The London School of Economics and Political Science

The historical roots of C.G. Hempel's D-N account of explanation: The protocol sentence debate and a candidate for philosophical methodology

Sheldon Steed

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Abstract

C.G. Hempel's D-N account of explanation marks a cornerstone in the history of philosophy of science. Standard interpretations construe it as a naïve, if noble, attempt to characterize scientific explanations: science locates laws at work in the world and facilitates explanations by indicating that the relevant circumstances fall under the cover of those laws – an idea these days appearing credulous at best. The present examination is motivated by a curiosity about whether there might be something more interesting to be said about the point of Hempel's account.

This thesis thus considers the historical roots of Hempel's thought in the Vienna Circle protocol sentence debate of the 1930s. Certain principles inherent in that debate suggest a reading of the subsequently developed D-N account not as a naïve scientistic approach, but as a conventionally adopted framework with which to clarify explanatory candidates in science. It argues Hempel was not after the 'right' account of explanation, but rather one which could be assessed for its merits based on how effectively it serves to clarify explanations.

This thesis first examines the point of the protocol sentence debate, which was not over justifying, via logic, grounding all scientific knowledge in sensation. Rather, as empiricists, certain members of the Vienna Circle took the language of sensation as a conventional starting point for the construction of scientific language and argued over the form and status of the basic statements comprising that language. Second, it surveys dissenting criticism within the debate and Hempel's problematic defence of the left-wing view in the Circle.

Third, it locates certain principles that came out of the debate and shaped the subsequent development of Hempel's D-N account.

Finally it suggests a reformulation of the D-N account as a conventionally adopted framework for the assessment of explanations and indicates how it may be understood as a candidate for philosophical methodology.

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Dedication

For Angus and Johnny: "The moon! You can go there!"

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Chapter One: Introduction – The historical roots of C.G. Hempel's D-N account of explanation

All transformations of science take place within language, not by confrontation of language with a 'world', a totality of 'things' whose variety language is supposed to reflect. –Otto Neurath. Physicalism. (1931c, p. 54)

After studying Carl Hempel's work for a number of years, a graduate student visiting Paris had the pleasure of meeting a distinguished scholar at the Collège de France à Paris. Upon confessing his fondness for Hempel, she replied, "Vous ne l'avez pas connu!" She seemed to suggest that if he had known Hempel and his work, he might not have bothered. Having worked in proximity to Hempel in the 1970s, she certainly was qualified to hold such a view. Hempel, to be sure, is regarded as a titan in the history of philosophy of science, but few appear to take his work seriously as a philosophical programme that may yet inform contemporary philosophers in their assessments of science and its philosophical underpinnings. That view is perhaps justified given what is commonly understood to be the point of the work for which he has become infamous in the field. It sees Hempel's D-N model as giving criteria of adequacy for what counts as a scientific explanation: science locates laws at work in the world and facilitates explanations of events by indicating that the relevant circumstances fall under the cover of those laws – an idea these days appearing credulous at best. On this view, the D-N account of explanation can be characterized as a commendable first-step, if a naïve and hopelessly flawed one, in the pursuit of accounting for explanation in science.

This thesis does not take up the standard assessment of Hempel's D-N account, but has been motivated by a curiosity about whether there may indeed be something more interesting to be said about the point of his explanation programme. Though it does not develop a full-blown, reconstituted rendering of Hempel's D-N account, it attempts to indicate why one might reconsider what Hempel was up to, and the direction such a reconstituted view might take.

In short, Hempel does not appear to be committed to the notion that science discovers laws in the world and one merely needs to formulate explanations applying those laws to empirical circumstances in order to render them scientific. Rather, his attempt to develop a methodology that does justice to both scientific procedure as it is carried out and the normative goals at which it aims, stems from scepticism regarding the ability to underpin scientific knowledge with an ontological foundation. Absent any such foundation, Hempel's emphasis is on the construction of a methodology with which to confront the world. The D-N account is not understood by him to be the 'right' account of explanation. Rather, it is one which appears to do justice to what scientists take themselves to offer in a good deal of their explanations and can be assessed based on how well it enlightens inquirers to the nature, structure and scope of scientific knowledge.

