we-know, rather, it lies in a shared
and bounded subjectivity, in the conditions of intersubjectivity. If, in other words, what
the epistemologist is concerned with primarily is the possibility of objectivity as a
precondition of particular empirical claims, then, "objective", justification may very well be
the tonic for the task. What other kind of justification could serve here? Note that the
distinction between epistemological concern with a priori and practical historical ("natural")
conditions of intersubjectivity here decisively shapes the theory in pursuit of
comprehending objectivity as intersubjectivity. For knowledge claims about the domain
Kant thought of as that of the synthetic a priori, and Carnap as that of the analytic, objective
justification was the only one available: here objective justification, the justification of a
claim "given all the facts" coincided with subjective justification, the justification of a claim
"given all the facts from a perspective".

Our earlier worry was: when would the dispositional reconstructive analysis of the
justification of a knowledge claim stop being the reconstruction of what we have any right
to call a "belief"? (The thought was that the reconstruction of subjective justification might
operate with such idealisations that the bounds of the humanly were, or seemed to be at
least, transgressed.) Might it now be argued that Carnap's explication of the possibility of
objective knowledge violated the doxastic justification requirement "given the facts as
believed from a perspective" for his a priori concerns what lies yet prior to belief?
Neurath's early complaint that Carnap provided justification in terms of purely formal of
structural determinations of beliefs which were not even in principle recoverable by the epistemological agent in question proceeded along these lines. It could be rephrased as follows: Carnap's reconstruction ended up abandoning subjective justification for objective justification and turned, at best, into a reconstruction of what matters of fact make formal claims true. Carnap presumably responded that the justification, so bestowed, can be transferred, under a wide enough construal of "reconstruction", to any beliefs in question: he'd be at pains to distinguish Kant's transcendental presuppositions of knowledge from his own efforts to exhibit the analytic constitution of representationality. Be this as it may, note that, in any case, it was Carnap's alternative neo-Kantian drift towards objective justification that was responsible for his adoption of the extremely liberal construal of the requirement for subjective justification and thus the doxastic requirement.

10.2.4 On Objectivity

As I stressed, the members of the Vienna Circle were not naive empiricists. Carnap's and Schlick's "critical" or non-naive foundationalism depended on subtle distinctions of wherein objectivity consisted. The possibility of objectivity rested in analytical forms which organised our cognition, so their alternative neo-Kantianism held. The possibility of objectivity was not therefore derived from the particularity of experience, but its universal form. The function which basic beliefs possessed was not that of providing the matter of objectivity, but that of mediating forms of judgement in which objectivity rested. This made both Schlick and Carnap non-naive foundationalists. Neurath's anti-phenomenalism, of course, precluded the very possibility of naive sensuous empiricism.21

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20 See fn. 5 above.  
21 But was he perhaps a naive realist? His early acquaintance with the conventionalist critique of positivism—see Haller 1985—rules this out. See also sect. 9.1.1.
Then there is the question, raised by the objectivity assumption, of whether, and if so what, a theory of truth be held. On this matter, the traditional view has it that Schlick held a correspondence theory of truth, whereas Carnap and Neurath held a coherence theory of truth. I noted my dissent repeatedly. It is clear, however, that the coherentist does owe an account of truth, or its absence, if he wants to cash his objectivity claim. Letting this matter rest for now, we may ask whether the objectivity assumption was intended to be met by any of the Viennese theorists. I argued above that unless the sceptic may claim an easy victory, the defender of knowledge better accede to the demand for objective norms. Objectivity requires norms robust enough to combat relativism. Did the Viennese scientific philosophers engage with this problem at all or did they go "merely descriptive"? My answer is that they did engage with it and sought to provide for epistemic norms.

Indeed, the central idea of Viennese scientific philosophy, that form is meaning, was intended to provide for objective norms: that was just the point of Carnap's and Schlick's alternative neo-Kantianism, and of Neurath's naturalism. According to the alternative neo-Kantian strategy, the rules of formation and derivation for formulae of symbolic systems and their axioms, in other words, the constitutive rules of the logico-linguistic frameworks themselves, were intended to provide the objective norms by reference to which knowledge claims were to be justified. The formal norms of language provided the epistemic norms needed. (The relation between a knowledge claim and its evidence statements was analytic.) According to Neurath's contrasting naturalistic proposal, it was whatever form or structure it was that went into the intersubjective

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22See above sects. 7.2.3, 8.2.2 and 8.3.2, and below sect. 11.4.
intelligibility of statements that provided for the epistemic norms required. Neurath aimed for the naturalisation of objective epistemic norms.²³

It is, of course, another matter whether these ideas succeeded. Did Schlick, Carnap and/or Neurath really show that form was not only determinative of content but also served as the fount of objective epistemic norms? All three Viennese scientific philosophies faced daunting challenges here. Foundationalists had to prove that they were "critical" enough to redeem their, after all, solipsistic starting point; Neurath had to deal with all the objections coherentism calls forth.

10.3 The Failure of Viennese Foundationalist Epistemologies

As we saw, the theories of justification put forward in the Vienna Circle were not finished products. Carnap certainly was the most fluid in his conception, but also Schlick and Neurath developed and changed their views somewhat.²⁴ To evaluate the Viennese epistemologies, it is best to consider their latest versions, that is, in terms of of their positions at and after the Paris congress. (Then Carnap's position also stabilised.) I begin with the criticism of the Circle's foundational wing.

10.3.1 Neurathian and Neo-Neurathian Evaluation of the Circle's Foundationalist Wing

The criticism to which Schlick and Carnap are liable is, as we shall see, in tone both Neurathian and post-Neurathian, that is, Late-Wittgensteinian and Quinean. This bears

²³This at present merely promissory claim is supported below, sect. 11.2 and ch. 12. For consideration of this question in the literature see Rutte 1979, Haller 1982c:31, Hempel 1982:15/6 and Nehmet 1982a:449.

²⁴See above sects. 8.2.4, 9.4.3, 9.6.1 for Schlick's changes, and 7.5.2.2 for a first indication of Neurath's change. As noted earlier neither of these changes resulted in significant changes of their epistemological position.
out, I believe, the correctness of Neurath's argument against their positions and of his understanding of the dynamics of their underlying conceptions.

Consider Schlick. Schlick's foundationalism did not extend, in the end, to specific pronouncements of empirical science, but merely to its claim to treat of reality. So far so good; yet what did Schlick put in place of the picture of scientific theories he had therewith rejected, namely, one which sought to comprehend theory and evidence within the same framework? What was the kind of knowledge he showed justified? Did he grant scepticism towards the hypothetical knowledge presented by science? Since that is highly unlikely, it seems Schlick subtly moved—as indicated by his last explication of his "affirmations"—towards thinking of science as an ultimately inexplicable "language game" or "form of life" (to use, somewhat anachronistically, the terminology of the later Wittgenstein). Schlick may well have held that the latter's rationality was not open to a meaningful sceptical question once the process of applying a suitably related symbol system in experience was philosophically comprehended. (Schlick argued that the bridge between affirmations and scientific propositions lay in the formally inexplicable rules of use which distinguished and related these two classes of statements.)

Consider Carnap. Quine later pressed the question of the individuation of intensions or meanings. Quine alleged that Carnap's use of the analytic-synthetic distinction continued to presuppose what it claimed to explain, a pre-analytic groundedness not granted by structure alone, a native familiarity with meaning. Carnap, however, thought this presupposition cashable in terms of a broadly psychologistic (early on even phenomenalist) reduction which allowed for a clear cut line that divided formal and descriptive concepts and formal and material questions. In response, Quine argued that

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25 Quine 1951.
26 Carnap 1955.
reference to observation concepts be replaced by reference to observation statements.
behaviouristically characterised. Quine argued, in so many words, that Carnap remained
rooted in internal foundationalism. Carnap's semantic observability-condition on protocol
predicates insured the success of the formal treatment of questions of knowledge, namely,
by a framework of semantic rules: yet it was only in an extenuated sense that formality still
ruled. A condition on the use of a sign-system which was not laid down in its own axioms
amounts to the specification of an intended meaning. (Quine is correct in holding that
Carnap presupposed all along access to meaning prior to any form.)

These broadly late-Wittgensteinian and Quinean points turn, in essence, on the
wisdom of ignoring what I called, in the previous chapter, the "radical 20th century anti-
Cartesian challenge". This kind of challenge to Schlick and Carnap animated Neurath's
central private language argument. Schlick's and Carnap's non-engagement with this
radicalised sceptic was highly problematic. Here is a brief sketch of the more strictly
Neurathian kind of argument to which they are vulnerable.

Schlick, as I showed, sought to defend the epistemic authority of the Cartesian
subject and even accepted serious limitations for its exercise. Once his view is extended, as
above, to include what would seem to be his final (pre)-later-Wittgensteinian response to
the frame problem of epistemology, however, the question arises whether the resultant
position does not undermine this motivation. If the defense of systems of scientific
statements as knowledge claims requires ultimately that the pursuit of science be seen as a
"form of life", then the authority of the solitary epistemic subject to justify knowledge

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27 Quine 1960.
28 In making this criticism I take Quine to be following a broadly Neurathian line of attack. See also
Passmore 1967 for an implicit recognition of this and Hegselmann 1985 and Koppelberg 1987 for a more
explicit discussion. Quine's arguments, of course, were not Neurath's, as far as developed up to 1934/5.
See also sects. 3.4.3, 6.4.1 and fn. 32 below.
29 See sect. 9.5.
claims has been forsaken. What I called Schlick's acquiescence into common sense anti-foundationalism thus spells the demise of the Cartesian subject after all.

Carnap's stance was problematical in another way. Neurath held that his conception was incompatible with naturalism. For Carnap to hold, then, that he did not make indefensible assumptions required him to claim that the native grasp of meaning which he presupposed could be physicalistically explicated. The question was, in other words, whether his methodological solipsism was naturalisable. (Conversely, it might be doubted whether the naturalisation of epistemology was a good idea; this returned the issue to the question whether the radical sceptical challenge had to be accepted.) Neurath criticised Carnap's idealising—and, he held, ultimately idealist and "metaphysical"—assumptions.30

10.3.2 Carnap and Schlick Fail the Objectivity Requirement

Do the Neurathian and the neo-Neuratian criticisms hold true? The questions about the adequacy of Schlick's and Carnap's defenses of scientific knowledge claims thus boil down to whether the radical sceptical challenge was properly confronted and disposed of correctly. As far as I can see the matter, Schlick did not properly confront and dispose of the radical sceptical challenge, and that, for the moment, it is at best an open question whether Carnap did so. Can we show, in terms of the epistemological taxonomy, how Carnap's and Schlick's evasion of the radical sceptic invalidates their epistemological theories? One way of putting this argument into our present taxonomy is to argue that Carnap's and Schlick's evasion led to their failure to meet the objectivity requirement.

30Compare Quine's argument, via his thesis of the indeterminacy of translation, was that intensions were not naturalisable. Both agreed, of course, that only a "radical" physicalistic conception of meaning was fit to serve in the theory of knowledge.
Carnap-1 would have succeeded, had it not been for the Aufbau's problem of the basic relation and the Neurath objection against the Aufbau's methodological solipsism. The former problem is that of the residual intensionality of the officially wholly extensional constructional language of the Aufbau. It may be viewed as the meaning-theoretic equivalent of the second, anti-solipsist objection, which unlike the first, was actually raised in the Circle. This objection said that basic statements with "remembered similarity" as their only descriptive constant were not intersubjectively verifiable, and so useless for legitimising scientific knowledge claims. Not even the possibility of objectivity, even intersubjectivity, was here achieved.

Schlick's and Carnap-2-3's withdrawal to the realm of "original protocols" or "affirmations"--statements outside of the system language in which scientific theories were formulated--also undermined the aim of providing objective norms. At a minimum we would want to hear some story about the embedding of these basic sentences in the language game of science. (Relaxing the logical criteria of ascent is one thing, abandoning individuation criteria for foundations is another.)

Schlick's final position was that the support which affirmations rendered to the theories of science was not a formal one; the explication of the informal support so given would, most likely, I suggested above, abandon methodological solipsism. Otherwise, no hope for providing objective norms remained. As originally pursued, in eclectic-nominalistic reductive terms, Schlick's epistemological theory failed the objectivity requirement.

Carnap-2-3's insistence on some sort of formal connection between extra-systemic protocol sentences and system-statements saved him from this objection. But against his position, there remained Neurath's private language argument. Neurath's private language

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31 See above for the former objection: sects. 3.4.3 and 6.4.1, for the latter objection: sects. 5.4, 7.4, 7.7 and 9.3.
argument thought Carnap's epistemologically phenomenalistical language not only useless due to its privacy, but objected even to the practical possibility of a language with a solipsistic sense. In practice, against the radicalised sceptic of meaning, methodological solipsism collapsed into solipsism-of-the moment. No objectivity here.

Carnap-4's difficulty was similar to Carnap-1's. Quine objected that, in his alternative neo-Kantian rational reconstruction of epistemic norms, Carnap presupposed knowledge of intension which he did not explicate on independent grounds.\textsuperscript{32} And as long as the semanticist Carnap took recourse to the relaxed phenomenological "reduction"\textsuperscript{33} he was of course liable to Neurath's private language argument. Still, was Carnap's objectivity claim thereby disturbed? To close the case of Carnap-4, we need to know more about the possibility of naturalising intensions and his continued assumption of individualism. This means not only that Quine's indeterminacy argument will have to be investigated, but also that Neurath's argument must be evaluated for its bearing on Carnap-4. Does Neurath's argument that the naturalisation of epistemology is the only possible response to the radicalised sceptical challenge hold even against a properly physicalist Carnap-4, if such he was, who continued to ignore the radical sceptical challenge?

Fortunately, the evaluation of Carnap's semantic conceptions lies outside the purview of this dissertation, so I need not pursue these arguments against a properly physicalist position of Carnap's. (In so far as Carnap remained an epistemological phenomenalist, Neurath's old arguments remained in force, of course). I shall merely remark that if, as seems to be the case, the properly physicalistic Carnap-4 also did

\textsuperscript{32}Neurath's arguments against Carnap-4 in their later correspondence seem to have run along broadly similar lines. (See Hegselmann 1985.)

\textsuperscript{33}See above sect. 8.3.3. What does it say about Carnap's non-phenomenalist defenses of empirical knowledge, that Carnap all along thought he could go phenomenalistic if he liked? Does it not mean that in his non-phenomenological defenses he made the same un-naturalistic assumptions he made when he used phenomenalistic reductions? Moreover, and separately, when Carnap did not rely on phenomenalism, was he not condemned to relativism of logico-linguistic frameworks?
presuppose knowledge of meaning in his reconstruction of scientific knowledge (as he did Carnap-1-2-3) then his alternative, namely, analytic neo-Kantianism failed (as did that of Carnap-1-2-3): logico-linguistic frameworks were intended, after all, to constitute the formal ground of objectivity and its possibility, so any reliance on an unexplicated prior choice of intended interpretation rendered the project to nought.

10.3.3 Neurath's Anti-Phenomenalist Argument and the Traditional Taxonomy

Granting the arguments for the logical inadequacy of the Aufbau and the Logical Syntax, it is clear that Carnap-1-2-3 failed the objectivity requirement. Such arguments were of course modern ones which had not been used in the protocol sentence debate itself. But I also argued above that Neurath's own argument was sufficient to establish the case against Carnap-1-2-3. Can the deciding features of the Carnap-Neurath dispute be mapped into the taxonomy as so far laid out?

Whereas Carnap remained seemingly obtuse to the radicalised sceptic, Neurath accepted the challenge to the Cartesian knowledge claim. What rendered Neurath's argument against Carnap's "critical" foundationalism persuasive was its re-delimitation of what is to count as evidence. In Neurath's book, the sceptic challenged Cartesian individualism correctly; what remained to show was the real ground which knowledge could possess. Neurath's position was resolutely anti-individualist. His private language argument against Carnap sought to replace the Cartesian subject with socialised cognition. The cognitively self-sufficient Cartesian individual was not something that could be naturalised--because there was no such thing or person. (Language mastery, and so intentionality-as-we-know-it, was irreducible to anything in or about the state of an

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34 And Carnap-4 qua epistemological phenomenalist. See also sects. 7.2.3 and 9.5.
individual in isolation, but was determinable only by comprehending this individual in his social relations as a language user.)

In play in Neurath's argument then were two elements not taken account of in the epistemological taxonomy as I have laid it out: Neurath's naturalism and Neurath's anti-individualism. Neither doctrine shows up in the traditional taxonomy. Not only is this taxonomy which traditionally thought sufficient to comprehend Vienna Circle epistemologies too coarse (1) to do justice to the "critical" foundationalism of Carnap and Schlick, and (2) to discern their different understandings of the requirement for reconstructed subjective doxastic reasons, but it also (3) fails to allow the representation of Neurath's true position altogether.

10.4 A Contemporary Epistemological Taxonomy

Before I can turn to evaluate Neurath's anti-Carnapian argument in neutral terms—

and, as promised, evaluate Neurath's position from the point of view of his Viennese Critics—I must first amend the epistemological taxonomy so as to accommodate his naturalism and, if possible, his anti-phenomenalist argument. In this section I therefore return to the epistemological argument about justification laid out earlier and expand the taxonomy to allow contemporary positions to be formulatable. In the next section I shall return to considering Neurath's naturalism and anti-individualism, and turn to a common argument against the success of Neurath's epistemology.

10.4.1 Apriorist and Naturalistic Anti-Foundationalisms

Short of providing for certainty, the anti-foundationalist will have to define what shall count as "good enough reasons" for holding a belief. Coherentists here put holism in pace of the assumption that extrinsic justification be linear. Yet they must tell us just how holistic justification works. (One will want to hear more than that linearity is given up and

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replaced by a holistic conception of justification: what is that?) There is one choice which the coherentist anti-foundationalist has and the foundationalist does not, which is particularly relevant here.35 The coherentist may challenge the presumption that justification even of empirical knowledge is a matter for a priori reason.

The coherentist may be an apriori or a naturalistic coherentist. The apriorist anti-foundationalist holds that justification (without certainty of the claims justified) can be achieved by means of recourse to a priori principles (other than the demand for certainty). Perhaps some a priori meta-principle could be found by recourse to which our justificatory practices are established as themselves justified.36 Alternatively, the naturalistic anti-foundationalist holds that justification can only be bestowed by a posteriori reasoning and empirical arguments for the claim in question. The naturalist may point out that the assumption that we do possess a priori knowledge about knowledge, namely, that it be certain etc., led to the sceptical conclusion. The naturalistic anti-foundationalist holds that we cannot begin to justify our knowledge claims without presupposing that we possess knowledge, and that such knowledge is natural, a posteriori knowledge and that a priori conceptions thus may fall where they may. The naturalistic anti-foundationalist thus holds that it is a matter of empirical investigation what knowledge is: the naturalistic epistemologist seeks empirical grounds for the concept of justification itself. The naturalistic anti-foundationalist thus acquiesces in the circularity of justifying knowledge by presupposing knowledge. Apriorist anti-foundationists (and sceptics and foundationalists) find this brute acquiescence intolerable. Aprioricists seek to provide superior grounding in a priori principles which order coherent systems of beliefs. On the other hand, naturalistic anti-foundationalists see themselves as pursuing one explication of what holism comes to:

35 See distinction between positive and negative coherence theories would not seem to possess the same relevance.
36 This is the strategy of BonJour 1985.
"science must explain itself". Naturalistic epistemologists will not tolerate the use of a priori principles in epistemology (beyond those embodied in the purely formal sciences). The knowledge process has to be understood in natural, i.e. scientific terms. (Needless to say, everything here depends on what counts as "scientific").

The aprioricist has a prima facie advantage here, for he could argue that it is totally unclear how coherence of facts yet to be justified can bring about the conformity to norms that is justification. In the view of the aprioricist, the naturalist must abandon normativity - and so, as a mere descriptivist must become a semi-sceptic by denying there to be a fact of the matter of "real" justification. Naturalists certainly face difficulties here. Note thus that naturalistic anti-foundationalists do not stop here in their argument. They go on to challenge any one or more of the sceptics underlying assumptions and so, depending on their temperament, either seek to disarm the demand for normativity or provide as much of it as it makes sense to ask for.

10.4.2 The Doxastic and the Internalist Assumptions

In distinguishing the coherentist from the regressivist, we saw the assumption challenged that extrinsic justification be linear. In distinguishing the naturalistic anti-foundationalist from the apriorist coherentist, we just saw the assumption challenged that a priori conceptions of what knowledge is must be met. One such a priori sceptical assumption which has been challenged in the pursuit of a naturalistic answer is the doxastic assumption. As we saw above, the doxastic assumption may be construed in different ways. Most coarsely, it simply says that justification is a matter of belief, that the good reasons we have for holding a belief are, if not that belief's self-evidence, then other beliefs. Doxasticists believe that justification is an intensional matter (and only because of this is a normative one).
I noted above that to make the sceptical claim interesting, we should construe his basic assumption of the justification requirement as liberal as possible. Thus the requirement for reconstructed subjective reasons does not require that the justifying reason be occurrent beliefs, but only such that they could be ascribed to a subject. We also decided to divorce the justification conditions of claims from the truth conditions of the propositions expressing these claims. Again here the presumption was that justification involve the reconstruction of subjective reasons because it is a matter of belief. The doxastic assumption thus says that justification is a matter of reconstructively ascribable beliefs. Anti-doxasticists say that justification is not a matter of only beliefs at all (occurrent or otherwise). Here the non-doxasticist breaks with the strategy of the sceptic, the foundationalist and the traditional coherentist anti-foundationalist. Other internal states of the epistemic agent can be appealed to in justification.

There is still another assumption which can be challenged. This is the internalist assumption. The internalist assumption is implied by the doxastic assumption but it can also be held independently of it. It says that only states internal to the epistemic agent can serve in the justification of a claim. The anti-internalist, or externalist holds that the justification of knowledge claims could revert to non-doxastic conditions, which are to be found in some sense outside of the epistemic agent.

Both challenges to the doxastic and the internalist assumptions uphold that knowledge claims must be justified by the reconstruction of subjective reasons. They focus on what may count as reasons. What the non-doxasticist says is that the reasons we have for making justified knowledge claims need not be beliefs at all. The having of justification instead consists in the possession of non-intentional states. Externalists go even futher down this road. What externalists say is that contingent states of the environment and our relation to them also play a role in the justification of knowledge.
claims. Externalism thus moves, in terms of the Gettier debate, furthest away from spelling out justification as we know it, namely as the justification of a belief.\textsuperscript{37} Whether a knowledge claim is justified becomes very much a matter of being in the right place at the right time. Unlike internalism, or simple non-doxasticism, externalism challenges the self-sufficiency of the individual epistemic agent. (Here externalism touches on contextualism as the latter's non-doxastic complement.) But even externalists need not abandon the reconstruction of subjective justification. They may hold that being subjectively justified simply involves the obtainment of certain states in the environment of the epistemic agent.

Anti-doxasticists thus can claim to go beyond the dichotomy of foundationalism and coherentism.\textsuperscript{38} It is to be wondered, however, whether something like the distinction between foundationalists and anti-foundationalists does not re-appear with internalist-externalist distinction. Internalists may be considered quasi-foundationalists who have given up on finding self-justifying beliefs, and who accept merely prima facie justified perceptual states as doxastically basic for empirical justification without becoming coherentists, for at the ultimate terminus of justificatory chains lie non-intentional states of the agent in question. Externalists, by contrast, may seek justification not in the material reasons of the belief, but in the method by which it is gotten. Externalists may thus seek justification not in how the justificandum belief is tied to the world in a direct fashion, but in the way in which the process by reference to which the belief is justified coheres with the object and the aim of cognition. Externalists may be considered quasi-coherentists who dropped the doxastic assumption.

\textsuperscript{37}Compare Goldmann 1979.
\textsuperscript{38}Kornblith 1980.
10.4.3 Naturalism and Objectivity

The naturalistic investigation usually begins with an investigation of the psychological processes of knowledge acquisition. What makes such an investigation possible is that the anti-foundationalist can challenge the doxastic assumption of traditional epistemology which holds that only other beliefs can justify a belief. But the naturalist can also challenge the internalist assumption. In either case, the naturalistic strategy of investigating what knowledge is hands an important part of epistemology over to empirical science, and thus to descriptive accounts. Unless the sceptic's assumption that the concept of knowledge implies objective epistemic norms were, after all, rejected, the naturalistic anti-foundationalist (doxastixc or internal or externalist) epistemologist must now ask: When would it be reasonable to break the chain of justificatory beliefs under the circumstances which a naturalistic inquiry into knowledge reveals as the biological, psychological and social constraints we must operate under? Given the objectivity assumption, this reasonableness better not be a parochial one. The conditions for breaking the chain of justification must therefore be such that they hold species-wide. Clearly, this naturalisation of epistemic norms is no small order.

10.5 The Difficulties of Viennese Anti-Foundationalism: Neurath in Contemporary Epistemological Taxonomy

Can we now map Neurath's anti-foundationalist argument onto the taxonomy as developed so far? How are the features that decide the Neurath-Carnap case to be understood in terms of contemporary taxonomy? Moreover, can we now evaluate Neurath's epistemology as a whole?
10.5.1 Trouble for Neurath's Anti-Individualist Naturalism

First, Neurath argued for a naturalistic epistemology. Does the introduction of the naturalism-aprioricism feature then render the taxonomical representation of Neurath's argument against foundationalism decisive? The answer is it does not do so. It prepares the ground, but does not clinch the point. Naturalism puts constraints on instruments of analysis and reconstruction. In "formal" terms, the naturalism-aprioricism feature distinguishes kinds of reasoning. Thus naturalism itself does not yet decide on the viability of Carnap's rational reconstructionism. Carnap could always claim that his foundations were natural.\[^{39}\] Naturalism does go a long way towards preparing the ground for the anti-Carnapian conclusion, however. It requires of all our analytical-reconstructive, that is, meta-theoretical concepts that they could weather the rough and tumble of explication in terms of processes antecedently understood as natural. But that does not yet decide Carnap's claim that, were one so inclined, a proper naturalisation of his Cartesian foundations could be given.

The second deciding feature of Neurath's argument was his anti-individualism. As we saw, the question whether to engage with this radical challenge or not provides a very deep dividing line in the Circle, one in comparison with which the foundationalist-anti-foundationalist argument about certainty and it's availability pales, though not into insignificance, but into the obvious. It was his anti-Cartesian, anti-individualist scepticism, to which Schlick's and Carnap's proposal seemed oblivious, and which for Neurath invalidated Carnap's and Schlick's "internal foundationalism", their an unquestioned familiarity with meaning. Now note that even as the contemporary

\[^{39}\]Indeed he did, early and late: Carnap 1928a:107 and 1955.
epistemological territory is laid out, the question of the radicalised scepticism of knowledge of meaning as confronted by Neurath has not yet surfaced.\footnote{It may well linger behind the justification of rejecting the doxastic and further the internalist assumption, but it does not seem to have to do so.}

It would seem then that our attempt at to evaluate Neurath's anti-Carnapian argument in contemporary terms is stalled: Neurath's distinctive anti-individualism is not captured by any of the positions of scepticism, foundationalism, coherentism, internalism or externalism. One form of anti-individualism does show up in our contemporary taxonomy, but only as externalism. Externalism is a non-doxastic anti-foundationalistic position. Yet wasn't Neurath supposed to be a naturalistic coherentist, someone who thus subscribed to the doxastic assumption? The question arises whether the taxonomy needs to be widened or new combinations of assumptions need to be considered, or whether Neurath's view is simply incoherent. What sense would it make to think of him as a "doxastic externalist"?

10.5.2 Neurath as Doxasticist and Externalist

I think it would only be honest to admit that our grasp of what kind of an epistemological position Neurath actually held is beginning to slip. Given it is Neurath we're dealing with, perhaps we should be prepared to jettison any preconceptions we may have of his epistemology. Let me take stock. It follows from my preceding chapters, that Neurath's Boat stands for Neurath's concern to develop a naturalistic defense of empirical knowledge. Now what precisely were Neurath's perceptions of and responses to the sceptical challenge? Which of the sceptical assumptions were challenged by Neurath and what did he put in their place? Any epistemology that finds its motto in Neurath's Boat clearly rejects the sceptic's certainty assumption, and thus, as I have construed it, rejects foundationalism. Neurath himself explicitly endorsed the naturalistic and rejected the
aprioricist anti-foundationalist position. But this still leaves other options open. Neurath is often pictured as a coherentist, wedded to the doxastic assumption. The question before us is whether that is correct, and, if so, how that sits with his anti-individualist naturalism.

Here are some good reasons for construing Neurath as a doxasticist. Neurath’s argument against Carnap’s and (Schlick’s) insistence in first-person authority on protocol sentences, the imaginary belief system evaluator which rings a bell when a contradiction is discovered, requires to succeed that it be construed as speaking about statements-held-true, thus beliefs.41 Also, Neurath’s coherentism, that only statements support other statements, clearly speaks about statements as expressing beliefs.42 Moreover, Neurath often signalled the difference between his own (socio-) behaviourism and the eliminativist behaviorism of Watson.43 Like Carnap, Neurath was a logical behaviorist: belief talk could be saved by dispositional analysis. And finally, at least in part, protocol sentences expressed beliefs, for that matter, perceptual beliefs.44 (It does not seem to matter whether their content was physicalistic or phenomenalistic.) According to these considerations then, Neurath, like his colleagues made the traditional doxastic assumption about justification.

Here is another argument for construing Neurath as an externalist, over and above his anti-individualism of his private language argument. To see it, consider the Viennese answers to what I called the “basis question”45. (Also recall that the doxastic and the internalist assumptions are closely linked: acceptance of the doxastic assumption implies acceptance of internalism; only the denial of the doxastic assumption allows for rejection of

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41 See Davidson 1982a.
42 Neurath stressed that the formal meta-linguistic mode discussed nonetheless intentional states, but without any metaphysical baggage in his 1933:G595/6.
43 E.g. Neurath 1931b:50.
44 See above sect. 7.4, 7.5.2.3 and 7.7.1 and below sects. 11.2.4 and 11.4.
45 See section 9.3 above.
internalism.) Schlick's experiential turn clearly made the doxastic assumption (and thus the internalist one): dispositional belief made up his "affirmations". Carnap as rational reconstructionalist upheld the doxastic requirement in a very loose fashion, namely, in spelling out the transcendental conditions of objectivity. Neurath's answer to the basis question was that the evidential statements of science spoke of the stimulation states of an organism and their relations to the environment. This suggests that with this determination of the protocol sentences, Neurath challenged not only the doxastic, but also the internalist assumption: the relation of an organism's stimulation state to its environment points clearly outside of that organism's body and thus lies also outside of its ken.

The puzzle is, of course, how Neurath could be a doxastic coherentist, implying internalism, and an externalist, denying internalism, at the same time. Note that there existed an at least prima facie tension in Neurath's views in the form of the rather curious position of a "doxastic externalist". This prima facie tension arose between, on the one hand, Neurath's across-the-board denial of the Cartesian assumption of the self-sufficient epistemic agent and his rejection of the idea of direct evidence, and, on the other hand, his simultaneous affirmation that knowledge claims could be justified. To render his anti-individualism consistent, Neurath had to drop the internalist assumption. Yet apparently Neurath did not, at the same time, challenge the doxastic assumption. Indeed, his anti-individualism said precisely this, intentional states cannot be comprehended as the states of an isolated individual agent.

So, can we now evaluate Neurath's anti-Carnapian private language argument in terms of the contemporary taxonomy? Since his argument traded on his externalist anti-individualism, it cannot be "mapped onto" the contemporary taxonomy either. In its terms, Neurath's own position would seem to be contradictory. Externalism denies doxasticism, and so coherentism. Either Neurath's argument must be faulted or the taxonomy needs
amendment, or both. There is still hope for Neurath, though, for it is a fact that even the contemporary taxonomy as laid out would, in any case, need further amendment to take account of some sophisticated contemporary attempts to solve the problem of empirical justification. It is similarly a fact that the discussion of various externalisms dominates current discussions in the philosophy of mind. Note then, that, whatever the final judgement on Neurath’s epistemology may turn out to be consideration of Neurath’s anti-Carnapian arguments lands us squarely in the middle of contemporary discussions.

10.5.3 Trouble for Neurath on the Objectivity Requirement:

Let me now also fulfill the promised strategy of trying to invalidate each of the Viennese conceptions of justification by the arguments of their opponents. Consider whether Neurath managed to meet the requirement of objectivity, a requirement which, he argued, Schlick and Carnap failed to meet. On this issue, Neurath received a mostly negative press, first so from Schlick. His supposed failure to meet the objectivity requirement has generally been taken to invalidate his entire program, naturalistic or not.

Here’s the argument: Neurath did accept the radical challenge and proposed to naturalise epistemology, but it must be wondered whether any defense of empiricism is given at all. Neurath held that evidence statements could themselves be doubted, so to what extent were knowledge claims then defended? How could consistent fairy-tales be distinguished from science? For Neurath, the problem would seem to be that he had to acquiesce in the sort of socio-cultural relativism for which Schlick (and Popper, Ayer, as Russell, to name but a few) criticised him. According to this common objection, Neurath

46 Thus in epistemology "internalist externalism" is a distinctly realised possibility: see Alston 1988 for drawing the relevant distinction between the accessibility of reasons--affirmed as internal component--and the accessibility of the adequacy of reasons--denied for externalist reasons.
47 Putnam 1975, Burge 1979. See e.g. Bilgrami 1987 for discussion.
48 For a further detailed review of Vienna Circle criticisms of Neurath, their influence, and their rebuttal see below ch.11.
had not succeeded in providing objective epistemic norms. (The Schlickean objection is of a kind often raised against coherentism. What I call the "objectivity objection", can, in fact, be further analysed as the combination of two objections, first, that coherentists cannot distinguish two equally coherent systems of belief in terms of their epistemic justification, and, second, that they cut off knowledge from the world it supposedly knows of.49 The objectivity objection, as put here, combines these two objection because it is commonly assumed that epistemic justification derives from well-groundedness of beliefs in the world they are of.) Note that this objectivity objection stands and falls with the assumption that epistemology be a normative enterprise. It might be thought, therefore, that Neurath had better become a merely descriptive naturalist, and that he did so. Yet this response, I argued above, would result in semi-scepticism. If we want Neurath’s naturalistic epistemology to be an ant sceptical response—and not a sceptical solution—we better see to it that Neurath be so construed that he can meet the objectivity condition.50

Is the criticism that Neurath violated the objectivity requirement a correct one? To evaluate it we must take account of the fact that Neurath may well have challenged the very conception of objectivity underlying this objection against him, while retaining normativity.51 We must also remember Neurath’s opposition not only to Schlick’s Wittgensteinian metaphysics of correspondence, but also to Carnap’s analytic neo-Kantianism. What defended empiricism for Neurath was not the objectivity of bare form nor the applicability of representational systems, but the demonstration of how questions of knowledge production could be pursued in empirical terms themselves such that "the

49See, e.g. Bonjour (1985:107ff, who distinguishes a yet further complaint, or Moser 1985:84ff, who discusses both sub-objections together as the "Lolitation" argument.
50In my conclusion I shall give reasons for thinking that Neurath himself wanted to provide such naturalised norms.
51Indeed, I believe he did, but I cannot go into this for to explore this would be to explore Neurath "encyclopedic" post-1934 position. The points I do make will lay the ground, however. See sect. 11.4 below.
possibility of science becomes apparent in science itself". Ignorant still of the full meaning of Neurath's obscure proposal for the form of protocol statements, we can conclude that, for Neurath, the form at issue—the form that provides objectivity—was a property of intersubjective language. Meaning was intersubjectively constituted. Ultimately, meaning arose from a kind of structure that was intelligible in interactive linguistic behaviour. Meaning was a matter of form in this extended sense of 'form' which comprehended the interaction of structure and context. For Neurath, meaning lay in the use of structures. That the defense of science by science, namely, by what the scientists of our cultural circle tell us is science, sounds "thin" was too bad, but nothing "thicker" could be gotten.

So far, so good; what Neurath must tell us, however, was just how the interaction of structure and context gave rise to representation so that it can be justified as knowledge. We need to know more about his obscure proposal for the form of protocol sentences than we have been able to establish so far. Neurath must tell us how, in other words, his naturalistic version of the form-is-content view worked. I believe the puzzle with which we broke off the analysis of Neurath's anti-Carnapian argument (doxastic externalism?) may be fruitfully considered if one wants to achieve clarity about Neurath's ability to meet the objectivity requirement. For answering the question whether Schlick was right or whether Neurath succeeded after all in meeting the objectivity condition, it is important to see how Neurath dealt with the doxastic and the internalist assumption. Challenges to these might provide the account Neurath needed to escape socio-cultural relativism, or at least the most blatant and objectionable forms of it. In the next chapter I will pursue this strategy of inquiry somewhat further, if in an oblique way. I shall consider the Viennese Neurath.

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52 Neurath 1932a:61.
reception and in response establish the desiderata which define Neurath's distinct
conception of naturalistic epistemology.

10.6 Summary: Current Evaluation Compared to Traditional Criticisms

In this chapter, I characterised the Viennese epistemologies in the light of traditional
and contemporary epistemological taxonomies. Their subtlety showed in their (differing)
understanding of the assumptions they shared with the sceptic. Neurath, moreover,
challenged more of the sceptic's assumptions than his "critical" foundationalist opponents.
I concluded that Schlick's and Carnap's epistemologies failed the objectivity requirement.
It turns out, however, that Neurath's argument to this conclusion is one which finds no
place in traditional taxonomy or in common contemporary taxonomy. This supports my
claim for Neurath's contemporary relevance, but it also raises the question of the tenability
of his conception, a question which may seem to be only compounded by the objectivity
objection to which Neurath, like coherentists generally, is vulnerable. Whether Neurath's
project succeeded or failed, is not all clear.

In conclusion, let me briefly raise, and bury, another issue that bears on the success
or otherwise of the Viennese epistemologies. The Vienna Circle's protocol sentence debate
is said to have been riddled with confusion. Carnap remarked at the Paris congress that up
to then the distinction between truth and confirmation had not been clearly drawn.53
Afraid to fall into variants of the liar paradox, the physicalists equated what empirical truth
there remained to speak about with what could be "verified" or confirmed.54 Hempel
suggested that, in addition, no clear distinction was drawn between the relative
confirmation of a sentence in virtue of its derivability from others and absolute confirmation

53 See above sect. 8.3.2.
54 I also noted Carnap's conflation of truth-criteria, truth-conditions and meanings in sect. 6.2.3.
of a claim by confronting it with an observational fact.\textsuperscript{55} Hempel's restrained coherence theory of truth of 1935 accordingly was really a theory of relative confirmation. (Given the physicalists' aversion to comparisons of propositions with facts, confirmation really could not have been conceived of otherwise.) Davidson notes unclarity amongst the physicalists between when an uninterpreted sentence and when an interpreted sentence was at issue.\textsuperscript{56} (Davidson's puzzlement here stems, I believe, from taking Carnap's "syntactic" avowals to renounce talk of meaning at face value.) Lehrer finally notes the Circlists' failure to distinguish between a statement's truth-conditions and its meaning (knowledge of truth conditions, if you like) which led Neurath, in his view, to the mistaken rejection of first person authority in the acceptance of final evidence statements.\textsuperscript{57} (This is a version of a very general complaint, namely, that the Vienna Circle's efforts were fated because of their verificationist theory of meaning.) Now clearly, any debate riddled with all these confusions at once is hardly likely to give rise to correct theories. But did the failures and difficulties of the Viennese epistemologies which I discussed in this chapter turn on these muddles?