This aspect of the thesis is rather programmatic: it indicates a research direction that might take shape and, possibly, influence the way in which philosophers ask questions about the world and the sorts of answers they may expect. The final chapter points toward the principal philosophical argument of this thesis: it examines the D-N account as a conventionally adopted methodology that affords a systematic framework with which to confront problems of scientific knowledge. It will indicate that three principles underlying Hempel's work – adopting the D-N account as a convention, applying it as a relativized a priori framework and advocating the methodological distinction between formal and empirical aspects of science – function not as a solution to the problem of knowledge, but mark an approach to it. Absent foundations – ontological, epistemological or otherwise – how can one construct a positive account of certain features of scientific knowledge? The D-N account can be understood to be an attempt to formulate such a construction. It does not 'attempt to get the world right'. Rather, it acknowledges that we are confronted with a world for which we cannot fully account and attempts to provide some systematic structure to a confrontation of that world. As Otto Neurath (1913) said, we are like wanderers lost in an intellectual forest and must decide on some direction without adequate means of determining the correct one.

Though the motivation for this thesis came rather out of the blue from a one-hour class on Hempel at the London School of Economics, its emergence may not have been unforeseen. The turn of the century saw the publication of two collected volumes on Hempel's works (2000b, 2001) that indicate, in part, his shift in focus in later life from the logical aspects of science to a socio-historical bent akin to Neurath or Thomas Kuhn. Another volume in honour of Hempel's life (Hempel, 2000a, J.H. Fetzer, [Ed.]) begins to delve into his philosophy of science, bringing to the fore, among other things, the question of the relation between historical and logical studies in his philosophy. Moreover, one may recognize a loose connection to the historical work of Michael Friedman (1999) who has begun to identify the empiricism of logical positivism in Rudolf Carnap, Hempel and others with their neo-Kantian roots (if examined as their reaction to neo-Kantianism), as opposed to the British empiricist tradition of Russell and Whitehead.

This thesis will forgo sweeping connections among the history of ideas in philosophy of science and attend rather to the historical roots of Hempel's thought. Attention there suggests why one might be compelled to re-examine his philosophical corpus generally, and his D-N account of explanation in particular. Thus a good portion of the present work is dedicated to an examination of the Vienna Circle protocol sentence debate of the 1930s, from which Hempel emerged. Barring some notable examples, the point of that debate has not been understood sufficiently, so this thesis surveys its dynamics to elicit what seems to have been at stake for Carnap, Neurath and Hempel in order to give a backdrop against which to place Hempel's subsequent developing philosophy. It also provides occasion to indicate how one might grasp the outcome of that debate.

The protocol sentence debate was a response to the epistemological project in Carnap's *Der Logische Aufbau der Welt*, published in 1928. The debate over this influential book centered on the question of the possibility of scientific knowledge. Carnap's *Aufbau* proposed constructing a constitutional system,[1] wherein all scientific concepts (indeed all meaningful concepts) might be derived from a few basic concepts rooted in observation. The debate thus centered on the status and form of basic observation statements (protocols) that serve as the basis for the constitutional system and knowledge that may be understood to be established within it.

Thomas Uebel (1992, p. 14) writes that in the traditional understanding of Vienna Circle philosophy, scientific knowledge was taken by its members to be "a body of statements whose claim to embody knowledge could be established, given experience, by logical means alone." Uebel worries that this understanding, though not altogether incorrect among respective members, obscures the relative lack of unity among the views within the Vienna Circle, and ascribes to its members a naïve foundationalist conception of scientific knowledge. As will be discussed below, the protocol sentence debate indicates that there was by no means agreement on both how the basis for scientific knowledge should be understood, and whether any such basis could be understood as foundationalist (indeed quite the contrary for Neurath and Carnap, in particular).

Much recent scholarship has noted the inadequacy of the traditional view of the goals of the Vienna Circle.[2] Uebel (1992, p. 15) states that the traditional view of the Vienna Circle stands, at least, in need of clarification because "[a]ll of the thinkers of the Vienna Circle struggled with the Kantian question of the possibility of objective knowledge and sought to develop an account of justification that addressed the normative force which talk of objectivity possesses." They did not ignore Kant's criticism of empiricism – "how epistemic

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[1] "Constitutional system" is used here, following Richardson (1998), though the R.A. George translation of the *Aufbau* used presently refers to a "constructional system."