Take Schlick and Carnap. The mistakes noted by Carnap, Hempel and Davidson pertained to the physicalists, so Schlick remained unaffected by them. (Schlick happily continued to think of meaning and truth in Tractarian terms.) The mistake noted by Lehrer, finally, affected both Schlick and the physicalists (but it affected foundationalists and anti-foundationists differently). Again however, the failure of Schlick's foundationalism did not turn on it essentially. Did the failure of Carnap's epistemology turn on any of these confusions? Again, this does not seem to be the case: committing these mistakes was not necessary for holding his Cartesian individualist assumptions. As I have construed the

\textsuperscript{55}Hempel 1959, see also Davidson 1982.  
\textsuperscript{56}Davidson 1982,  
\textsuperscript{57}Lehrer 1982.
issue above the decision to remain epistemological phenomenalists invalidated the foundationalist positions.

Now take Neurath: are the difficulties which his account faces due to his having made the said confusions? At first blush, it would seem so. Supposedly holding a coherence theory of truth, it is unlikely that he could meet the objectivity requirement for knowledge claims. And given the supposed refusal to speak of interpreted sentences and meanings, it is extremely unlikely that anything but mere formal coherence could be a measure of truth. Given the confusions noted by Carnap and Davidson then, the failure of Neurath's project seems predictable. But matters were otherwise. True, Neurath was a victim of the confusion noted by Carnap, but he was not as muddled about whether interpreted sentences were at issue in the justification of knowledge claims as Davidson suggests.\textsuperscript{58} First, note that it is consistent with Carnap's admission to hold that Neurath's apparent truth-talk was confirmation-talk: he did not hold to a coherence theory of truth. Second, recall my efforts in chapter 6: both Carnap and Neurath were still concerned with matters of meaning, albeit under a different name--"logical syntax"--and with differing conceptions how to do so. Furthermore, as I shall show in the next chapter, Neurath's coherence theory--if that is what it was--possessed resources over and above the criterion of merely formal coherence. Again then, it seems that the traditionally noted difficulties are not responsible for his difficulties either.\textsuperscript{59}

It follows that even in the case of Neurath the noted Viennese confusions did not lead to the failure or difficulties of their epistemological projects, at least as these failures and difficulties are construed here. In particular, Neurath's difficulties did not arise from doing the standard Vienna Circle program badly, rather they arise from his attempt to

\textsuperscript{58}Davidson himself notes that at times Neurath obviously has interpreted sentences in mind.
\textsuperscript{59}A full judgement must of course await the full reconstruction of what I call below his "theory of protocol sentences".
approach the question of empirical justification in a radically different fashion from his colleagues. They arise from his attempt at an anti-individualist, that is, doxastic-externalist naturalism. In the next chapter I shall thus review the Viennese Neurath reception with whatever hints we picked up about his non-standard naturalism in mind. Doing so will allow me to develop Neurath's response and to sketch, in my subsequent brief concluding chapter, an outline of his naturalistic epistemology which connects the bewildering details of Neurath's proposals with the insight of the Austrian school that Neurath's radically different approach to the problem of empirical justification was to conceive of it as a theoretical, but a practical problem.⁶⁰

Chapter 11

HOW THE VIENNESE NEURATH RECEPTION MISSED THE BOAT:

TOWARDS NEURATH'S THEORY OF PROTOCOL SENTENCES

AND META-EPISTEMOLOGY

Abstract: Overview of the Viennese Neurath reception and isolation of two types of criticism (11.1). "Mechanical" objections, due to Popper, Schlick and Hempel, concerned the question of how Neurath's alternative was supposed to work (11.2). "Ideological" objections, due to Carnap and Schlick, concerned the point of Neurath's alternative (11.3). Neurath's response is twofold: he envisaged a first-order meta-theory, the theory of protocol sentences, and a reflexive meta-theory, meta-epistemology (11.4).

Neurath's conscious efforts to develop a naturalistic epistemology in the Vienna Circle resist placement in the epistemological taxonomies common then and now. I return to question with which my the internal analysis of the protocol sentence debate during the years 1928-1935 has left us, and which reemerged towards the end of the previous chapter: What was Neurath's account of idea of the form–is–content doctrine and its underlying motivation? Showing where to begin to give such an account is the point of this chapter. I place Neurath's critics' problems with his proposal for the naturalisation of epistemology in the context of their understanding of him and the interpretation of Neurath I have advanced so far. This allows me to sharpen the question as to the precise nature of Neurath's proposal further, and eventually partially answer it. After an overview of the Viennese Neurath reception, I consider first the misgivings about the exact workings of Neurath's proposal. We shall see how the objectivity objection was raised in the Vienna Circle itself and how in answering it for Neurath from hints he provided, we can also resolve some
initial puzzles about the sheer working nature of his theory. Then I return to the question
of how Neurath's anti-Carnapian anti-individualist argument must be understood, and how
conclusive Neurath's demand to naturalise epistemology is.

11.1 The Viennese Neurath Reception

Neurath's proposal for the naturalisation of epistemology mostly fell on deaf ears,
in his day and since. There are three major reasons for its obscurity, over and above the
somewhat obscure manner in which it was put forth.

First, Neurath's proposals not only failed to gain acceptance and were
misunderstood in the Vienna Circle at large, but were also ignored by those closest to him
in the Circle. Some rejected Neurath's proposed naturalisation of epistemology because
they misunderstood Neurath. The "foundationalist" Schlick was not the only one who
disputed and partly misunderstand Neurath's position; a fellow "anti-Schlickean physical-
language-protocolists" like Popper (who can hardly be called a physicalist in the usual
sense) misunderstood Neurath even more so.¹ Even among the "physicalists" proper,
Hempel missed and Carnap apparently disregarded the point of Neurath's arguments.
That, finally, even other fellow Circle "left-wingers" Frank and Zilsel either followed
Carnap or Schlick, respectively²—sealed the fate of Neurath's project. Apart from his own
writings, no records remained of it.

Secondly, the distinctive thrust of Neurath's challenge was obscured by the partial
agreement with Neurath on the part of Hempel and Carnap. The impression that Neurath's
arguments carried the day—at least for a while—led interpreters to neglect that aspect of his
thought which did not win any followers at the time. That Neurath's own arguments were

¹For what Schlick's and Popper's reasons for disagreeing with Neurath's conception were, independent of
their misunderstanding of it, see above Appendix to Ch. 8 and below sect. 11.3.1 respectively.
²Zilsel 1933, Frank 1938.
often rather cryptically put did not help, of course. What pointers towards his naturalism there were, were interpreted as (bad) arguments for the position at least contemporaneously thought to be correct and more convincingly argued for by his colleagues. As we have seen, however, Neurath's own conception of 'form' was different from Carnap's whose understanding of it already was non-standard; already this means that we cannot rely on explanations appropriate for Carnap's conception to make sense of Neurath's.³ (In reviewing the initial critical reaction to Neurath's position I shall show that this difference has been all too often neglected, first by close sympathisers like Hempel and the Circle's "official opposition" Popper, though not in the Circle itself.)

Third, there are the serious difficulties Popper, Schlick, Hempel and Carnap saw with Neurath's proposal. To them, their objections to Neurath's project appeared fatal. Given the interest of subsequent analytical philosophy in the works of Neurath's critics and the relative inaccessibility of his own writings and ideas, his critics' appraisals of his work were accepted and, in some combination or other, still inform the contemporary opinion of his work. An unfortunate consequence of the wide acceptance of his critic's judgements has been the barrier they posed for the proper understanding of Neurath. Each critic saw Neurath's project partly through his own eyes only, and so misunderstood it. What was lost thereby was what none of his colleagues shared with Neurath.

Popper, Schlick, Hempel and Carnap thought they perceived serious difficulties for Neurath's proposal for the naturalisation of epistemic norms. Yet already they either misunderstood or ignored his argument. (By "misunderstanding" here I mean, first, the false attribution of views he did not hold, as by Popper, and, second, the failure to understand his views fully, as by Schlick and Hempel. By "ignored" I mean the failure to take account of his arguments even when they contradicted central thoses of one's own, as

³See above sects. 6.4, 7.4, 7.7, 8.3.3, and chapter 9.

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by Carnap.) A review of the Viennese Neurath reception which follows recommends itself, however, for more than archivist's reasons: the questions raised by the misunderstandings and the disregard of Neurath's proposal serve as a useful heuristic to the further analysis of Neurath's theory. For us here, these misunderstandings are instructive. Each of Neurath's contemporary critics specified important points on which Neurath must provide answers if his conception is to work. (That each critic was too involved in his own perspective to think through things from the Neurathian angle is besides the point here: making sense of the Neurathian angle at all requires that the critics be answered.)

The best way to make precise the critical question--what is Neurath's conception of 'form'?--is to divide the Viennese objections to Neurath's proposal into mechanical and ideological criticisms. This distinction means to separate the questions which deal with the question of how Neurath's conception was supposed to work from the questions which deal with its philosophical point, the view of knowledge which the technical apparatus was intended to support. (These questions can be asked of any epistemology. For instance, Descartes' answer to the mechanical question was: God; his answer to the ideological question was: the clear and distinct ideas of a solitary epistemic subject.)

11.2 The Mechanics of Neurath's Proposal Questioned: Popper, Schlick, Hempel

I first consider the mechanical objections to Neurath's theory: Did Neurath's proposed naturalisation of epistemology work? I begin with Popper's Neurath reading and then turn to Schlick's and Hempel's.⁴

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⁴I comment briefly on Carnap's qualms in this respect (see sect. 7.6.2) below in sect. 11.3.5.
11.2.1 Popper's Neurath Reading: Puzzlement over the Defense of Empiricism

In view of the somewhat diminished importance of Popper's argument for the protocol sentence debate proper it is not without interest to note that his criticism of the logical positivists' theory of science was flawed not only by his conflation of the views of other thinkers--e.g. Dingler--to the views of the Vienna Circle, but also by his failure to distinguish the doctrines of Carnap and Neurath. (Popper's relevant text here is Die Logik der Forschung of December 1934; Carnap-4 did not fall under its purview.)

The first thing to note about Popper's Neurath reading is a clear misunderstanding. Both Carnap and Neurath fell under Popper's classification of epistemological "psychologism". For Popper this view represented the confusion of psychological problems with epistemological ones. It held that empirical knowledge could be justified by reference to "immediate experience". Beliefs about the material world were legitimated as knowledge by sense experience. Carnap, in fact, did hold that such beliefs were legitimated to the extent that they were related to evidence as presented by protocol sentences (and that this relation was analytic). Popper rightly noted that the Carnap of the "Unity of Science" retained the fundamental ideas of the psychologistic approach to the problem of validating empirical knowledge: "the theory of protocol sentences is nothing but psychologism translated into the 'formal mode of speech'." Popper correctly believed that Carnap was a psychologistic foundationalist because only statements about immediate experience possessed certainty. Popper's own view (but not only his) was that

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5See above appendix to ch. 8.
6Popper 1935:96.
7This perhaps is the one point Neurath and Popper agreed on, though they reached this result by different arguments. See appendix to ch. 8.
knowledge claims could not be so justified because the certainty of these statements did not transmit to the knowledge claims of science.\(^8\)

Popper, however, overextended his correct diagnosis of Carnap when on its grounds he wrote that "much the same can be said of Neurath's view"\(^9\). Popper wrongly read Neurath as a Carnapian. He seems to have missed that Neurath did not require phenomenalistic protocols.

\[\text{[Neurath] demands that in protocol sentences such words as 'perceive', 'see' etc. should occur together with the full name of the author of the protocol sentence. Protocol sentences, as the name indicates, should be records or protocols of immediate observations or perceptions.}\(^10\)

Now, if there is one thing clear about Neurath's position it is that Neurath did not require protocols of "immediate observations" in Popper's sense of the term. In Popper's sense, "[a]n 'immediate experience' is only once 'immediately given'; it is unique".\(^11\) Popper failed to distinguish between Carnap's and Neurath's way of securing the empirical ground of scientific knowledge. In fact, the belief in the existence of any such immediacy as epistemologically relevant was the very conception Neurath argued against, whether it was held by Schlick or by Carnap.\(^12\)

Popper's misreading of Neurath was compounded by a second, in fact, classic misinterpretation of Neurath. The question arose as to what ensured that scientific knowledge be demarcated from non-scientific knowledge.

We need a set of rules to limit the arbitrariness of 'deleting' (or else 'accepting') a protocol sentence. Neurath fails to give any such methods and thus unwittingly throws empiricism overboard. For without such rules, empirical statements are no longer distinguished from any other sort of statements. Every system becomes defensible if one is allowed (as everybody is on Neurath's view) simply to 'delete' a protocol sentence if it is inconvenient. [...] Neurath avoids one form of dogmatism yet he paves

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\(^8\)Popper did not give up the assumption that knowledge requires certainty; thus he was led to the view that we only ever had, as it were, negative knowledge, namely when a statement was falsified.

\(^9\)ibid., 105.

\(^10\)ibid.

\(^11\)ibid.; 95.; compare also his 1963:199.

\(^12\)Compare Neurath's own rebuttal of this charge: 1935b:128.
the way for any arbitrary system to set itself up as 'empirical science'. Thus it is not quite easy to see what part the protocol sentences are supposed to play in Neurath's scheme. Since Neurath does not try to solve the problem of demarcation, it seems that his idea of protocol sentences is merely a relic— a surviving memorial of the traditional view that empirical science starts from perception.\(^{13}\)

Against Neurath, Popper took the position of a "relative" sceptic: he held that, on Neurath's view, science could not be distinguished from mythology.

Popper is right in claiming that Neurath owes an account of when and how to "delete" protocol sentences, but his final criticism is mistaken.\(^{14}\) Neurath's protocol sentences are a clearly syntactically defined class of sentences. Only members of this class can be employed in the process of theory testing. Furthermore, Neurath's protocol sentences contain statements about the stimulation state of an organism and its relation to the environment; that alone sets apart empirical science and arbitrary systems. Neurath did intend to solve the problem of demarcating empirical science from arbitrary systems of statements by identifying a class of statements which occupy an epistemologically epistemologically distinguished position: only to these do we recur in checking our theories.

It would seem that Popper simply discounted Neurath's efforts to set protocol sentences apart from other statements of science and read his proposal for their distinguished form as an inessential variation from his own proposal (which had been accepted by Carnap). Even without going into the details of Neurath's conception we must note that Popper did not understand Neurath's proposal even in its outlines. Popper's correct perception that Neurath, like Carnap, transposed talk of verifying experiences into the formal mode was confused by his attribution of Carnap's phenomenализation to Neurath.

\(^{13}\)Popper 1935:97.
\(^{14}\)Popper can only partly claim extenuating circumstances: while Neurath's amplification of the conditions upon the acceptance or rejection of protocols in his reply to Schlick was presumably not yet published when Die Logik der Forschung went to press, Neurath's caustic clarifications in Einheitswissenschaft und Psychologie, nevertheless, were. Popper only referred to Neurath 1932a and 1932d.
What Popper failed to see was that Neurath's naturalism forbade Carnap's phenomenalism. Thus he barred any recognition of the conditions Neurath had placed upon the deletion of "inconvenient" protocol sentences, for how, if these sentences spoke of the "immediate given", could conditions be placed upon it such that adherence to them was intersubjectively controllable?

Popper thus misunderstood Neurath in two ways. First, he attributed a phenomenalistic reading of protocol sentences to Neurath and so accused him of psychologism. Second, he complained that uncertain phenomenal protocols failed to demarcate science from non-science. While the first is just a mistake, the second points to a legitimate criticism, because, whether the uncertain protocols are phenomenal or physicalistic, Neurath's theory of protocol sentences had to explain how the form of protocol sentences aided the defense of empiricism! His rules of deletion had to be spelled out. Until that was done, we must admit, Popper had a right to be mystified and to doubt whether Neurath's naturalistic epistemology could succeed.

11.2.2 Schlick's Neurath Reading: the Puzzlement over Meaning Without Correspondence

Like Popper, Schlick too had a right to be mystified by Neurath's proposal, irrespective of what his misunderstandings may have been.

Recall Carnap's (official) abstinence from talk of reference and meaning in his Logical Syntax. Now recall Schlick's conception of meaning:

Every proposition is given empirically as a spoken or written sentence, a complex of physical signs, which is itself a fact in the real world: comparing a proposition with the state of affairs it expresses is, therefore, nothing but a comparison of two facts.¹⁵

¹⁵Schlick 1932b:228; compare also his 1935a:67.
Against Carnap, and the physicalists generally, Schlick clearly sought to defend a notion of correspondence between propositions and states of affairs expressed by them.\textsuperscript{16} Now compare Neurath's conception of meaning. Neurath held that questions concerning meaning (to be precise, concerning what Carnap had called the "correlation" problem of the expression and the designation relation) could be dealt with physicalistically:

The words 'blue' [...] indicate either that a man shows a certain behaviour under certain circumstance [...] or they indicate that there is a certain oscillation somewhere.\textsuperscript{17}

Neurath's view was that, first of all, linguistic events were physical events. Secondly, he held that these physical events could be correlated with other physical events. In this fashion Neurath was able to speak about meaning in terms of correlated information in two senses. First, expressions possess what might be called "expressive meaning" where the linguistic sign is correlated with an internal event in the speaker (as in Carnap's expression relation). Second, they possess what could be called "designative meaning". Knowing the designative meaning of an expression is to know (commonly) what external events outside of the speaker its use is symptomatic of, what (commonly) external events were correlated with the use of an expression type.

On the surface, Schlick and Neurath would seem to have endorsed views of designative meaning as correlated information which were not that far apart. The difference between Neurath's "correlation" and Schlick's "comparison", however, lay in what was the decisive terminus in the process of confirming a claimed correlation or correspondence. Here a veritable gulf opened between the two conceptions. Schlick assumed that a Wittgensteinian correspondence was what Neurath should have had in mind and, because he assumed this, he misinterpreted Neurath.

\textsuperscript{16}See above sects. 3.4, 3.5, 7.2, 8.3.1-2.
\textsuperscript{17}Neurath 1931b:52. See sections 3.3.2 and 5.4.2.
For Schlick, the idea of correspondence of statement and fact was absolutely basic to the conception of meaning. Given Wittgenstein's explication of how such a correspondence was mediated through logical form, Schlick concluded that the formal mode could not be rendered servicable for the theory of knowledge by the physicalisation of meta-linguistic discourse, as Neurath thought. Schlick adhered to Wittgenstein's dictum that logical form could not be represented and thus dismissed Neurath's as well as Carnap's efforts as mistaken. Schlick correctly held that Neurath needed a conception of content or meaning so that protocol sentences could be clearly distinguished from non-protocol sentences. He found, however, that Neurath gave only their form as what was peculiar to them. Neurath could not, on Schlick's view, provide their logical form. Since any non-logical form could have any content, Schlick may even have granted to Neurath that he could provide for expressive meaning, but objected that he could not provide for designative meaning. For Schlick, Neurath's conception of form was too thin.

Did Schlick here really "misunderstand" Neurath? Might it not be held that he understood him all too well? Schlick pointed to a lacuna in physicalist "syntactic" thought. Amidst the plethora of forms, how was the sense of realism to be defended which empiricism after all was meant to articulate? Without the requirement that the structures which science produced corresponded to something, how could empiricism be defended? (Thus Schlick's chastisement of idealist tendencies amongst the physicalists. Schlick need not even have falsely attributed Carnap's formalist conception to Neurath; his complaint may simply have been that he could not see how Neurath's theory of meaning, whatever it was, could anchor his epistemology as needed.) Given the cryptic rendering of Neurath's proposal, I must concede that Schlick's misunderstanding of Neurath is defensible. Schlick's question to Neurath may most concretely be put as: how is theorising about meaning without correspondence possible? Still, however, the fact remains that Schlick did not get Neurath right, as I shall indicate below.
It was but a consequence of their disagreement about the kind of relation involved in meaning—correlation or correspondence—that Schlick and Neurath disagreed about what kind of legitimation could be provided by the determination of the relata of the relation of sign and designated object. For Schlick, the process of confirmation was to lead, via the disclosure of the logical form of a proposition as its meaning, to the establishment of its agreement or disagreement with that of the experienced fact. For Neurath, correlations did not confirm statements, they themselves were what yet other statements expressed which in turn stood in need of confirmation; only a statement could confirm another statement. Justification could not be sought in some private immediacy. (That understanding any empirical sentence may itself require that the presence of physical objects or occurrence of bodily states are correlated with its assertion (under normal circumstances) was yet another matter altogether.) Schlick and Neurath were divided by the philosophical paradigm within which the test of truth or acceptability was conceived.

11.2.3 Hempel's Neurath Reading: Puzzlement over Observation Without Truth

What is perhaps most remarkable about the Vienna Circle's protocol sentence debate is that even amongst Neurath's fellow physicalists the distinctive thrust of Neurath's arguments was either overlooked or dismissed (despite the obligatory nods towards the initiatory role of his challenge). Hempel's reaction to Neurath is one of the two cases in question. (The other is Carnap.) The differences between Hempel and Neurath are obscured by their partial agreement. Officially, Hempel's response to Schlick was a defense of the Neurath-Carnap line. However, Hempel differed from Neurath on several counts.18

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18 This speaks against, e.g., Stegmüller 1954:354 and BonJour 1985:63.
First, Hempel agreed with Schlick that empiricism required a theory of truth. Hempel defended an--albeit "restrained"--"coherence theory of truth". Unlike Schlick, Hempel did not think it an objection that this physicalist theory of truth did not provide for foundations. Hempel agreed with Neurath in rejecting a concept of truth of the sort Schlick advanced, namely correspondence truth. Neurath, however, was uncomfortable with the label of a coherence theorist of truth already in his own reply to Schlick; later that year he explicitly rejected it. Hempel's youthful defense of radical physicalism obscured a central point of Neurath's argument which, unfortunately, was less than crystal clear to start with. As the later Hempel correctly noted, Neurath was not concerned with giving a theory of the truth but instead a theory of the acceptance conditions of scientific statements and theories.

Secondly, Hempel and Neurath differed over the form (and content) of protocol sentences. Hempel agreed with Popper when he wrote: "There is no essential difference left between protocol statements and other statements [...] Thus the concept of protocol statements may have become superfluous at the end." But Neurath, as we have seen, stressed the distinctive role of protocol sentences--a distinction reflected in their form (and content)--in his reply to Schlick.

Thirdly, Hempel and Neurath differed over how science was demarcated from non-science. It was Hempel's view (following Carnap-3 and Popper) that protocol sentences could not be distinguished other than in context. How then was he to answer the question

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19Hempel 1935a:54, 59n.6.
20Neurath 1935c:666. (For Hempel's own post-Paris-congress withdrawal of the claim that physicalists need the coherence theory of truth see his 1945:n.49.)
21Hempel 1982.
23This speaks against Romanos 1983:135.
by what historical facts "our" system of science is distinguished (in the absence of a logical
criterion) from those of a fairy tale?

[I]t is the system which is actually adopted by mankind, and especially by
the scientists of our cultural circle; and the "true" statements in general may
be characterised as those which are sufficiently supported by that system of
actually adopted protocol statements. 24

Because protocol sentences were not evidentially distinguished for Hempel apart from their
appearance in a context, Hempel's notions of truth and the demarcation of science from
non-science were contextual as well. Hempel's Carnapian defense of empiricism spelled,
as critics were not slow to see, a peculiar relativism. How could the systems of other
"cultural circles" be criticised as giving a true or false picture of the world? 25

Neurath added an important qualification to the picture of objectivity-as-communal-
agreement view here outlined by Hempel. His qualification was the addition of the
condition that protocol statements contain explicit reports of observations. Neurath
specified this condition in a totally different way from Carnap. (Carnap-3 remained, as I
showed, an epistemological phenomenalist and Carnap-4 had to invoke the "dogma" of the
anaytic-synthetic distinction.) 26 Neurath meant to do so by the elaborate form into which
protocol sentences were put. Neurath's preferred form of protocols allowed for the
specification of a more elaborate content than Hempel's, Carnap's and Popper's. It
specified, amongst other things, the condition that the state of affairs reported be an
observational one. The elaborate form of Neurath's protocol sentences had no other point
than to facilitate the understanding and management of what were complicated processes of
theory legitimation. Neurath's "coherence theory" was thus even more "constrained" than
Hempel's (apart from being a theory of justification, not truth).

24Hempel 1935a:57.
25See above sects. 7.7.3, 8.2.1 and 8.2.3 and 10.5.3.
26See above sects. 7.7.3, 8.3.3, 9.4--9.6, and 10.3.
Neurath's qualification, however, was overruled by the Popperian conception of basic statements accepted by Carnap and Hempel. What was missing in Hempel's account, therefore, was the explicit condition that protocols were observational. Thus Hempel's basic statements were relative to whatever a given culture might deem to be basic. Hempel was defenseless against Schlick's and Popper's charge that, if protocol sentences could not be distinguished from non-protocol sentences, then science and non-science cannot be demarcated across cultures. On the old view, science was distinguished from non-science because it was answerable to evidence and to it alone. Hypotheses were advanced to account for evidence and they led to predictions which were checked against further evidence. Protocol sentences were to provide this evidence; now, however, these evidence statements were contextual; thus protocol sentences as conceived by Carnap and Hempel did not provide the surrogate of the empirical evidence of old. If Hempel were asked what warranted the adoption of a system of scientific statements he could provide no answer.

By contrast, Neurath added a restriction to the objectivity-as-communal-agreement view which concerned the evidence class. Neurath sought to provide criteria by reference to which "our" adopted system of protocol sentences could be justified over and above the fact that we did adopt it. Such criteria which were simply unavailable once Popper's basic sentences were adopted as protocol sentences and coherence was deemed the criterion of truth. Neurath's restriction was meant to ensure that the system which was actually adopted was the one that should have been adopted.

Yet Hempel too had a right to be baffled and to claim to have extended, as it were, Neurath's theory into a coherence theory of truth only with noble reconstructive intentions. The question which is raised by Hempel's misreading of Neurath is this: how could his theory of protocol sentences provide a defense of empiricism without a concept of truth altogether, coherence or correspondence? (As we saw, a truth-less Carnap could in the end
always fall back on his epistemological reduction for the defense of knowledge claims as empirical; Neurath could not. What did Neurath put in place of Carnap's residual phenomenalism?) How could Neurath define protocol statements as observation statements in ways different from Carnap and yet decline all talk of truth? How, in other words, did his theory of protocol sentences do the trick?

11.3 Neurath's Theory of Protocol Sentences

Popper's "psychologistc" misunderstanding apart, his query to Neurath and Hempel's cavalier conflation of Carnap's and Neurath's theories raised important questions about the mechanics of Neurath's proposals. Popper asked: with protocol sentences "deletable" and no methodological canon provided, what prevented sheer caprice to run the business of science? Hempel wondered: what could distinguish the empirical data base of science as indeed empirical but the truth of its assertions which could conceivably be otherwise? Schlick's objection was perhaps already more attentive: he noted the difference between Carnap and Neurath and for that reason remained mystified, not just convinced that it was a mistake, whatever it was that Neurath was doing. But Schlick's criticism also raised an important question for the mechanics of Neurath's proposal of naturalistic epistemology. Schlick asked: how could Neurath claim to make objectivity intelligible without thinking of representation as correspondence?

I believe that Neurath had an answer to these published expressions of puzzlement by Popper, Hempel and Schlick. I shall call his answer Neurath's "theory of protocol sentences". I cannot fully answer the mechanical questions here, but only suggest that Popper's, Schlick's and Hempel's questions can be answered by noting Neurath's own pointers and seeing how they worked together. Not only does Neurath's theory of protocol sentences answer to their versions of the anti-coherentist objectivity objection, it
also answers to our own puzzlement as to how Neurath's epistemological theory is to be comprehended in contemporary taxonomy.

The first thing to note is that Neurath promised to provide for the objectivity of knowledge by the specification of the *acceptance conditions*—not truth conditions—of scientific evidence statements. I would thus argue in support of the later Hempel's reading as a coherence theorist of justification and against the early Hempel's (and Schlick's) reading of Neurath as a coherence theorist of truth (which continues to dominate opinion to this day²⁷). Neurath did not seek the justification of truth claims but sought to establish workable criteria for the acceptability of scientific theories. Neurath sought to "explain science by science" without presupposing any (substantive) theory of truth: this led him to look for an empirical basis for science whose determination did not presuppose one.

Second, note the mechanics of Neurath's empiricism without metaphysics ("truth"). For Neurath, the point of the notion of observation was not to arrive at a conception of empirical truth, but to establish a conception of evidence statements sufficiently restricted such that systems of assertions accepted by their means were such that their adoption was indeed warranted. That is to say, the conception of warrantability Neurath intended was, after all, meant to provide for the objectivity of knowledge claims over and above the intersubjective intelligibility even mere storytellers share. The important point here was that the distinctive observational status was itself determined by science, not by "philosophical" truth-mongering. The observationality condition on evidence statements had to be conceived of scientifically.

Third, note the conceptual mechanics of representation without correspondence.

Neurath's conception of meaning by recourse to which the objectivity of knowledge claims

was to be defended, was that of correlated information. Just where Schlick went private
and Carnap turned alternative neo-Kantian, there—on the all-important matter of meaning—
Neurath turned naturalistic. What made for meaning then was was not their
Wittgensteinian logical form, nor their Carnapian logical form; it was the use of symbols,
namely, what a symbol was commonly correlated with. What information was correlated
with a given statements was determined by the facts about the common usage of that
statement type. This was the point of Neurath's physico-behaviourist theory of meaning:
meaning as correlation, without correspondence.

Fourth, note the mechanics of Neurath's anti-foundationalist holism without
idealism, his theory of the "deletion" of protocol sentences. When Neurath repeatedly\textsuperscript{28}
attacked the idea of a "canon of scientific methodology" he attacked the idea of a special
path to "Truth" or "Untruth". To provide the criteria for the deletion of protocol sentences
was the very point of the "queer form"\textsuperscript{29} Neurath required of them. For Neurath, the trick
was to establish a form for the evidence statements which allowed the interaction of
different sets of criteria to be displayed. I believe, and would show in a continuing
inquiry, that it was not accidental that Neurath's protocol sentences had the form of a
threefold embedding of a sentence stating an observational material state of affairs.
Neurath's theory of protocol sentences meant to account for the epistemic norms of
acceptability by specifying three different sets of conditions with each of the three
embedding sentences. Recall his proposal:

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 table in the room perceived by Otto)]. [...] 
after 'deletion' of brackets, further factual statements appear which,
however, are not protocol statements.\textsuperscript{30}

\textsuperscript{28}E.g. Neurath 1935b.
\textsuperscript{29}Popper 1963:199.
\textsuperscript{30}Neurath 1932d:93. See also above sects. 7.4, 7.5.2.2, 8.2.2, 10.4-5, and ch. 9 passim.
Neurath stressed that this protocol sentence was deconstructible. Neurath's protocol sentences were anything but simple! Their complexity holds the key to Neurath's theory of justification. As he himself noted: "The protocol sentences must be built in a more complicated fashion so as to allow room for different variations."\(^{31}\) A protocol statement was typically a statement in which all the conditions laid down in its component sentences were claimed to be met. Those conditions are, as Neurath stressed, factual conditions. Here is one way of decomposing them into three components.

(i) 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 table in the room perceived by Otto)].

(ii) Otto's speech-thinking at 3:16 o'clock was: (at 3:15 o'clock there was table in the room perceived by Otto)

(iii) At 3:15 o'clock there was table in the room perceived by Otto

The three sets of conditions concern matters of perception, matters of thought, and matters of making of protocols. To accept a Neurathian protocol sentence means to accept that, at the place and time specified, a perception has taken place, that a thought was being thought, and that a decision had been taken to accord the statement of the fact perceived the status of an evidential status in science. Other "variations" of the combined acceptability of the component statements of the total protocol statement would lead to the rejection of the protocol statement in its entirety.\(^{32}\)

Now the reader may still be sceptical whether Neurath succeeded in satisfying all the conditions of empiricism to which his theory of protocol sentences committed him.

Particularly as regards the question of truth, it must be admitted, Neurath was hampered by the understanding of the concept about at his time. As is obvious by now, he thought the

\(^{31}\)Neurath 1933:G591.

\(^{32}\)A fuller account along these lines would provide a detailed analysis of how the "forms", i.e., the components of Neurath's protocols, translate into the specification of acceptance conditions. It would show how, contra Ayer 1936/7, some truth-functionality for protocol sentences is retained, after all: after decomposition, each one of the components must be acceptable.
correspondence conception metaphysical, but also was no friend of the coherence theory. In this respect his position was like Carnap's before Tarski's theory of truth became known to him. But whereas Carnap thought the metaphysical qualms allayed by Tarski's formalist explication of the concept of truth, Neurath was alarmed by Tarski's endorsement of his explication as a redemption of the correspondence conception. For Neurath this only showed that modern logic on its own did not hold scholasticism at bay. The question arises, however, whether a conception of meaning as correlated information could do without a theory of truth altogether. What Neurath should have wanted, so it seems, was something like a deflationary conception of theory of truth which recognises the need for the disquotational property of 'true' in talking about, e.g., unspecified, even unspecifiable statements. In a continuing inquiry I would show that Neurath noticed this ever so dimly: while he did not see that Tarski's theory did not imply a substantive metaphysical claim—nor did many others before Quine—he did show sympathies to the redundancy theory of truth. Thus he was led to the belief that he could do without the notion of truth altogether. I suggest we make things a little bit easier: Let us read Neurath as a proponent of what Arthur Fine has dubbed the "natural ontological attitude", a broadly deflationary conception of truth.

That said, let me now briefly return to our puzzle about how to place Neurath in contemporary epistemological taxonomy: Neurath did accept the doxastic assumption and did challenge the internalist assumption. Yet close attention to the form of his proposal for protocol sentences indicates Neurath's strategy. Neurath did not fall into incoherence because of the decomposability of protocol sentences. His theory of protocol sentences did

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33 Carnap 1936c:39, 1936/7.
34 E.g. Neurath 1935c and hence.
36 I do not think much violence is done to Neurath this way: he himself repeatedly endorsed the "natural conception of the world" of Richard Avenarius 1892. See, e.g., Neurath 1933:G609, 1936f:151.
not require that the very same linguistic entity satisfy either conditions. Different "parts" of the protocol statements address different sets of conditions satisfiable independently of each other. The conditions on the revisability of scientific evidence statements "fall out" of the various relations between the assertability conditions of a protocol statements and its substatements on their own. Neurath's theory of justification was multi-dimensional. Roughly, Neurath's theory was externalist in its pursuit of meeting the objectivity requirement with the observationality condition on the fact reported in causal terms. It was doxastic in its demand that justification involve intentional states, the thoughts of epistemic agents at least dispositionally ascribable. It was anti-individualist in its stress that the proposal and the acceptance of evidence were social processes (and the presupposed conception of meaning as information correlated with patterns of communal use of symbols). It was coherentist in its demand that the acceptance of factual evidence as such be supported by the fulfillment of the justification conditions spelled out in each protocol sentence (and its logical compatibility with other protocol and non-protocol statements).

11.4 The Ideology of Neurath's Proposal Questioned: Schlick and Carnap

I now turn to the questions which pertain to the point of Neurath's conception. These questions arise from Schlick's and particularly Carnap's responses to Neurath's theory of knowledge. Schlick was not only mystified by the mechanics of Neurath's defense of empiricism. He also had a difficulty with Neurath's account that was more ideological. Neurath sided with the radical sceptic in the attack on the Cartesian subject. This issue was ideological in that it concerned the underlying conception of knowledge. (It was on this issue that Schlick remained on Carnap's side, while on the question of the scope of the anti-sceptical defense--actuality vs. possibility of objective knowledge--he sided with Neurath.) I consider Schlick's ideological objection as a lead into the consideration of Carnap's Neurath reception. Carnap's reaction to Neurath's proposals
raised the ideological question even more sharply and thus helps to highlight the conception of knowledge behind Neurath's naturalism.

11.4.1 Schlick's Neurath Reading: the Ideological Point

Schlick and Neurath were clearly divided by their allegiance to different conceptions of what a scientific philosophy would look like. Schlick required foundations of the sort which Neurath's Boat had long precluded. Yet Schlick was no old-time foundationalist. He recognised that once stated in a public language any empirical statement was open to revision. What Schlick unearthed clearly did not merit the title "foundation" in a "logical" sense but, at best, in a "psychological" sense. We may even recognise his "affirmations" as a heavy-handed attempt to explicate the causal conditions of the making of protocols.\(^{37}\) Schlick's turn to such "psychological" questions of course was prompted by his desire to show the actual foundations of scientific knowledge. As I put it in the previous chapter, Schlick sought the actuality of knowledge in the act of application of a symbol system. It was Schlick's view on how to illuminate the process of the application of a symbol system which still occasioned sharp disagreements between him and Neurath, when all along, they both disagreed with Carnap's conception of the task and form of scientific philosophy and both insisted on the actual justification of knowledge claims.

Neurath and Schlick agreed on the point of the insufficiency of Carnap's syntactic reconstruction to uphold empiricism. This holds true even though the disputants themselves may not have noted this themselves. Schlick and Neurath arrived at their actual agreement by different trains of reasoning and sought to remedy the shortcoming differently. Where Neurath required complicated scientific system statements, there Schlick required extra-systemic "affirmations". What Neurath thought to require

particularly careful wording in order to be reckoned with in the formal mode, Schlick claimed as the province of philosophy to be explicated as rules of language use that are not capturable by the axiomatisation of theories as linguistic frameworks. For Schlick, the formal mode was unable to exhibit actual foundations of scientific knowledge; the formal mode could not represent how a system of concepts applied to reality—that could only show itself. For Schlick, the truly foundational step in the exhibition of the actuality of knowledge was experiential: the application of a symbol system to reality was to be viewed as an act, a deed.

Neurath here, in fact, agreed with Schlick up to a point—only to disagree all the more sharply thereafter: what for Schlick was an individualistic act—the application of a symbol system—was a social process for Neurath and could not be individualistic! It follows that it was a mistake to seek the warrant for scientific statements in immediate experience. (This response of Neurath's to Schlick remained, of course, implicit but it follows from his response to Carnap to which I shall turn next.) Since Schlick obviously found Neurath's anti-individualism unacceptable, his mechanical objections to Neurath's project (noted above) were compounded by ideological ones.