[2] For examples and bibliographic resources, see Giere and Richardson (Eds.) (1996) and Uebel (1992)

norms could be derived from an investigation of fact," as suggested by Popper (ibid) – rather they "sought to answer Kant," since in the debate the skeptical claim regarding the impossibility of knowledge "was itself at issue."

Friedman (2000, p. 40) notes that the debate began in the Vienna Circle meetings in 1928-1930, "wherein quite a fundamental split developed between Schlick and Waismann, on the one side, and Neurath, followed quickly by Carnap, on the other." He writes further that (ibid), "the two sides can be seen as adopting opposing stances toward the *Aufbau*, with the Schlick-Waismann camp (...the right wing of the Circle) pushing in a foundationalist-subjectivist direction and the Neurath-Carnap camp (...the left wing...) pushing in a 'physicalist' or intersubjectivist direction." Further, between Neurath and Carnap, Neurath objected to the methodological solipsism of the *Aufbau*. 'Methodological solipsism' characterized the notion that a construction of scientific knowledge is rooted in immediate experience. Neurath resisted this notion on the grounds that solipsism itself cannot be given meaning outside the language of physicalism because statements about immediate experience cannot be given meaning until framed within the intersubjective language of physicalism (and that consequently it cannot form the basis of physicalism). Schlick from the other side pushed for a more explicit reliance on "individual subjective experience" as a foundation for scientific knowledge (ibid). The dynamics of this interaction shall be discussed below.

Uebel (1992, p. 24) divides the debate into four stages. The first, as mentioned, was initiated chiefly by Neurath's criticisms of Carnap's epistemological stance in the *Aufbau* and lasted until 1930 within the Vienna Circle weekly meetings. The second marks what Friedman calls the 'public' form of the debate and occurred principally between Carnap and Neurath in publications in *Erkenntnis* in 1931 and 1932. Uebel marks the third stage as Moritz Schlick's response to the debate over protocols among the left wing of the circle and Hempel's subsequent reply, and lasted until about 1935 when Carnap adopted Tarski's theory of truth. The final stage lasted from 1935 until "it petered out in the early 1940s" (ibid). Considering this latter stage suggests the means for a less disjointed understanding of both the development and relative agreement on philosophical methodology within the left wing of the Circle.

Though members of the Vienna Circle shared the goal of formulating a language purged of metaphysics, the protocol sentence debate emerged over how, exactly to do that. Thomas Oberdan (1996, p. 274) notes that metaphysical statements are problematic for physicalism because these were, in Carnap's terms, "metalinguistic statements masquerading as factual statements." Since metaphysical claims lie outside the domain of the physicalist language, they were taken to be meaningless because physicalism is understood to comprise the most general framework for meaningful statements. Thus one particular problem for metaphysics is that it purports to provide claims that lie outside the language of science, while making factual claims that appear to be meaningful within it.

This thesis is divided into four discussions. First, it survey's Alan Richardson's (1998) historical evaluation of Carnap's seminal text. Richardson numbers among proponents of what might be called the "new received view" of logical positivism, attempting to grasp Carnap's programme in its own terms. Richardson argues that the traditional received view, inherited from Quine, misconstrues the point of Carnap's *Aufbau* and obscures tensions within that programme that led Carnap eventually to reject it in favour of his logic of science. In short, Carnap did not understand the reductionist project within the *Aufbau* to be a solution to the problem of scientific knowledge. Rather, he understood reductionism to be the problem itself: he asked how far one could enlist new tools of logic to facilitate a reduction of theoretical terms in science to basic observation. He aimed neither at an epistemological justification of subjective experience as a starting point for a system of scientific knowledge, nor a justification of the analytic framework employed to facilitate any such potential reduction. Rather he took observation and a specified formal framework as conventional starting points for the construction of a constitutional system of knowledge: he maintained no underlying ontological or epistemological commitments to the construction of his system, but held that one could not raise epistemological worries at all without first specifying (through empirical descriptions of conventions) a class of basic observation statements and formal system of relations to link those statements within a constitutional system. Richardson distinguishes the epistemological project in Carnap's *Aufbau* from Russell's external world project (1914), arguing that the former's was not merely a more rigorous formulation of traditional empiricism, but aimed at reformulating what is at stake in the construction of a scientific system of knowledge.

Richardson's interpretation of the Carnapian programme suggests a way to understand the point of the protocol sentence debate – and a philosophical orientation of the participants in the debate generally. That debate centred not on whether basic observation statements were possible, but what the form and status a class of these sorts of statements might take. That is, it was waged over how to formulate an adequate – conventionally adopted – philosophico-scientific methodology for the assessment of scientific knowledge. In contrast to Russell's external world programme, it was not aimed at justifying any prior epistemological commitments that might underwrite their empiricism. Rather, assuming empiricism as a conventional starting point, Otto Neurath worried that the structure of Carnap's programme was given to metaphysics, which led to the debate over whether basic observation statements (protocols) ought to be understood within the intersubjective language of physicalism, or whether proposing a phenomenal language as distinct from physicalism could be given meaningful formulation.

Carnap adapted his view throughout the protocol sentence debate both in response to Neurath's worries about metaphysics in a system of scientific knowledge, and worries about tensions Carnap came to discover within his own project. By the mid-1930s Carnap had come to reject the project of the *Aufbau*, which is characterizable as the problem of how to get from subjective experience to intersubjective knowledge, in favour of the logic of science, wherein philosophy is understood to consist in the logical clarification of languages.

By 1932, before his full procession toward the logic of science, Carnap and Neurath's views can be compared and contrasted. Whereas Carnap had adapted his views closer to Neurath's worries by suggesting that phenomenal language (of subjective experience) ought to be understood to be a part of intersubjective physicalism, Neurath still worried that an unwarranted status might be given to protocols so understood. Basic observation statements could not be understood as distinct from any other non-protocols within the language of physicalism. He worried that, understood in Carnap's terms, protocols might be given some special status as indubitable building blocks for the edifice of science. Neurath remained adamant that protocols could not be conceived as somehow primitive or unrevisable. As with all statements in science, they were open to revision.

The second discussion outlines Schlick's contribution and Hempel's introduction to the debate in the mid-1930s. Schlick worried principally that the shape of the debate had ignored a central feature of scientific knowledge – that it must be rooted in certainty. Allowing for protocols to be revisable relativizes the entire programme, thereby rendering the Carnap/Neurath views characterizable as a coherence theory of truth. Hempel's problematic reply argues that theirs is indeed a coherence theory of truth, but of a restrained sort.

Both Carnap and Neurath would reject, in their respective ways, Hempel's assessment of their views. And that rejection can be understood through a discussion of their adoption of Tarski's theory of truth. In short, Carnap rejected that his was a coherence theory at all, originally because he thought 'truth' was not syntactically formulable, and eventually because he understood 'truth' to be a concept only specifiable in formal, and not natural, languages. For his part, Neurath ought to be understood as advocating a coherence theory of justification or acceptance, rather than truth, although he himself was unclear about this in his own work. To be sure, Hempel attempted to rectify his own response to Schlick nearly fifty years after the initial publication.

Third, in spite of Hempel's misfire in his 1935 paper, he nevertheless has demonstrated a striking coherence in the development of his thought, which can be obscured by even those sympathetic to his overall programme. As will be seen, Richard Jeffrey remarks that Hempel held significant regret about his 1935 paper and postulates that this was in part because of Bertrand Russell's 1940 paper, which heaped scorn upon his and Neurath's alleged 'coherence' stance during the 1930s. However, Hempel's view (and certainly the views of Neurath –

and especially Carnap) was in transition throughout the protocol sentence debate. Indeed it is in virtue of the protocol sentence debate itself that certain problems emerged in their views, which caused them to sharpen their positions. As mentioned, Carnap came to see a tension in the epistemological project of the *Aufbau* of accounting for the move from subjective experience to intersubjective knowledge, which led him to reject it. Furthermore, the protocol sentence debate came to see a sharpening of the distinction between confirmation and truth. Russell criticizes what he calls the Hempel/Neurath coherence theory of truth on the grounds that such a theory conflates confirmation with truth. However, this would have been old news for Hempel who, by 1940, had not only understood the distinction pushed by Carnap, but employed it in his 1939 paper "Vagueness and logic." Russell's paper is taken to be an important document of that period, but he misunderstands the point of the protocol sentence and attributes to Hempel views that he had already abandoned.