10.4.2 Carnap's Deflection of Neurath's Radical Challenge

What was Carnap's criticism of Neurath's conception? That is by no means obvious. Carnap rarely addressed this important matter and, when he did, not in a decisive way.

Apart from the worry whether it was advisable to have protocol sentences say of themselves that they are protocol sentences or in other ways explicitly specify the observationality of the matter reported, Carnap never publically criticised Neurath's view as
mistaken (though he did reject Neurath's criticisms).\textsuperscript{38} Some passages in his autobiography suggest that he viewed the disagreement with Neurath (like his disagreement with Quine) as not over facts, but over the convenience of equally possible research programs.\textsuperscript{39} What this shows is that Carnap thought Neurath's position in principle acceptable. It does not show that Carnap agreed with Neurath in all respects.

While taking due note that Carnap preferred Popper's to Neurath's proposal it is often thought that Carnap came to agree with Neurath in all essential points (at least for a while)\textsuperscript{40}: the evidence statements of science were to be formulated in the intersubjective physicalistic language and to be viewed as revisable. What is overlooked in such an assessment is that Carnap still thought it a possibility to formulate protocol sentences in a private language and have them remain unrevisable in this form (and that until the Paris congress epistemology had to fall back on this phenomenalist reconstruction).\textsuperscript{41} Carnap came to make significant concessions to Neurath's attack on methodological solipsism and yet managed to deflect the force of Neurath's criticism. (When he scrapped the intertranslatability thesis in so far as it meant complete reducibility, he did so on independent grounds.)

Carnap never addressed Neurath's challenge to the plausibility and theoretical fertility of individualistic assumptions in epistemology.\textsuperscript{42} It is true, of course, that Carnap

\begin{tabular}{l}
\textsuperscript{38}Carnap 1932e:465, 1936/7:9-13. \\
\textsuperscript{39}E.g. Carnap 1963a:23/4. \\
\textsuperscript{40}E.g. Joergensen 1951, Belke 1967:24, Quine 1984. Heidelberger 1985:183 carefully hedges his bets, noting that Carnap allowed himself to be convinced by Neurath "to a large degree" ("ein grosses Stueck weit"). See section 9.3 above. \\
\textsuperscript{41}See above sects. 7.3.1, 7.7, 8.3.3, and 9.3. \\
\textsuperscript{42}Though darkly hinted at already during stage one of the debate, Neurath's challenge was most explicit during its second stage. Their remaining disagreement only deepened over the years. (In his semantic period, in fact, Carnap again moved further away from Neurath's view. Carnap's first step away from the reach of Neurath's radical challenge was effected by the semantic turn of his perspective on the nature of rationally reconstructivist theorising. The new point of view allowed him to somewhat compatibilise his alternative neo-Kantianism and his non-radical physicalism: no longer was a phenomenal reduction required for epistemological purposes.)
\end{tabular}
came to recognise the intersubjective physical language as basic—soon even the
intersubjective everyday thing-language\textsuperscript{43}—and he continued to consider protocol sentences
revisable at least in principle, however begrudgingly\textsuperscript{44}; all the while, however, Carnap still
recognised the possibility of basing science on a phenomenalist language.\textsuperscript{45} That an
epistemic agent had to rely on the intersubjective language for the systematisation of her
experiences was something Carnap never conceded. That Carnap thus never accepted the
challenge to knowledge to be as radical as it was for Neurath is, I believe extremely
significant.

We do well to suppose that, just as Neurath was not ignorant of the alternative neo-
Kantian dimension of Carnap's formal philosophy\textsuperscript{46}, so Carnap was not ignorant of
Neurath's naturalistic project. Accordingly, Carnap must have had a reason to ignore
Neurath's challenge. To see what it was we must first determine precisely what the
argument was which Carnap deflected. I deal with this issue in the next three sections and
then return to the question of what Carnap's reason for this deflection was. As I noted,
Carnap did not discuss this issue in his publications. Carnap's deflection of Neurath's
challenge, and his implied criticism of Neurath's argument, requires as careful a
reconstruction as the challenge which it criticised.

11.4.3 The Difference between Carnap's and Neurath's Physicalisms

Carnap's and Neurath's central and continuing disagreement was about the
necessity of formulating protocol sentences in the regular physicalistic language of science
(and about the form they should take). Behind their disagreement lie their different

\textsuperscript{43}Carnap 1936/7.
\textsuperscript{44}Carnap 1938.
\textsuperscript{45}Carnap 1936/7:9, 1961:v, vii, 1963b:945/6. (It seems that Quine's 1951 charge that the analytic-
synthetic distinction rested on the possibility of a phenomenalistic reduction correctly applies to Carnap.)
\textsuperscript{46}See above, sects. 4.3.4, 6.2.3, 6.7.3, and 8.4.
understandings of the argument for physicalism and against the use of a phenomenalist language. In my review above I spoke—only somewhat anachronistically—of their different "private language arguments". Comparing their difference will throw light on Carnap's deflection of Neurath's sceptical challenge.

To discuss this matter now with some clarity, let me fix the following terms. A phenomenalistic language that is to serve any foundational purpose at all must be capable of being known and used independently of scientific fact, or facts about the physical and the so-called hetero-psychological realm. Let us call this the "individualistic assumption" behind the appeal to phenomenalist language. We might thus say that a phenomenal language that serves the purposes of epistemology must be "private". Against the practical possibility of phenomenal language and the infertility of its theoretical assumption Neurath argued that a language must be usable by one individual over time. Let us call this the condition that there be "constancy of use".

11.4.3.1 Carnap's Private Language Argument

Carnap cast his 1930 acceptance of physicalistic and revisable protocol sentences in the form of an argument for the thesis that the physical language was the universal language of science. Phenomenalism was only discussed under the guise of the refutation of the objection that physicalist language could not comprehend talk of mental phenomena. Against this objection Carnap fielded what was in fact an elaboration of Neider's 1929 argument againsts the methodological solipsism of the Aufbau. Carnap's private language argument may be summarised as turning on whether phenomenalistic evidence statements were intersubjectively testable. Thus Carnap concluded that scientific evidence

47 See sections 6.2.1, 6.3.2, 6.4, 6.5.1.1 and 6.7 above.
48 Carnap 1930b, 1932b, 1932c.
49 See above sects. 4.4, 6.3.
statements had to possess at least a physicalistic extension. He did not question the assumption that evidence statements spoke ultimately of the "immediate given". (He held, in fact, that phenomenalistic statements were used in epistemology.) In the Aufbau, Carnap had endorsed psycho-physical parallelism, but around 1929, Carnap rejected ontological phenomenalism but retained epistemological phenomenalism. (Ontological phenomenalism assumed the immediate given to be non-physical entity. Epistemological phenomenalism could allow the immediate given to be an ultimately physical state, namely the stimulation state of an organism. Epistemological phenomenalism holds that, whatever its ultimate make-up, the immediate given serves as the first base for epistemological reconstructions of knowledge claims.)

It was not until 1935 that Carnap engaged the question of the intertranslatability from the physicalistic to the phenomenalistic languages. (This intertranslatability he had assumed since the Aufbau.) Then Carnap concluded that ordinary statements about physical objects were not equivalent to any finite conjunction of phenomenalistic statements and that only an incomplete "reduction" of physicalistic statements to phenomenal ones was possible. Neither his adoption of Neider's argument nor this failure of translatability, however, did imply for Carnap that phenomenalistic statements have no evidential role in science, still less that a phenomenal protocol language was not possible. Both of the latter were distinctive theses of Neurath, which were seldom discussed, much less conceded.

Carnap's private language argument in effect provided for a private phenomenal language by showing it to fit into the scientific, the physicalistic scheme of things. Carnap's version focussed on the extension of the languages available in and compatible with unified science. His argument for a physicalistic interpretation of the protocol

50 See above sect. 8.3.4.2.
51 See above sect. 7.3.3.
language showed that, unless they shared their extensions with expressions of the
physicalist language, the protocol statements bore no evidential relation to the statements of
physicalist science. Still, however, the phenomenal protocol language spoke only of an
individual's experiences. Thus even a solitary individual could use this language: the
individual was fully apprised of the sense of its expressions (though not its extension) and
thus could ascertain what was the content of his thoughts and psychological states
completely unaided by others.

11.4.3.2 Neurath's Private Language Argument

Just that self-sufficiency of the solitary individual, however, was challenged by
Neurath. In addition to the papers cited in my review above, a lecture given in April 1932
(between the publication of "Physikalismus" and "Protokollsaetze") gives insight to his
reasoning. Neurath argued:

All the problems with the "hetero-psychological" are familiar from the field
of the "auto-psychological". If Robinson wants to establish correlations
between his anger and his work efforts then he must use the diary. [...] 
About what Robinson "really felt" when he wrote down his note Robinson
will have as unsuccessful a conversation with himself as with another. The
"experience" of yesterday is just as "alien" [literally: "hetero" as in "hetero-
psychological"] as the experience of the other.52

I suggest the following interpretation. Neurath argued that anything which solved the
problem of reidentification of intra-subjective objects (e.g., how I can know that my
experience now is of the kind that I anticipated 5 minutes ago?) would also solve the
problem of reidentification of intersubjective objects (e.g., how can we know that the
objects before us are the same?). The conclusion was that the tempting line of reasoning
from the corrigibility of statements about what our common experience was to the
conclusion that the evidential basis for science must be sought in language that speaks only
of the experience of an individual speaker could not be right.

52Neurath 1932g:105/6; my italics.
Up to the break in this quote Neurath would seem to be concerned, like Carnap, with the verification conditions of psychological statements. He argued that no radical discontinuity obtained between the verification of statements about other minds and one's own. That was the point of stressing Robinson's inability to know what he "really felt" any better than any other person (with access to the diary): a "conversation with himself" was of no help. This means that Neurath thought that the constancy of an individual's language use could not be presupposed as unproblematic. The question is of course why that should be so.

Neurath held that any language use was socially mediated. That one person understood another, of course, presupposed that the extension of the other's expressions was a public one. But Neurath's point did not stop there. A speaker's sense of the meaning of the expressions of his/her language was shaped in and by the process of communication: it did not precede it. For Neurath, the public accessibility of the extension (of at least some if not most) of the expressions of a language was also a precondition of the constancy of the use of a given language by an individual. How could the constancy be assured? By being corrected by other speakers! What this meant was that a speaker's sense of the meaning of his/her own words depended on the public accessibility of their extension. Accordingly, there could not only be no language with exclusively non-public extensions but also no language whose sense was essentially private. (If it were otherwise, how could the constancy of use be guaranteed?)

11.4.4 The Communalist Reading of Neurath's Private Language Argument: Neurath's Attack on First Person Authority on Meaning

Where, the reader may wonder, did Neurath explicitly put his private language argument as reconstructed? Here I must agree that it would indeed have been very nice if Neurath had made his argument more explicit in his publications of the protocol sentence
debate up to 1935—but he did not. Still, my investigations above strongly pointed towards it. In the appendix to this chapter, I consider and disarm the objection that not only my name for, but also my interpretation of Neurath's anti-phenomenalistic argument as a private language argument is anachronistic. Here, I briefly recall some evidence from the debate and present additional evidence from Neurath's post–1935 writings which shows that this interpretation was intended by Neurath all along.

In support of my interpretation of Neurath's private language argument two facts may bear repeating. First, there is the fact (which I already noted in my review above) that in his "Protokolsätze" Neurath reasserted his private language argument of earlier papers in support of his contention that there could not be a phenomenal protocol language. He did so after Carnap had presented his own version of the private language argument: clearly then, Neurath must have thought his version of it to go beyond establishing the mere impossibility that the protocol language may have non-public extensions. What could this be but the contention that already the sense of language must be publically accessible? Second, my reading of Neurath's argument accords well with his 1933 remark that the phenomenal protocol language is "unrealisable".53 For Neurath, formal reasoning was pertinent to the theory of knowledge only if it concerned languages which humans could possibly speak. Carnap's phenomenal languages violated this condition.

Yet still over and above Neurath's remarks in the papers cited in chapter 6, and in the lecture quoted in the previous section, and in the historical connections discussed in the appendix to this chapter, there is Neurath's own reprise of his private language argument in 1941. Neurath's original private language argument and its various restatements dealt with the verification conditions not of third person, but of first person statements. The self-understanding of an individual language user was clearly as problematical for Neurath

53Neurath 1933:G591.
as was the understanding of others. Just this train of thought, already present in the earlier manifestations of his private language argument, was most clearly brought out in a passage from Neurath's lecture to the Aristotelian Society in 1941 in which he took issue with Bertrand Russell's view of meaning.

A discussion of Robinson Crusoe with himself is full of 'operational' problems dealing with social implications of language. Russell is of a different opinion [...] It is remarkable how [...] Russell [...] imagines the 'ideal' constancy of language, how he imagines two people or a man at different periods using identical languages. [...] This tendency of Russell to imagine a solitary thinker of absolute constancy of personality explains perhaps his opposition to our proposal that all statements should be regarded as historical elements. [...] Our proposals lead to [...] a stressing of the social implications of language.54

Russell was criticised here for not recognising that the problem of when expressions have the same meaning is the same as whether one speaker at different times or different speakers at the same time are involved. That the problem of the constancy of language use lies at the heart of Neurath's views about the protocol language and that the solution points to the essentially intersubjective character of language was clearly stated here.55 Neurath's lecture presented an overview of his encyclopedic conception of the language of science and the structure of scientific theories and repeatedly referred his listeners to earlier articles of his; it did not state any new views but amplified his old ones. Neurath's argument here echoes his earlier ones, e.g., "The Robinson of yesterday and the Robinson of today stand in precisely the same relation in which Robinson stands to Friday."56 Such echoes strongly suggest that the passage cited from Neurath's address to the Aristotelian society recalled his reasoning during the protocol sentence debate when his private language argument was put forward.

54 Neurath 1941a:228/9.
55 Incidentally, Russell 1948 produced an argument against solipsism roughly along these Neurathian lines, namely, that it necessarily collapses into solipsism-of-the-moment.
56 Neurath 1932d:96/7.
How then did Neurath argue against the servicability of Carnap's phenomenalist language with a physicalist interpretation? By disputing first person authority on meaning! What was important for him was the thought that only publically accessible sense allowed for intersubjectivity which, in turn, allowed language use to be correctible if it became inconstant. Thus the condition of constancy of language use required that all human language be essentially intersubjective, that is, that sense be public. Neurath granted the radical scepticism of knowledge of meaning given a Cartesian base and Cartesian expectations. He recommended to take socialised cognition as a base and knowledge as a social product as the measure of the adequacy of reconstruction. Neurath's was an anti-sceptical response, not a sceptical solution.

Let it be admitted that the precise nature of this anti-individualist response is still unclear. Already though it is clear that his conception of socialised cognition and knowledge as social product requires a new apportionment of options in terms of contemporary epistemological taxonomy. "Doxastic external naturalism" is at best the name of a problem. More fruitful is the idea, suggested by his theory of procol sentences, that Neurath's epistemological theory sought a multi-dimensional analysis of justification. The sets of conditions indicated by the embedding sentences of Neurath's protocol sentences seek to define justification conditions for test sentences of empirical science in relational physical terms, in intentional terms, and in terms of social practices. Like Carnap, Neurath sought to account for the possibility of objectivity, but, unlike Carnap, Neurath rejected transcendental frameworks and sought to locate it in the historical conditions of cognitive life.
11.4.5 On the Relevance of Arguments from Scientific Practice:
Naturalism vs. Rational Reconstruction

Neurath's across-the-board attack on first person authority—as regards meaning statements as well as observation statements—means that Neurath "gave in" to the sceptic to a degree: he did not try to salvage first person authority. His private language argument granted the sceptical challenge to the Cartesian subject. But Neurath did not concede the case to the sceptic. Neurath instead sought to give knowledge claims a communal grounding. The question arises what allowed Carnap to resist Neurath's anti-individualist challenge. Did Carnap not think the first person authority challenged by the demand to exhibit the conditions of constancy of language use? As I showed, Carnap worried only about the possibility of objectivity. For him, then, it was perfectly fine to theorise about logically possible but unrealisable languages. Just that which Carnap did not accept of Neurath's arguments, however, I take to have been Neurath's central point: the incoherence of the practical use of such a phenomenalistic language forced theorists of science to adopt the physicalistic language.

I do not believe that Carnap did not accept Neurath's private language argument only because it was cryptically expressed. As some late comments of his indicate, Carnap still held to the possibility of providing a justification for empirical knowledge by means of a phenomenalist reconstruction—long after Wittgenstein's later detailed private language argument had been published and widely discussed. What may have been Carnap's arguments in support of his dismissal of the conclusions drawn from Neurath's and Wittgenstein's private language arguments? I believe it was the following. Carnap correctly noted that all such private language arguments which turned on considerations of what's required for the constancy of language use, take their start from the fact that any

actually used language must be intersubjectively intelligible by its users. (Neurath did not, after all, attack Carnap's intertranslatability thesis directly: what he did was to attack the idea that such a phenomenal language could do the job required of it in practice for it remained ensnared in the trap of solipsism-of-the-moment.) Clearly, Carnap was not moved by arguments that turned on the merely practical impossibility of the phenomenal language.\footnote{In his later disputes with Quine, this attitude of Carnap's is very clear. After Quine had shown the reduction of physical object talk in Carnap's Aufbau-reconstruction to be technically flawed, Carnap did not retract his view that it was possible and to the point to use such a language in the course of providing a rational reconstruction of empirical knowledge: instead he regarded his constructional steps there as in principle defensible as "non-eliminative" definitions (Carnap 1963a:19). And when Quine doubted the point of the analytic-synthetic distinction by pointing, once again, to the revisability in principle of every sentence held in science Carnap declared accounts of language change were outside his domain, the strict logic of science (Carnap 1963b:921), withon which the analytic-synthetic distinction was employed.}

It is important here to recall Carnap's conception of epistemology. Carnap's conception of epistemology was that of the logic of science, the "theory of knowledge considered as applied logic"\footnote{Carnap 1932b:33.}: "The sentences of the logic of science are sentences of the logical syntax of language. [...] logical syntax is nothing but the mathematics of language."\footnote{Carnap 1934d:7; see also 1936d:145.} The statements of the logic of science have the same status as statements of mathematics, they are analytic. This is a consequence of Carnap's view of rational justifcication. It follows that the logic of science was in an important sense an \textit{a priori} discipline: empirical enquiries would not enlighten it. Carnap's deflection of Neurath's anti-individualist argument, and his implied criticism of it, can only be understood against the background of his aprioricism. Note that against Neurath's insistence that he had an argument against phenomenalist reconstructions Carnap could thus hold that such reasons proceeded from an empirical basis--namely, practical considerations--and that that constituted an illegitimate move in epistemology. The intrusion of empirical considerations was illegitimate for it violated Carnap's principle of tolerance in the choice of
representational systems. It also violated the very distinction which underlay Carnap's conception of reconstructionist epistemology, namely, that between the contexts of discovery and justification.

Just this distinction of the contexts, however, was not recognised in this form by Neurath. Neurath accepted the distinction of the contexts of discovery and justification\(^{61}\), but for him this did not "save" analytic (or aprioricist) epistemology. What remained of epistemology was, apart from applied logic in the service of the anti-metaphysical crusade, part of the empirical sciences.\(^{62}\) Neurath was concerned with the actual "business of science" and he viewed the efforts of the Vienna Circle in this light - rightly or wrongly.\(^{63}\) It is significant then that in arguing for the revisability of protocol sentences Neurath did not only apply his private language argument, but that he also referred to the history of science.\(^{64}\) That was, after all, Neurath's aim: to do justice to the practice of science in order to gain from such considerations hints for the best pursuit of science henceforth.

What might be called Neurath's "pragmatism"--an affinity stressed retrospectively by Frank for all of the members of the "first" Vienna Circle--thus emerges as a major determinant of his anti-Carnapian argument. But Neurath's pragmatism also is fruitfully seen as a major determinant of his own epistemological theory, as I show next.

11.5 Neurath's Meta-Epistemology

For Neurath science was an instrument in the service of the rational mastery of the environment, not a magnifying glass for insight into reality. It was this ultimately practical orientation which alone was able--in the absence of epistemological foundations--to provide

\(^{61}\) Neurath 1932a:60.
\(^{62}\) Ibid:61.
\(^{63}\) Neurath 1930f:74; see also his 1931c:G522.
\(^{64}\) Neurath 1932d:94, 1936d:714.
the measure by which proposals for the conduct of science could be evaluated. This practical orientation required Neurath to insist that scientific theories be investigated as they presented themselves in practice, not in abstraction.

Neurath disagreed with Carnap on the merits of the project of providing a complete structuralisation of knowledge in Carnap's sense. Carnap's project of rational reconstruction was conceived as determining the content of human experience in purely structural, formal terms. Between the Aufbau and his Logical Syntax, Carnap had only given up his belief that there was one such structure which held of "normal humans" of necessity and thus moved from the structuralisation of the "given" to that of conventionally chosen languages. Since his own reconstruction did not pertain to actual cognition, Carnap did not deem relevant or objectionable a violation of the demand that accounts of knowledge production respect the actual processes by which knowledge was acquired. Thus Carnap could agree with Neurath that "every language as such is intersubjective" only in part, namely with the understanding—not shared by Neurath—that only the extensions of its expressions had to be intersubjectively accessible. Carnap and Neurath were operating with different notions of 'form'. Neurath held, by contrast with Carnap, that the meaningful structures which carried the sense of expressions themselves had to be intersubjectively discernible. It was structures in use that made for meaning and, ultimately, knowledge. Carnap's conception of form was suspect for Neurath on methodological grounds. Carnap lost the intersubjective control of form: even a physically interpretable phenomenal protocol language was not intersubjectively available, if the sense of its expressions remained private.

Neurath did not, of course, try to show that all languages had to be intersubjective. How then did he handle Carnap's objection that he, Carnap, dealt in logically possible

65Carnap 1961:ix/x.
languages and so could afford the counterfactual assumption of first person authority? Neurath's argument that the constancy of language use required that it be intersubjectively controllable apparently assumed as its starting point that a linguistic fact like constancy of meaning must be like any other fact intersubjectively accessible: first person reports are not to be blindly trusted. So reconstructed, Neurath made the assumption that all intelligible meaning must be naturalistically determined. Did Neurath's anti-individualism, his attack on first person authority on meaning, then presuppose an a priori truth of his own? No. Intersubjective control of assertions was for Neurath the very essence of scientific method; on it his conception of meaningfulness was built. Despite its centrality, however—so I suggest Neurath's unspoken reasoning went—it was not to be established a priori (and so supported from beyond science)—but instead to be taken as an empirical claim like others.\(^66\) To be sure, it was an extremely central datum. It constituted, to use Neurath's metaphor, the central beam of the boat to be reconstructed at sea. But it remained all the while an empirical datum. Neurath's private language argument did not rule against the logical possibility of phenomenalistical solipsistic languages. What Neurath argued against was the possibility that such a language was actually used. Language for us humans was, as far as one could make out the matter scientifically, essentially intersubjective. Neurath's private language argument established, if you like, the impossibility that a solipsistic language could be used by beings like us.

What this means for Neurath's private language argument is this: it gains its anti-foundationalist force only within Neurath's anti-apriorist conception of epistemology. Within his own scheme of things Neurath could not possibly be pressed further for an answer why considerations of what's required for the practice of science cannot be

\(^{66}\)In this respect then Neurath agreed with Carnap's and Schlick's conclusion that the thesis of physicalism was an empirical thesis, though not with their further reasoning that being a contingent fact philosophers and epistemologists could disregard its consequences. (Carnap 1932b:445, Schlick 1935:276/7.)
disregarded. Yet within Carnap's scheme, Neurath continued to owe a defense of the
primacy of practice. For Carnap, Neurath's deference to the practice of language use was a
moot point: he did not see his alternative neo-Kantian position endangered thereby. What
we find, then, as the basic disagreement between Carnap and Neurath, is a difference in
their conception of what the non-metaphysical theory of knowledge aimed for and thus
what it could consist in. (We already saw that Carnap and Neurath aimed for different
things: Carnap for an explication of the possibility of objective knowledge in principle,
Neurath for an explanation of the actuality of scientific knowledge.67) For Carnap,
scientific philosophy became the a priori logic of science. For Neurath, "scientific
philosophy" was really a misnomer: Philosophical theories of knowledge were to be
replaced by an integrated meta-theory comprising psychology, sociology, and descriptive
linguistics, with the logic of science as a preliminary defense and rear guard against
metaphysics. From the beginning of the debate onwards, Neurath had argued that the
theory of knowledge had to be "empiricised", that epistemology had to be naturalised.

If Neurath's private language argument is granted, Carnap's conception collapses.
To grant it, however, we need to grant his pragmatic orientation, which Carnap did not.
Carnap apparently saw no conflict between his and Neurath's different pursuits of theories
of knowledge; there was his a priori rational reconstruction and there was Neurath's
investigation of scientific practice. Neurath, however, did not recognise this compatibility:
a phenomenalistic reduction was unrealisable, a practical impossibility, and that was the
end of it. For him, Carnap's conception missed the mark altogether. The questions which
here arise are, first, whether Neurath was correct in seeing this conflict between his and
Carnap's conception of epistemology and, second, whether there are good reasons to
prefer Neurath's naturalism over Carnap's alternative neo-Kantianism, particularly,

67 See above sect. 8.4.
whether Neurath's reasons for doing so were good ones. The ideological question to be put to Neurath then is: on what ground did he decree that scientific meta-theory had to be as empirical as first-order theories? What limited the conceptual resources of meta-theory? Was meta-theory exempted from the strictures it placed on canonical first order theories? (Carnap's technical objections--why specify the rules for and on protocol sentences in the very same system in which they function as such?--are best seen in an ideological light, namely as different answers to the question: was meta-theory exempted from the strictures it placed on canonical first order theories?) Here we are pointed in the direction of their different responses to what, in chapter 9, I called the "frame question" of epistemology: May the language of epistemology presuppose knowledge which science cannot explain? The satisfaction of the conditions of justification bespoken by Neurath's protocol sentences can be established by empirical science. By contrast, establishing the satisfaction of the conditions of justification bespoken by Carnap's private protocol language required the illusory support of philosophical fictions, private intentional givens. Neurath's pragmatism placed constraints on the means of epistemological reconstruction, for only conceptions that can be scientifically supported will ultimately support the enlightenment quest of universal emancipation.68 What the analysis of Carnap's and Neurath's dispute then turns up as the underlying issue of what I called their "ideological" conflict is the conflict between a broadly realistic and a broadly pragmatic conception of epistemology. Taking any one of the candidate positions, the other is mistaken in its criticism or its expectations. Their underlying presumptions about the concept of knowledge were incommensurate.

11.6 Summary

My internalistic systematic analysis of the protocol sentence debate suggested that what was ultimately at issue was the question of the nature of knowledge and of the theory

68 Compare Neurath 1928a: last chapter.
of knowledge itself. (A crucial point in this conceptual web was occupied by each
disputants' conception of representation as relevant to epistemology.) My externalist
analysis of the Viennese epistemologies demonstrated the impossibility of accounting for
Neurath's proposal in terms of established taxonomies. In the present chapter, I attempted
to pursue further the hints Neurath himself gave. I sought to indicate how the logic of
Neurath's documented arguments leads, even forces, Neurath's hand in response to his
Viennese critics. I suggested how Neurath sought to flesh out his promise for an
alternative to both Carnap's and Schlick's conceptions, and how his conflict with Carnap
could only arise from large-scale differences of philosophical orientation.

Does my sketch of Neurath's conception of form--his theory of protocol sentences-
throw any light on the conception of frame over which Neurath's meta-epistemology came
to disagree with Carnap's? It is clear how they conflicted on a simple practical point.
Carnap's "original" protocols could not be used to reconstruct the social processes leading
to a rational consensus in science in anything like the detail which Neurath's protocol
sentences encouraged (if they could be so used at all). Given that Carnap and Neurath
placed different adequacy conditions on their epistemological analyses, however, this does
not decide their meta-epistemological dispute. For Carnap could always claim that the basic
elements of analysis--single concepts, not whole statements--were naturalisable after all.
The question for Neurath's meta-epistemology thus is: Can Neurath supply a pre-Quinean
argument with Quinean conclusions on this point, as he could on the point of the
epistemologically and ontologically phenomenalist reduction? (Neurath developed his
private language argument as it were in place of Quine's demonstration of the
incompleteness of Carnap's reduction in the Aufbau.)

Since Neurath abjured the rejection of the logical possibility of private languages,
the force of his private language argument does not reach to decide his meta-
epistemological argument with Carnap about the naturalising of intensions. But even if Neurath had come up with an argument like Quine’s for the indeterminacy of translation\textsuperscript{69}, Carnap would not have been moved, of course, for he was not moved by Quine either.\textsuperscript{70} Neurath’s conflict with Carnap arose from large-scale differences of philosophical orientation: naturalised epistemology vs. alternative or analytic neo-Kantianism. It seems that there was not suffi ent theoretically non-prejudiced “ground” shared between Carnap’s analytical neo-Kantianism and Neurath’s naturalism to be able to decide between them objectively from a neutral perspective. Carnap took Neurath to be engaged in applying descriptive meta-logic to sociology of science which was compatible with pursuit of the pure logic of science as philosophical epistemology. For Carnap, philosophy did not disappear but became the logic of science. For Neurath, philosophy did disappear and anti-philosophy recognised only formal and empirical science and their meta-theories in unified science. The issue between them was the status and nature of meta-theory. (Which is but another way of rephrasing the frame problem.) Utility—and thus adoption of one end or another for meta-theory—may, in the end, be the only criterion to decide.

Neurath’s theory of protocol sentences instantiated the demands of his meta-epistemology but did not defend it. I turn to discuss Neurath’s distinctive insistence on the practical relevance of epistemology in the next chapter.

\textsuperscript{69}There are, of course, intimations of thoughts familiar from the indeterminacy argument (Neurath 1945/6:233/4, 237) but I cannot explore them here.

\textsuperscript{70}Compare Ricketts’s 1982 analysis of the stand-off between Carnap and Quine and Quine and Chomsky.
Appendix to Chapter 11: On the Origin of Neurath's Private Language Argument

My reading of Neurath's private language argument may arouse suspicion for two reasons. The question arises whether Neurath's private language argument should be viewed as a version of Wittgenstein's later argument about rule-following. If it is, then the next question would be whether my interpretation is really tenable, whether it is not anachronistic. I answer these suspicions first by considering the historico-ideal context within which Neurath's protocol language must be set.

Neurath's argument, like Wittgenstein's, clearly turned on the need to secure the constancy of language use. To make the point that such constancy is only guaranteed by a language whose sense is public Neurath did not, however, appeal, like Wittgenstein, to an explicit argument about rule-following: instead, he merely pointed to the reliance of individual speakers and epistemic agents on "outside" criteria of the constancy of their language and cognition. To make this point all Neurath seems to have done was to extend his attack on the incorrigibility of first person authority in the making of observation statements to an attack on the incorrigibility of first person authority in the making of statements about what one's statements mean. Neurath attacked the idea of a Cartesian thinker by rejecting it across the board. But how did he do so? A broadly similar strategy to that of the rule-following argument does lurk in the implied background of Neurath's private language argument. Central to Neurath's argument was the notion of correctability which afforded constancy of use. Neurath's private language argument thus focussed on a thought very similar to Wittgenstein's: thinking that one is following a rule does not constitute following a rule.71

Is my reading of Neurath's private language argument thus anachronistic? It is not. First, the material assembled in chapter 6 and summarised in the previous section clearly demands it. Second, later texts, particularly Neurath's 1941 reprise, clearly support it, as I shall show below. Third, the starting point of his private language argument had a matter of concern in Vienna Circle philosophy. Fourth, Neurath's private language argument had yet earlier historical precursors.

To see this third point, recall that Schlick's depreciation of intuitive, ostensibly defined concepts as unable to support knowledge claims against the sceptic had already focussed on something very similar to what was at issue here, the instability of the meaning of concepts defined with reference to private contents. Just at this point, of course, Schlick and Carnap went "structural" in their conception of what meaning consisted in. During stage one of their debate Neurath argued that Carnap's strategy of the privatisation of this structure falsely promised success. The constancy of language use was simply "written into" the phenomenal language reconstructionally attributed to epistemic subjects. Neurath argued that the constancy of language use had to be intersubjectively demonstrable, it could not simply decreed. The point of his private language argument then was just that--that if a language was intersubjective (as it had to be to allow for constancy) then it was already a physicalist language: none other was practically possible. Despite its broad similarity to Wittgenstein's then, my interpretation of Neurath's private language argument is not, I would urge, anachronistic: it emerged quite naturally out of the problems and problem solutions Neurath and his colleagues were dealing with in the Vienna Circle. (It must also not be forgotten that Carnap too had a private language argument, though a weaker one, still prior to Wittgenstein.)

72Schlick 1918/25; see sect. 4.1 above.
What is more, Neurath was not the first to use a broadly private language-ish argument. Frege's "Thought" may be taken to contain one, as can be Dewey's "Experience and Nature". My point here is that Neurath was familiar with two related private language-ish arguments in the yet earlier philosophical literature and respected their authors highly, if not uncritically. The first of these historical influences was his old mentor Ferdinand Toennies. Neurath's private language argument cohered fully with Toennies' argument in his "Philosophical Terminology" that it was only on the basis of a prior understanding of the public natural language that any private (cognitive) meanings can be fixed by convention: all "private" representation systems are parasitic on the public ones with which humans grow into language and intentionality. This was central point of the semiotics Toennies developed: the social ground of "custom" as the natural starting point of value and meaning. It may well have been in the back of his mind when Neurath rejected the idea of a private language in 1931, as he first did in 1913. I believe Toennies' point was clearly in mind in 1913, when he specifically rejected the idea of a conventional determination of the form of the scientific language as unmediated by the language of everyday--and arrived at his first Boat!

Crucial to his private language argument of 1931 was of course his recognition of the primacy of the physicalistic language. Neurath perhaps not yet gained this recognition in 1913, nor clearly so when he embraced materialism in 1917 or when he remployed the Boat metaphor in his Anti-Spengler, but provoked in the mid-1920's by Carnap's "quasi-analytic" rerun of Mach's phenomenalist program and brought home by Neider's

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73Carles Travis suggested the Frege connection with "the" private private language argument in conversation. Quine points to Dewey in his 1969c:27.
74Toennies 1899/1900:305-15. This is the Welby Prize essay, translated into German in 1906, which Neurath referred to retrospectively as of great importance for his development in his 1941a:217.
75Neurath 1913a:357.
76Neurath 1917:106
77Neurath 1921:199.
argument. Still, this Toennies connection suggests that for Neurath the recognition of the essentially social character of language and cognition preceded his recognition of the "physicalistic" nature of this common language.

Toennies, in any case, was not the only precursor of Neurath's private language argument. Yet older—not entirely unrelated—thinkers played an even more significant role here for the way in which the process of growing into language and the essentially intersubjective nature of language was fused with materialism and naturalism. These precursors were Marx and Engels. Note, first, that Neurath did not come across their anti-private-language for 20 years after he learned Toennies's views, and, second, that he could not have come across it much earlier if he had tried: though written in 1846, the pertinent first part of Marx' and Engels' *The German Ideology* was first published only in 1926.79

Note, thirdly, the relevant passage from *The German Ideology*. In two of his publications of 1931, Neurath quoted from it, once briefly and once extensively.

[Only now, after we have considered the primary historical relationships, do we find that man also possesses "consciousness"; but, even so, not inherent, not "pure" consciousness.] From the start the 'spirit' is afflicted with the curse of being 'burdened' with matter, which here makes its appearance in the form of agitated layers of air, sounds, in short, of language. Language is as old as consciousness, language is practical consciousness that exists also for other men and for that reason alone it really exists for me personally as well; language, like consciousness, only arises from the need, the necessity of intercourse with other men.80

78Toennies 1887 carried for its first edition the subtitle "Treatise on Communism and Socialism as Empirical Forms of Culture". This indicates much of Toennies' topic, the discussion of the Marxian theme of the constitution of the individual under capitalism.

79For the history of this rediscovery see Riazanov 1925, Meyer 1926, Riazanov 1926, Bahnke 1962.

80Marx & Engels 1845/6 as quoted in Neurath 1931c:350/1, my emphasis. Neurath 1931d:411 quotes the sentences outside of the square brackets. (The passage is in Tucker 1978:157/8.) Note that the long quote ("Blutenlese") in Neurath 1931c represents Neurath's arrangement of phrases taken from the first publication of Part 1 of the German Ideology by Riazanov. However barbaric this may appear, note also that editors of the German Ideology do not agree on the correct arrangement of the passages of Part 1 of the manuscript: Riazanov's version in *Marx-Engels Archiv* of 1926 differs from Adoratski's version of 1932 (MEGA I, 5), and both differ again from the version in *Marx-Engels Werke* 3 of 1958. Yet different versions are on offer in different English translations.
It would go too far to settle Marx and Engels here with a private language argument. The impossibility of an essentially private language is clearly implied here, however. (Marx lead into this passage having considered four "moments, four aspects of the primary historical relationships". For the benefit of Germans "devoid of premises", Marx declared "the first premise of all human existence, and, therefore, of all history" to be the need of life to reproduce its condition, and the second to be that the self-reproduction of needs leads to the first "historical act", the creation of new needs. Further, Marx declared that the reproduction of life determined the natural unit of its kind, the family in various forms, and that "modes of production" and of "co-operation" and the "multitude of productive forces determin[ed] the nature of society". 81) That Neurath cited the emphasised passage twice in 1931, the very year in which I place his physicalist charge against Carnap with the help of his private language argument--both times in the course of his advocacy of "sociology on a materialistic basis" 82--strongly suggests that Marx' and Engels' point was not lost on him. Correspondingly, it strengthens my reading of his anti-Carnapian argument.

Now, both Toennies' and Marx' objections to essentially private languages take their start from considerations of human practice, not, that is, from considerations about language in the abstract. The growing into language is of course but another aspect of finding oneself at sea, with the intersubjective accessibility of sense the main beam of one's vessel. The, for Neurath, "anti-philosophical"--we'd say, naturalistic--force of this approach to questions of knowledge was also clearly noted in the passage Neurath twice quoted from the *German Ideology*.