By the late 1940s the protocol sentence debate largely seemed to have dissipated, rather than find any express resolve. Thomas Uebel (2001) questions whether any such resolution might be possible: though Uebel argues that a resolution is not impossible in principle, this thesis argues that one need not search for an 'in principle' resolution. Rather, the Neurath and Carnap views divide into two aspects of a common methodological inquiry into the structure of scientific knowledge: Carnap attends to formal attempts to clarify the structure of statements, while Neurath focuses on the more general contextual considerations within which a formally reconstructed framework of a set of statements (or model) might be said to be applied to the empirical domain. Formal and empirical considerations are both indispensible for any adequate account of scientific knowledge. Hempel, it is argued, can be seen as a unifying methodologist in this respect: even while attending to the logical aspects of explanation in his D-N account, he remains cognizant of questions of empirical relevance of that account – indeed the formal features of the D-N account raise the question of empirical adequacy.

Fourth, the final chapter reconsiders how the D-N account can be understood given the historical background. The examination of the protocol sentence debate gives an opportunity to understand the dynamics and outcome of an important movement in the history of philosophy of science. Furthermore, for the present purposes it suggests certain principles for which one needs to account when examining Hempel's D-N explanation. The *Aufbau* sought no prior epistemology or ontology to justify Carnap's empiricism. The protocol sentence debate thus is understood to be about the status and form of basic protocols that can serve, conventionally, as the basis for Carnap's constitutional system. The entire edifice of Vienna Circle physicalism is adopted without reference to some underlying state of the world. It is assumed as a systematic, relativized a priori framework with which to confront phenomena in the world and their discussions centred on how to conceive of an adequate characterization of that general framework. Moreover, the distinction between logical and pragmatic aspects of science came to be a central methodological device in their assessment of science.

Likewise, the subsequently developed D-N account of explanation need not be understood to be an attempt to provide the right account of explanation. Rather, one can adopt it via convention and apply it as a relativized a priori framework that employs an indispensable methodological distinction between formal truth and empirical acceptance for the purpose of explicating explanatory candidates in science. It can be assessed based on its fruitfulness. The D-N model provides the inferential framework within which to structure explanations of empirical phenomena. Of course most interesting science provides incomplete explanations and it remains to an empirical examination to suggest how one might categorize them. However, the complete model provides a systematic framework against which to do so. And it gives a mandate to push partial explanations or explanation sketches toward completeness, thereby specifying a goal of expanding the classes of accepted complete explanations of the world. Science, by this account, is a heuristic in the search for general laws that apply over ever-expanding empirical domains.

Thus the final chapter first, outlines the principles underlying Hempel's D-N account, as extracted from the protocol sentence debate. Second, it examines two important papers in Hempel's canon to indicate how those principles appear to be in accord with his overall programme. Third, it examines Hempel's constructed categories of incomplete explanations with an eye for showing how these categories serve as methodological constructs for the purpose of clarifying explanatory candidates. Indeed, the categories are defined according to their logical structure. Empirical explanatory candidates can be placed within a given category to raise the pragmatic question of whether and on what grounds they should be accepted – one looks at their constitutive premises outlined by the D-N framework to determine whether they will be accepted or not. The fourth section looks at two examples to show the way in which the D-N methodology can clarify certain problems of explanation. And finally it will look briefly at how the canonical view of the D-N account goes wrong and point to a programmatic outlook for the D-N methodology as a systematic framework with which to confront and assess scientific knowledge statements.