[Where speculation ends--in real life--there real, positive science begins].
Empty talk about consciousness ceases. When reality is depicted,

82 Neurath 1931c:353; 1931d:411 makes the point that "materialism" and "physicalism" share their denotation but may, depending on one's company, differ in their connotation.
philosophy as an independent branch of knowledge loses its medium of existence.\textsuperscript{83}

That "[b]esides of science there are no meaningful sentences expressing philosophical systems"\textsuperscript{84} was by then, of course, a well established plaint of Neurath's and of the Vienna Circle in general. Thus Neurath noted Marx' and Engels' anticipation of "modern turns of phrase which often virtually lead up to behaviourism".\textsuperscript{85} (In Neurath's understanding, of course, behaviourism--both as "individual" and "socio-behaviorism" and both understood non-eliminatively vis-a-vis intentional states--was one of the successor disciplines of the theory of knowledge of old.\textsuperscript{86}) Neurath's quote from Marx, then, indicates a well-considered agreement with Marx on his, Neurath's part.\textsuperscript{87}

I submit that recognising Neurath's private language argument is by no means reading Neurath anachronistically. My reasons over and above the fact that Neurath's own texts bear out this interpretation well, are (1), Neurath's familiarity with the general anti-intuitionist thrust of Vienna Circle philosophy, an aspect of its linguistic turn which had exercised Schlick from early on and led him to take more radical anti-sceptical path than Russell, for example, and had considerable influence on Carnap.\textsuperscript{88} (2) Neurath's familiarity with the thought of Toennies and (3) his familiarity with Marx' and Engels' German Ideology and either party's ruminations on the matter of the social nature of cognition. The philosophical conclusion Neurath came to, namely, to develop his anti-

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\textsuperscript{83}Marx & Engels 1846 quoted in Neurath 1931c:350, my italics. Neurath 1931d quotes the sentences outside the square brackets here.

\textsuperscript{84}Ibid.:407.

\textsuperscript{85}Neurath 1931d:411.

\textsuperscript{86}See above, sect. 6.4.2 and 7.3.3.

\textsuperscript{87}Neurath once scandalised the readers of Der Kampf when he noted "what is correct does not become more correct by having also been taught by Marx and does not become false because Marx is of a different opinion." (1923:288.)

\textsuperscript{88}See above sects. 2.2.3 and 3.3.2.
phenomenalistic argument as a private language argument against Carnap, was by no means outlandish for his time and place.89
Chapter 12

CONCLUSION:

OUTLINE OF NEURATH'S NATURALISED EPISTEMOLOGY

Abstract: Suggested strategy for further research (12.1). Outline of Neurath's naturalistic epistemology (12.2). Neurath's epistemological project viewed in the context of his scientific activity as a whole, and, like it, recognised in its practical, emancipatory intent (12.3).

The protocol sentence debate was not only rich in terms of the questions pursued by the disputants, but also in terms of questions still of interest today. Earlier, I showed that the questions we could press each of the disputants further on point to issues still of contemporary concern. Schlick's Wittgensteinian path was cut short and so can only point the direction; Carnap's unhindered development led to his debate with Quine and thus expressly laid the ground for the ongoing debate about the indeterminacy of translation and its consequences. Neurath's idea of an anti-foundationalist naturalistic epistemology as expressed in his Boat pointed the way for Quine, yet his own proposals remained so far obscure to posterity. Where, in terms of finding the theme of Neurath's reading of the Boat, have we thus ended up?

My reconstruction of the protocol sentence debate did not only show Neurath to be arguing against Carnap at a much earlier date than is commonly thought1, it also revealed his mature argument against Carnap to be a private language argument2, and further showed that all along Neurath's intent was to render the theory of knowledge empirical, in a word, to naturalise it. My internal analysis of the debate confirmed suspicions of deep

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1See ch. 5.
2See ch. 7.
philosophical splits in the Vienna Circle, indeed within the physicalist camp. My external analysis of the debate showed Neurath to anticipate contemporary views over and above "mere" naturalism. My analysis of the Viennese Neurath reception showed, first, Neurath's project to be far more coherent and articulate than his critics supposed—in particular, it pointed to his proposal for the form of protocol sentences to contain valuable hints how a non-individualistic but intentional theory of justification might go (where justification is already naturalised) — and it showed, second, that at least the seeming intractability of the Carnap-Quine debate was anticipated in the Neurath-Carnap debate. None of which, of course, is to say that Neurath's view was without problems, nor that it has finally been revealed in its totality—far from it.

In conclusion, I now draw together some of the salient elements of my analysis. first, I briefly indicate how a further inquiry might explore Neurath's conceptions of 'form' and 'frame' more fully. Then I try to specify the distinctive features of Neurath's so far unknown variant of naturalistic epistemology in outline and press somewhat further on Neurath's meta-epistemological conceptions. Finally I shall place Neurath's philosophical or "anti-philosophical" project, to speak Neurathese, project in its specifically Neurathian context, and thus return to the question of the interpretation of Neurath's Boat with which this inquiry began.

12.1 A Two-Pronged Strategy for Further Research

The critical questions put to Neurath by his colleagues were these. First, Neurath must explain how to theorise about meaning and yet decline all talk of correspondence? Second, Neurath must explain how he could define (unlike Carnap) certain statements as observation statements without appeal to talk of truth. Third, Neurath must explain the

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3See ch. 9.
4See ch. 10.
5See ch. 11.
constraints on the deletion of protocol sentences. Fourth, Neurath must explain the
incompatibility of Carnap's and his proposals. These questions and answers, as we saw,
fall into two groups. First, there is Neurath's answer to the mechanical questions raised by
Schlick's, Popper's and Hempel's reception: this is his fully explicated theory of protocol
sentences. It will provide for much needed clarification of the workings of his "formalist"
conception. Second, there is the ideological conflict of his and Carnap's conception: how
can representation be legitimately theorised about? Neurath's answer here is his response
to what I called the frame question of epistemology, that is, his meta-epistemology.

Both the mechanical and the ideological questions put to Neurath converged on this:
what was Neurath's conception of 'form' and 'frame' such that it supports empiricism and
denies Cartesian individualism? Presenting Neurath's full answer would require another
return to the historical records for the consideration of Neurath's stage four contributions.
(The reader will have noted that my review above did not yet cover the "news sent home"
from the philosophical wilderness in which the exiled Neurath found himself after the Paris
Congress.)

To see what Neurath's conception of 'form' was, first clarify Neurath's conception
of the structure of scientific theories (his encyclopedic turn) and his stance on the
observation-theory distinction. This will lead to a first indication of his pronouncedly
instrumentalist stance towards all parts of scientific theories and his "pluralist" attitude
towards scientific explanations. Then turn to pursue the consequences of his abandonment
of "philosophy" for epistemological inquiries and clarify his views on truth, and his
proposal for the form of protocol sentences. Finally, consider to the views on language
and mind which his multi-dimensional theory of protocol sentences presupposes and so
gain a perspective on the relation of the distinct naturalisms of Neurath and Quine.
Proceeding along this line of inquiry would spell out further just how Neurath fits squarely in the domain of contemporary discussion. 6

Now to Neurath's conception of the 'frame' of epistemological analysis. As in the case of the mechanical questions to Neurath, I can in the case of the ideological question only claim to have established some broad pointers here. First, Neurath did not simply confound the Carnapian distinction of the contexts of discovery and justification. Neurath challenged Carnap's ideas about "rational reconstruction" as a strategy of justification and Carnap's understanding of the distinction of the contexts. Neurath did make the distinction but rejected Carnap's interpretation of it because he rejected Carnap's program. Against Carnap's alternative neo-Kantianism, Neurath set the project of naturalistic epistemology, and thus a different concept of justification. Second, note that Carnap and Neurath saw the difference between them differently. Carnap saw the difference to lie mainly in the object of theoretical labour, that is, whether one was concerned with establishing the possibility of knowledge or with comprehending the practice of science. Thus both projects seemed compatible. Neurath, by contrast, focussed on the means of theoretical labour themselves: what can a theorist of knowledge legitimately assume as given in the process of knowledge acquisition? As we saw, Neurath held that constancy of language use could not be presupposed as an uncontroversial given. Carnap's project was incompatible with this conception.

The answer to the question of why to prefer Neurath's conception over Carnap's lies not only in that in Neurath's view Carnap made an untenable assumption. Unless Neurath were to turn at this juncture into a table-thumping realist, how could he respond to Carnap's relativism of justificatory schemes? I believe that Neurath's true answer must be sought in the utility of the different conceptions of, and theoretical approaches to,

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6See above sect. 10.5.3.
representation held by Neurath and Carnap. Here the leading question becomes this. What was Neurath's guiding interest in the questions which the Vienna Circle pursued? I suggested in my introduction, that it was to develop a conception of knowledge that could serve as an instrument of emancipation. Neurath's espousal of a naturalistic epistemology over Carnap's rational reconstructionism was motivated and defended by instrumental considerations. Only what we can do with given candidate epistemologies (and their implied conceptions of knowledge itself) could provide for an argument here? (At this advanced point of the inquiry then, it would also become instructive to join the Neurath of the protocol sentence debate and after with the early pre-Vienna Circle Neurath. With a better picture of what his long-term aims and proposals were one then might finally proceed to the evaluation of his overall project of providing a conception of knowledge as an instrument of emancipation.)

12.2 Outline of Neurath's Naturalistic Epistemology

Even before engaging in any of these further inquiries, however, I can now draw a sketch which integrates the various aspects of Neurath's proposal for the naturalisation of epistemology, which we were able to pinpoint in the course of our review and analysis of the protocol sentence debate, and indicate its connection with aspects of his thought which the present dissertation has not focussed on. Combining (i) the historical facts of the debate (chs. 2-8), (ii) the thematic questions arising out of their historical setting (ch. 9), (iii) the thematic questions arising out of attempt to map Viennese views into contemporary taxonomies (ch. 10), and (iv) the puzzles behind the the vicissitudes of the popular response to Neurath's theory in Vienna and since (ch. 11), with the leading intuition of a "rich" reading of Neurath's Boat (ch. 1), this is what we get.

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7Here it would be useful to explore the thought suggested in the Austrian school that Neurath sought a radical reorientation of the anti-sceptical response. (Nemeth 1981, Haller 1982c, Rutte 1982b.)
12.2.1 The Core Conception

First, the guiding thought of Neurath's epistemology—and the cognitive complement to his Boat and his Judge metaphor—is Neurath's declaration that *the possibility of science should become apparent in itself*. If materialism provided the anchor for Neurath's naturalism, this motto provided the *dynamic principle* along which the theory of knowledge was to be constructed. Against Carnap, Neurath disqualified a priori reasoning from all but purely logical questions; the theory of knowledge, in particular, had to be empiricised. Carnap's logic of science could not claim the mantle of the queen of the sciences: instead there obtained the shop-floor equality of different meta-theoretical inquiries like the psychology, sociology and history of science. Neurath resisted Carnap's alternative neo-Kantianism: a priori epistemology was finished, the logic of science could not on its own make for a theory of knowledge. This dynamic principle of Neurath's—let the possibility of science become apparent in science itself—is clearly the motor of his epistemological thought.

Second, the *anchor* for this naturalist dynamic is Neurath's materialism, his resolute *opposition to Carnap's methodological solipsism*. This trait found its crystallisation in his proposal for the content of protocol sentences: he gave them what I called a "relational" physicalistic reading. (This relational content specified both the stimulation state of an organism and its relation to its environment: Carnap never accepted it.) By itself, materialism did not make for naturalism, of course; yet it provided for the anchor of Neurath's naturalism (in concert with other theses). Neurath's physicalism is best viewed as the expression of what is nowadays called the "natural ontological attitude" and in his time was known as the "natural world conception".  

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8Fine 1984; Avenarius 1892.
Third, note what prompted the adoption of this anchor and dynamic principle, Neurath's acute awareness of the sceptical challenge at its most radical. For Neurath, the basic coherence of the individual epistemic subject was indeed put in question, but that did not concede the case to the sceptic. Neurath conceded what had to be conceded: the self-sufficient epistemic individual was finished. This, above everything else, suggested the demise of classical epistemology. With the radical challenge accepted naturalism remained the only defense against the sceptical conclusion. Neurath did not concede therewith that there was no knowledge about; rather, he set to providing a conception of knowledge as a communal project and thus as the product of social processes. Neurath's anti-individualism required the socialisation of cognition. As we saw from his private language argument, Neurath distinguished the socially mediated sense of expressions from the inevitably private and presumably neuro-physiological medium of mental representation. Whereas Carnap relied upon an original protocol language, Neurath accepted the physicalist language from the start; where Carnap relied upon private sense, there Neurath placed socialised cognition. From this diagnosis and remedial strategy important consequences follow.

Fourth, note the key to the conception of socialised cognition, Neurath's view of meaning as correlated information. For Neurath, epistemology became the empirical study of the acceptance conditions of scientific statements. These acceptance conditions on observation statements were expressed in the broadly formal aspects of Neurath's preferred protocol sentences. Neurath sought to develop a notion of intersubjectively intelligible and available 'form' such that claims to objective knowledge find their grounding in it. That which Schlick declared unrepresentable and that which Carnap sought to capture in ever more complex languages, that--namely, the form that made for meaning and objectivity—that Neurath required to be itself physicalistically-behaviourally explicated. (Here Neurath addressed the question what made representation possible: representationality could only consists in something like correlated information, that is, a correlation of structure and
context of use, not by a correspondence of structure between statement and fact.) The form of protocol sentences, so important for Neurath, encoded crucial information about their use in context. By so answering his critics, Neurath hoped to prevent the anarchy of scientific opinions, the demise of empiricism.

12.2.2 Meta-Epistemological Tangents

I already sketched above his multi-dimensional theory of protocol sentences to the extent to which it is possible to do so without a detailed investigation of his psot-1934 texts. On the matter of his meta-epistemology Neurath can here be pressed a little further yet, however.

The principal element here is obviously what could be called Neurath's "pan-physicalism". First we may note its rationale. Neurath required the physicalisation of meta-theoretical concepts for reasons of the controllability of meta-theoretical thought. (Examples are the crucial notions of form and observationality: neither of them could be explicated merely "philosophically".) Only when questions of knowledge production were pursued in themselves empirical terms can the explanations provided by formal inquiry claim attention of the practitioner of knowledge who believed in unified science. What made Neurath "anti-philosophical" was the constraint he put on the solution provided, namely that it defend empiricism: the concepts of philosophical reflection thus had to be cashable in empirical terms. For Neurath, this meant that philosophical reflection itself had to be comprehended within the scientific scheme of things--if it was science one was defending! With his pan-physicalism Neurath might at first appear to try to tame meta-theoretic concept formation and in fact to close off from discussion the very province and instruments of pure reason. To be sure, Neurath tried to ward off idealist academic philosophy; but his move here can also be read positively, namely, as trying to furnish a criterion of physicalist acceptability such that meta-theoretic concepts can be explicated.
Note then Neurath's attempt to comprehend philosophical reflection in the very same system by which scientific reflection orders experience. Against the immediate worry that a semantic or type-theoretic paradox would follow from Neurath's conception, it must be pointed out that Neurath did not require logical completeness of epistemological meta-theories. Neurath did not hanker after some idea of pre-Gödelian completeness. A matter which needs to further investigated is just what it was that, on pain of paradox, the language of science--here the language of epistemology or scientific meta-theory--could not express. It might just be that the "unspeakable" of the Tractatus casts its shadow even over Neurath's unified science.

Note that, in any case, Neurath's meta-epistemology must be strong enough to accommodate concepts of reflection and rationality, like that of the reasoned acceptance of theories. After all, science has to explain science--and Neurath's meta-epistemology would have to belong to it, since there was no more philosophy--so the activity of the theorist of knowledge must itself be comprehensible in the theory of knowledge. This means, of course, that, in some way or another, the reflecting subject was to be comprehended, qua reflecting subject, in Neurath's theory of knowledge. What is clear, therefore, is that Neurath did grapple with the way in which philosophical reflection was self-reflexive.

What may appear as mere tantrums of the "anti-philosopher" Neurath were in fact

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9This worry was raised by Koehler 1984. Koehler transposes to the logical realm Cohen's worry (1963:157n.128) that Neurath's curious turns of phrase which suggest a puzzling invocation of the idea of the closedness of a theoretical system ("geschlossenes System") harbours the danger of dogmatism. Contrary to Koehler, it must be noted that this talk of closedness and completeness appears in response to the claim that physicalism needs a philosophical foundation. An examination of stage-4 Neurath, his encyclopedic turn, would show that Neurath was not interested in a sense of completeness that would neutralise Goedel's result (for example, in Carnap's extended sense of 'formal' in his Logical Syntax). Neurath also did not seek deductive closure for the belief system by which scientific knowledge claims were to be justified (in fact, thought it an objectionable idealisation). Neurath was interested in when it was reasonable to acquiesce in a knowledge claim; he did not seek to provide justification for truth claims about purely formal matters of fact. Thus Neurath did not seek deductive closure but the conditions of reasoned trust in the natural ontological attitude. The completeness Neurath aimed for was the methodological self-sufficiency of empirical science. Neurath acquiesced in the incompleteness of justification which any anti-foundationalist would predict. Neurath fully accepted the inevitably patchiness of and local incoherences in the system of unified science as a comprehensive theory of what there is. (Compare also Rutte 1982b and Haller 1982c.)
denunciations of ways of meta-theoretical thought which exempted themselves from the controls which they themselves, qua meta-theorists, placed on first order theories, to escape, in other words, the responsibility of self-reflection. This line of thought opens the following perspective on Neurath's naturalism. Neurath's desire to provide epistemic norms was one of the reasons why he required that philosophical reflection be comprehendable in the same system in which philosophical reflexion comprehended experience. That the epistemic norms themselves be intersubjectively intelligible and evaluable--"controllably rational", as I call it below--was the point of his naturalism.

A crucial focus for Neurath's pan-physicalism was his attempted naturalisation of epistemic norms. Consider the question whether Neurath was condemned to remain descriptive in his epistemology. I argued above that an anti-foundationalist had better aim for the provision of objective epistemic norms, if he wished to escape the objection of cultural relativism. This challenge applies to Neurath, and Neurath recognised it. Neurath's protocol sentences were meant to uphold the objectivity requirement: they were to ensure--without a "philosophical" definition of observationality--that only those systems of statements were accepted by their help whose acceptance was indeed warranted. The requirement that evidence be evaluated for three sets of conditions sought to establish epistemic norms of science that recommended themselves for universal adoption, whose rationality possessed species-wide appeal.

Now wherein lay the ground of their species-wide appeal? Were these norms to be thought of as conducive to truth? Neurath would not have wanted to say that, even as as a deflationary theorist of truth. Yet what he, as a broad pragmatist, would have said was that their adoption would contribute to the process of enlightenment, of release from enslaving dogma. The question arises, of course, whether the conative ground on which the superior functionality of scientific reason was established can and must itself be universally shared and whether Neurath's account itself possesses prescriptive force. What if Neurath's
account said that universal argument about ends was not to be expected: would there be something left to say?\textsuperscript{10}

In sum. Neurath's naturalism sought to make scientific rationality self-evident. For Neurath, it was not in the material data of experience, nor in their abstract form that objectivity resided, but in the conditions of intersubjective agreement, the interactive cognitive processes involving these data and their forms.

\textbf{12.3 Neurath's New Enlightenment Naturalism: The Practical-Critical Dimension of "Controllable" Rationality}

My outline of Neurath's epistemology and its meta-epistemological tangents point the interpretation of Neurath's philosophy in the direction of what, in the introduction, I called the "rich" reading of the metaphor of Neurath's Boat, and thus points the Neurath interpretation beyond Quine. On this rich reading, "natural and social science explains (knowledge of) natural and social science", unlike as on what I called the "austere" Quinean reading where "natural science explains (knowledge of) natural science". It was this rich reading of the Boat which the later Neurath tried to articulate and defend, mostly against total incomprehension, in what might be called his "new enlightenment naturalism". To see what Neurath's new enlightenment naturalism amounted to, we have to view his philosophical efforts in the wider context of the scientific, pedagogical, and political work he pursued.