It is hoped that the present inquiry will contribute to the new understanding of the point of logical positivism and indicate how Hempel fits in with that understanding. One guiding impulse is the response to a philosophical undercurrent that attempts to hijack Hempel's D-N model in service of realism or foundationalism. Neither of these appears to characterize what Hempel was after in his account of explanation, but both reinforce Hempel's reputation as misguided and naïve. Examining the historical roots of his emerging philosophy of science ought to shape readers' expectations of the point of the D-N account and indicate an otherwise overlooked richness that might suggest aspects of his work as a resource for addressing contemporary problems: reconstituting an understanding of the point of the D-N account of explanation as a framework for assessment (as it appears Hempel intended it), can indicate the way in which Hempel may be viewed as providing a fruitful candidate for a contemporary philosophical methodology.

Chapter Two: Carnap's *Aufbau* and the protocol sentence debate

In the *Aufbau* Carnap is concerned with, "a step-by-step derivation or 'construction' of all concepts from certain fundamental concepts.... It is the main thesis of construction theory that all concepts can in this way be derived from a few fundamental concepts..." (Carnap, 1928, p. 5). Thus, Carnap in the *Aufbau* intended some form of a reductionist programme by which, given the success of such a programme, all scientific concepts would be reducible to a class of basic concepts rooted in observation. Metaphysical concepts cannot find a place within the constitutional system on the grounds that they cannot be constituted within it. What is crucial for present consideration is how the nature, scope and limitations of such a reductionist programme was to be understood by Carnap himself. Furthermore, how was his view affected by the reception of the *Aufbau* among Vienna Circle members?

The present thesis rests, in part, on the new received view of logical empiricism. Historical reevaluation over the last decades of the ambitions and scope of logical empiricist philosophy of science has suggested the need to reshape conventional understanding of those ambitions and the point of logical empiricism. Though by no means yet providing a unified account, historical analysis suggests certain salient features for which one must account to grasp fruitful lessons that might be drawn from the logical empiricist programme – in particular, its attempts to account for the desired normativity in a 'scientific philosophical' assessment of scientific knowledge given foundational problems for any system of knowledge. One of those features is the nature of the reductionist programme attributed to Carnap. Another is the extent to which (and the ways in which) logical empiricists were committed to undergirding the traditional empiricist attempt to justify rooting knowledge in sensation. The present section considers the first of these features, while the second can be more clearly examined by considering the dynamics of the protocol sentence debate in the final section of this chapter.

Alan Richardson numbers among contributors to the new received view and explores the question of how Carnap understood his own project within the *Aufbau*. His *Carnap's construction of the world* (1998) provides a comprehensive assessment of Carnap's *Aufbau* and will be useful for the present survey of key themes that motivate the protocol sentence debate. Richardson (1998, p. 13) argues that grasping Carnap's programme in its own terms indicates the way in which the traditional received view misses the epistemological point and domain of that text; conventional reception attributes to Carnap "certain goals that he does not in fact have and makes central certain problems that are not central to Carnap's own avowed concerns." As will be discussed, Richardson argues that Carnap's empiricism needs to be distinguished from traditional empiricism on the grounds that he remained ambivalent about empiricist ontological commitments. His construction of scientific knowledge takes experience as a conventional starting point for empiricist accounts of science and he does not feel compelled to justify this starting point beyond locating behavioral psychologists' accounts of practitioners who, in fact, take sensation statements as the basis. Moreover, by missing the intended point and scope of the *Aufbau*, the received view obscures what Richardson takes to be the deeper tension that led Carnap eventually to reject its epistemological programme.

This chapter divides into four parts. First, it outlines Richardson's contrast of Carnap's project in the *Aufbau* from Russell's external world programme and it specifies the nature of the epistemological project of the *Aufbau*. Second, it outlines Neurath's critique of the *Aufbau*. Third, it accounts for Carnap's shift through the early 1930s toward his rejection of epistemology. Carnap's thought provides a general indication of the orientation of Vienna Circle members, both to indicate their shared assumptions and how they differed.

And finally, this chapter provides a comparison and contrast of Carnap and Neurath's views in 1932.