Neurath's interests and activities were bewilderingly wide.\textsuperscript{11} His wide theoretical interests found their common focus in the practical aim of his work. Unlike Carnap or Schlick, Otto Neurath had a strong background in the fledgling social science of his day\textsuperscript{12}

\textsuperscript{10}Compare Carnap's answer: 'no' in his 1934d.
\textsuperscript{12}Otto Neurath was the son of Wilhelm Neurath, utopian economist and noted early opponent of Social Darwinism, and found in Ferdinand Toennies an early scientific mentor during his university days. For
and favoured their use of formal models. In the first Vienna Circle from 1907 to 1912, the writings of Positivists like Mach and the so-called "French conventionalists" Poincaré and Duhem, impressed upon Neurath the importance of considering even highly formalised scientific theories, like those of physics, in their historical context. (Like Schlick, he was well versed in the history of the natural sciences, and like Carnap he advocated the use of formal models.) During World War I, Neurath's longstanding professional concern with the nature of economics under war conditions turned into open advocacy of planned economies, and in 1919 he found himself actively involved in revolutionary politics, if only as a "social engineer" administrating the socialisation of Bavaria. After his deportation from post-Soviet Munich to Austria, Neurath found himself barred from academia, but he continued his scientific work by foundering, over the years and in his various countries of residence, a number of institutes which were concerned with the development of workers' councils, the promotion of public housing programs, and the development of pictorial statistics and an international picture language, and—not to be forgotten—the International Encyclopedia of Unified Science.

Neurath's numerous writings are thus by no means exclusively about philosophical or methodological issues in the sciences; most, in fact, concern topics in economics, in the development of pictorial statistics, and broadly political questions of the day. Despite their

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13Neurath 1910, 1911.
14By 1914, Neurath explicitly noted the social determinants not only of theory change in the social sciences, obvious along, but also of theory change in the natural sciences. See Neurath 1915, 1916.
15This is how Neurath described the role he was to play within months in 1919a:138; in retrospect and during his subsequent trial he stressed his wish to conduct his work as an "unpolitical administrator" (Neurath 1920:21). For a narrative with reference to court documents etc. see Mohn 1978:66-78; for Neurath's own version of the rationale for his defense see Neider 1977.
16The terms of the deportation (arranged by Otto Bauer, one of the chief theoreticians of Austro-Marxism and the the first Austrian republic's foreign minister) which saved him from serving his full prison sentence for treason forbade him to enter Germany for six years and thus prevented him from taking up the appointment in Max Weber's department in Heidelberg where he received his habilitation in 1917.
17After seeking his first exile from Austria in Holland in 1934, Neurath fled the Germans, by boat no less, to England in 1940, where he died in 1945, reportedly preparing for his return to Vienna. For biographical matters, see the collections of memoirs of Neurath in Neurath 1973 and Stadler 1982a.
great variety, however, all of Neurath's publications are united by a common theme: the betterment of the human condition through the improvement of the social conditions for the broad masses of humanity. This concern of Neurath's is already discernible in his economic writings of the early 1910s, and it remained the clearly announced goal of his practical and theoretical activities from 1917 to the end of his life. Virtually all of Neurath's writings must accordingly be read as motivated by his social commitment.18

What emerges from this overview of Neurath's life and work is Neurath's distinctive perspective on the project of scientific philosophy. In line with Marx' admonition that philosophy so far had merely interpreted the world, whereas it should change it, Neurath sought to develop a conception of knowledge and pursue an epistemology that provided the tools for such a change. When Neurath fought the depreciation of scientific knowledge claims by proponents of "metaphysics"-philosophies incompatible with science--he did so because he saw such depreciation open the doors to wholesale irrationalism which threatened disastrous results, not only in the intellectual sphere of theory, but also in everyday economic, political, and social life.19 Neurath's emphasis in developing the scientific philosophy lay in the provision of a conception of what I shall call "controllable rationality".

By "controllable rationality" I mean to denote the guiding idea which underlies Neurath's philosophy. Neurath sought to import the Enlightenment idea of scientific knowledge as liberator from the realm of the natural sciences, the natural world, to that of the social sciences, the social world. What allowed science to serve as liberator from dogma was its empirical method, its reliance on intersubjectively accessible evidence. Whenever Neurath equated the growth and spread of scientific knowledge with the growth

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18See also, in addition to the papers listed in fn. 8 above, K. Fleck 1979, Nehmet 1981,1982a, 1982b, Dvorak 1982a, 1985, Wartofsky 1982, and other contributors to Stadler 1982a.

19Neurath 1921, a devastating critique of Spengler's The Decline of the West which reportedly even impressed Wittgenstein (Janik & Toulmin 1973:177) confronted this threat explicitly and without compromise.
and spread of enlightenment, he had this conception of controllable rationality in mind.
Neurath’s scientific philosophy was dedicated to its development, and so continued the line of the 19th century Viennese enlightenment thinkers like Mach, Popper-Lynkeus, and Wilhelm Neurath.\(^\text{20}\)

Now clearly, the idea of knowledge as an instrument of emancipation raised normative questions. Ultimately then, Neurath’s project required him to define the place of scientific thinking in reasoning about ends and the role of the end of scientific activity in determining what kind of justification for science was required.\(^\text{21}\) More immediately, however, Neurath’s aim required the reconceptualisation of scientific knowledge and its attendant epistemic norms. To be a tool for emancipation, scientific knowledge had to be properly understood. For Neurath, the sceptical challenge to the knowledge claim of science was a useful tool in this required investigation of the nature of scientific knowledge. Neurath came to two conclusions, a material one and a methodological one. His result was, first of all, that the foundationalist pretension of traditional defenders of knowledge had to be rejected. Secondly, Neurath concluded that the relation of theoretical to practical reason, and of scientific meta-theory to scientific practice, had to be reassessed. The proper understanding of scientific rationality required that its embedding in practical pursuits be taken to account of.

Neurath’s concern with the practice of science was motivated by the idea that the investigation of the practical conditions of intersubjective control of scientific assertions would provide for the required naturalisation of epistemic norms. For Neurath, the task of scientific philosophy was to conceive the objectivity of epistemic norms as resting in the conditions of intersubjective understanding. But epistemic justification so-constituted

\(^\text{20}\)Again, fellow first Vienna Circle pragmatist Frank 1917, 1938 bears out a shared appreciation of Mach (and Nietzsche) as enlightenment thinkers. For Neurath’s admiration of Popper-Lynkeus, see his 1919d. For more on Wilhelm Neurath see footnote 9 above.

cannot be isolated from the interests of the cognitive agents which shape the communicative situation. There was for Neurath nothing unduly partisan in the idea that an account of epistemic justification faced amongst others, the adequacy condition that it empowered agents for the mastery of their natural and social environment. Only once scientific rationality shed foundationalist pretensions and recognised its kinship with practical reasoning, then it could, Neurath believed, provide the enlightenment tool for universal empowerment.

All this suggests that for Neurath it was not a coincidental sideline of his "anti-philosophy" that his guiding conception of controllable rationality was "critical" also in its engaged Marxist stance. Consider first the anti-individualist conception of language and knowledge put forth by Neurath in his private language argument. Neurath's adoption of this argument not only coincided with his acquaintance with Marx' and Engels' German Ideology, but also coheres with Marx's central claim that the very idea--the very human idea--of individuality was a historically determined fact and that the individuality of persons was mediated through their social relations. In Marx' aperçu which Neurath quoted twice in 1931: "Language is practical consciousness."

Second, note the task for Neurath's socio-naturalistic epistemology set by his overall project of the provision of a conception of knowledge as an instrument of emancipation. Neurath's new enlightenment pathos prescribed a positive answer to the question of whether naturalistic epistemology should provide for robust enough objective epistemic norms to serve the purposes of a radical critique. (How could a conception of knowledge aid emancipation if reflection, its subject, and the epistemic norms it is guided by, are left out of the equation? Reflection in turn, had to be comprehended in its historical determination.) At the very least then, Neurath tried to provide what amounts to a self-

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22 See above appendix to ch. 11.
conscious alternative to the retreat into "philosophy" in what is known as the Western Marxism of Lukacs, Korsch and the Frankfurt School.

Thirdly, and finally, note Neurath's own concern and doubt that his conception of radical critique was fully adequate to the task. I submit that what Neurath wrote about Philipp Frank may well hold equally for Neurath himself:

as a member of the "Vienna Circle" he contributes to the struggle as it is led with the latest weaponry on the soil of science and philosophy against bourgeois ideology. He knows well enough that such a fight by argument does not have too great an effect, but that it constitutes the superstructural equivalent of the great revolutionary transformation of our social and economic order, which is already under way in several respects.24

Clearly, Neurath was no "wide eyed" and innocent utopian, at least as far as the efficacy of intellectual critiques for social change were concerned. Here Neurath prefigured the well known pessimism of latterday social critics from Adorno and Horkheimer to Chomsky about the efficacy of their own, different critical tools and efforts at enlightenment.

What then about the case envisaged above, where discussion comes to a halt for lack of agreement on the ends for which, amongst other things, science and its meta-theory could be pursued? Is the Frankfurt School criticism of the "scientistic" philosophy of the Vienna Circle correct, that, in a word, they did not recognise that instrumental reason cannot serve in the critique of instrumental reason?25 Neurath was fully apprised of the fact that the "iron cage" of modernity, to use Max Weber's phrase, was a creation of instrumental market rationality. Given the Boat and his Marxism, it would only be consistent for Neurath to deny that a neutral point outside of the conflict of human interest could be found. But this did not mean that there was nothing left to say in terms of a cognitively meaningful critique of capitalism. In its highest reaches—arguing for one or

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24Neurath 1932b:576.
another conception of science—Neurath’s meta-epistemology could not assume normative authority, but it could still claim descriptive adequacy for its account of the decision process rational agents undergo on these matters: one might not convince the other, but could at least explain the disagreement. It had, however, nothing to say to those Carnap called, perhaps following Neurath, "the enemies of social progress" who would not accept the proposed conditions on scientific rationality.

It is of great importance to recognise that to demonstrate the scientific canons of social criticism had been Neurath’s concern from the beginning of his scientific career. Thus Neurath sought to provide the conceptual tools for discerning and preferring competing conceptions of rationality in his meta-epistemology, and sought to think even of the norms of meta-epistemology in such a way that they stood the demand for scientific explication. Neurath’s result was that critical rationality had to be such that it, the rationality of the social critic, could recommend itself as a controllable scientific rationality. For all its groundedness in practical considerations, however, Neurath’s controllable rationality could not, on pain of losing its scientific character, provide for more than conditional norms.

Clearly, to present "the compleat Neurath", then, much more, in particular Neurath’s other Boats of 1913, 1919/21, 1937, and 1944 must be investigated. I hope the reader will agree that already the results presented in this dissertation make a case for the interest of the project for an "Encyclopedia of Unified Neurath". For not only can we see some of the concrete theoretical difficulties which Neurath’s proposals must surmount; we can also already see more clearly than before the direction into which Neurath’s proposals aimed. Neurath dark proposals for the naturalisation of epistemology holds interest still

26 I hope I can be excused here from even only beginning to explain Neurath’s understanding of historical materialism and how his pragmatism was itself comprehended in it.
27 Carnap 1963a:23; see above sect. 7.1.
today, for the problems he confronted confront us again today. The problems which this dissertation leaves unresolved for Neurath's project—externalism in the philosophy of mind, a multidimensional social theory of epistemic justification, and the naturalisation of epistemic norms—demand the formulation of a naturalistic epistemology that is non-austere. Neurath's naturalistic epistemology sought to include social science among its tools so that indeed natural and social science explain not only knowledge of, but also interest in natural and social science. In filling out the skeleton of the theory of protocol sentences and the epistemological meta-theory to which Neurath's naturalism is committed, Neurath, or his interpreter, must flesh out the rich reading of the metaphor of Neurath's Boat.

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28 Note that one of the NEH summer seminars for philosophy teachers this year (1989) has as its topic: Can naturalised epistemology be normative?

29 What makes such an epistemology still "naturalistic"? Its non-reductive "scientism"—the demand that epistemological terminology in turn be explicable in terms applicable to the explanation or description of processes antecedently understood to be natural. All now depends on what "comes naturally".
Chapter 13

BIBLIOGRAPHY

13.1 Bibliographical Remarks

The following remarks and listings are not meant to be exhaustive and definitive but merely to serve the purpose of very general orientation. It leaves off, furthermore, where my Introduction takes over: see the footnotes in sects. 2.3.1, 2.3.3, 2.3.4, 2.4.3, and 2.5.3 for the listings of current schools of Vienna Circle scholarship.

Background Accounts for Neurath and the Vienna Circle

In their different ways, Wang [1986] and what I call the "New Canon" (see sect.2.3.3) provide the large scale setting of Vienna Circle philosophy in analytic philosophy as it concerns the philosophy of formal and natural science. For overviews of the currents within 19th and early 20th century German academic philosophy--against much of which the Circle stood--see the excellent Schnaedelbach [1983], of Austrian cultural history of the same period see the somewhat speculative Johnston [1972], and of the early 20th century Vienna see Janik & Toulmin [1973]. With Neurath an additional concern enters the picture, the philosophy of social science. For exploring its dimensions I found the following general background accounts particularly useful. For supplementation pertinent to Neurath's milieu and perspective see, on contemporary Vienna culture, Nemeth [1981 :h.2]. On the tradition of Austrian scientific philosophy in general see Rutte [1977] and Haller [1977] and on the Viennese Mach tradition in particular see Stadler [1982b] and Haller and Stadler [1988]; on positivism in sociology see Giddens [1976] and on the beginnings of German sociology see Liebersohn [1988]; on Austro-Marxism finally see

**Historical Accounts of the Vienna Circle**

The successive stages of both the self-reflective understanding of the participants and the general reception of their outlook can be charted by distinguishing four different batches of such historical reviews within Logical Empiricism and Analytical philosophy. The earliest "historians" of the movement are contemporaneous with its Vienna phase and actively involved: their tone is that of a messianic and decidedly forward looking Logical Positivism: the phenomenon of knowledge had become transparent! (Neurath et al [1929], Feigl & Blumberg [1931], Neurath [1936a], [1936b], Schlick [1937]; with criticism: Kaila [1930], Petzaell [1931], [1935], Weinberg [1936].) The second wave of historians is located doctrinally midstream of its Americanisation as the Received View: their tone expresses the reduced expectations for the Viennese program as engendered by its liberalisation as Logical Empiricism (von Mises [1939], Frank [1941], [1949], Feigl [1943], Kraft [1950], Joergensen [1951]). The third wave, the first of really retrospective accounts falls into two kinds. The first was furnished by veteran participants and sympathetic students of the movement. These retrospectives come in various guises: though their tone is clearly indicative of the fact that the Viennese dream is over, their assessments of its failures show varying degrees of either apologetic reconstructionism for its crude early enthusiasm or tenacity in retaining at least some of the (various) original goals in their broadest outlines.(Ayer [1959b], Carnap [1963a], Feigl [1969a], [1969b], Hempel [1969], Hanfling [1981].) The second type of retrospective account is far more critical of the shortcomings of the original program, follows Quine's persistent criticisms ([1948], [1951]) and (radicalising Hempel's [1951] chronicle of modifications) rejects the
assumption of the Received View in its entirety (Putnam [1962], [1969], Romanos [1983]). Their "internal" criticism complements that of the "external" critics of Logical Empiricism's philosophy of science (to name but Kuhn [1962]). All of these writings about the Vienna Circle--apart, of course, from Carnap's, Frank's and Neurath's own accounts--concentrate on its orthodox development and leave its Neurathian dimension out of account.

Recent Developments

Of late, a fourth generation has begun to establish a new perspective on the Vienna Circle which challenges various aspects of these various, by now traditional, historiographies. With the historical distance between current Analytic philosophy and its emergence from the Received View steadily increasing it has become easier to discern a philosophically respectable rationale behind the rejected doctrines of Logical Positivism. There is, so the reappreciation goes, a continuity between the efforts of Carnap and Quine which has not been broken by the radical shift in the paradigm of philosophical inquiry brought about by the latter. Quine remained committed to a structuralist conception of knowledge: Carnap's grand logical designs had swept out the whole range of structuralist reason and so prepared the ground of which Quine discarded what could not be mastered by austerely extensional means. This is what I call the "New Canon" of Vienna Circle scholarship (see sect. 2.3).

Neurath has not yet received this attention in Anglo-American literature. Apart from exceptions listed in sect. 2.3.4 Neurath remained unrediscovered throughout the 1970's and largely remains so today: the recently published volume of essays on Quine (Hahn & Schilpp [1986]) for example, contains not a single discussion of his philosophy's relation to Neurath's. (The lone review of Neurath [1973]--Watkins [1974]--even misinformed its
readers that Neurath's Boat would be absent there and that of Neurath[1983]--Eisser[1986]--mixes unattributed quotes and unreflected prejudice.)

The not only by contrast very lively Continental Neurath rediscovery is noted in sect. 2.4. (This is not to say that what I call the traditional view of the Vienna Circle is not still being propounded, and refuted, in German-speaking lands: see, e.g. Tuschling & Reismueller [1981].) Some of the earliest papers of the Austro-German Neurath renaissance were presented at the Third International Wittgenstein symposium of 1978 (Berghel, Huebner & Koehler [1979]), more are collected in the proceedings of two conferences in 1982, the Viennese Schlick and Neurath symposium (Grazer Philosophische Studien 16/7) and the Bielefeld colloquium on the history of the Vienna Circle (Dahms [1985]); the same year also saw the publication of an exhibition catalogue of the graphical work of Neurath's museums and institutes in combination with a collection of short papers on all the varied are as of Neurath's work (Stadler [1982a]), perhaps the best prospectus of Neurath's broad field of activity. Some monograph-length studies of Neurath in the context of the Vienna Circle have by now appeared and are duly noted in the text above.

Bibliographies

The following bibliographies are to be noted. On the yet developing Vienna Circle and early associated groups see—particularly for their annotations, Neurath et al [1929] and Erkenntnis [var. vols.]; for more complete listings of the Circle, its sympathisers, their reception and their assimilation into Anglo-American philosophy see Feigl & Sellars [1949], Ayer [1957], Rorty [1967], for its precursors and for their reception in the German philosophical literature see Schnaedeelbach [1971] and Tuschling & Rischmueller [1983]. More or less definite listings on individual authors are contained in individual collections of their writings or of essays about them: on Neurath's publications see Neurath
[1973] as supplemented in Neurath [1983]; on the publications and exhibitions of his museums see Stadler [1982a]; see also sect. 2.5.3. On Carnap see Benson [1963], on Tarski see Tarski [1956], on Gödel see Gödel [1986] and on Zilsel see Dvorak [1981]; on most other members of the Vienna Circle and some of their forerunners see the volumes of their writings in the Vienna Circle Collection: Schlick [1979], Menger [1979], Hahn [1980], Feigl [1981], Waismann [1982], Mach [1905/76], Boltzmann [1974]; on the school of Polish logicians see McCall [1967].
13.2 Bibliography

Note: References in the text are given by author followed by the year of page number of original publication. When more accessible reprints are accessible the page numbers cited refer to the edition of the work in question marked in this bibliography by "(*)". Where the page references is preceded by "G" the page reference is to Neurath 1981, and where preceeded by "E" it is to the original in Erkenntnis. All translations from materials available only in German are mine.

Abbreviations: "E": Erkenntnis; "GPS": Grazer Philosophische Studien; "JP": Journal of Philosophy


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