The *Aufbau* , Russell's external world programme and the received view

Richardson argues that Carnap's *Aufbau* needs to be distinguished from Russell's external world programme. According to Richardson (1998, p. 5), Carnap's *Aufbau* marks his "most sustained attempt to provide a general epistemology of empirical knowledge." Starting from immediate experience, Carnap attempts to formulate two languages: the phenomenal language, which consists of statements of immediate experience and the intersubjective "physicalist" language, which marks the domain of scientific statements. The epistemological problem of the *Aufbau* then is to account for the move from immediate, subjective experience (his version of methodological solipsism)[1] to the intersubjective domain of the body of scientific statements (physicalism). The project is to examine the extent to which formal tools of logic can be enlisted to provide translation rules from the phenomenal to physicalist languages, which exhibits important differences from Russell's external world programme.

Richardson (ibid, p. 5) notes that the *Aufbau* is "a central document of analytic philosophy" and is seen therefore as crucial to understanding "the formation of the project of logical positivism,"[2] and analytic philosophy more generally. However, its influence has come to be understood via the lens of the received view of the point of its impact. The principal conduit of this view has been W.V.O. Quine in his monumental papers "Two Dogmas of Empiricism" (1951) and "Epistemology Naturalized" (1969).

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[1] Methodological solipsism can be understood here as the notion that subjective experience is the starting point for a systematic, philosophic account of scientific knowledge.

[2] Although, they have historically distinct affiliations in Berlin and Vienna, 'logical positivism' and 'logical empiricism' can refer presently to a common programme without obscuring their different orientations.

In particular, according to Richardson (1998, p. 10), Quine provides what may be taken to be the "essence of the received view" of the *Aufbau*. Carnap's constitutional systems are the best attempt to fulfill traditional empiricism's promise to deduce systematically all language of science from an epistemologically privileged language of sense data. Bertrand Russell provided an updated version of the empiricist project, while Carnap made the most successful attempt to carry it out.

Of course, Quine claims the *Aufbau* fails in this task, both in practice and principle. Carnap's attempt fails in practice, while scientific discourse simply cannot always be interpreted strictly in terms of observation statements, since individual theoretical statements cannot be tested against observation. Rather, empirical assessment pertains to groups of theoretical statements collectively. Thus, one cannot deduce individual higher-level theoretical statements in science from observation. Insofar as the goal of the *Aufbau* was to construct a constitutional system wherein scientific statements can be constituted via a step-by-step construction from observation, that goal was not (and could not be) achieved.

Richardson (ibid, p. 12) remarks that Quine sees this failure within Carnap's own internal moves in the *Aufbau*. When moving from phenomenal language to the physicalist world of intersubjective language, Carnap shifts from giving explicit definitions to "methodological principles for the mapping of qualities from the private realm of experience onto physical space-time points". As Quine says (1951/1961, p. 40), the proposition 'Quality *q* is at *x;y;z;t*' could never be translated into Carnap's language of sensation and logic because 'is at' is undefined: "the canons counsel us in its use but not its elimination." Richardson (1998, p. 12) suggests that Quine sees this conclusion as unavoidable and "indicates that there is in fact no way Carnap could have succeeded in the task he set himself." If Carnap cannot do it, Quine concludes that the programme of reducing science to sensation should be given up altogether.

Richardson (ibid, pp. 12-13) writes further:

> This, then, is the received view of the [*Aufbau*]: It was the most ambitious and successful attempt to use the resources of modern logic to carry out the reduction of all scientific discourse into the terms of immediate experience. The principal legacy of the book is that it failed in this reduction – that it failed not merely in fact but in principle.

However, Richardson argues that the received view fails to take into account Carnap's view on the point and domain of his epistemological worries. He does this by distinguishing Carnap's *Aufbau* programme from Russell's external world project. The received reading comes in part from Quine's affiliation of the Carnapian project and that of Russell's external world programme. Certainly they shared many key points of convergence, like the notion that new tools of logic (developed, influentially, by Russell and Whitehead) are to be enlisted in the service of answering the question of the problem of knowledge. However there are substantial points of difference regarding where logic plays such a role and the domain and nature of epistemology.

The account of those differences, according to Richardson, is that for Russell's external world programme, logic is used in service of justifying his prior epistemology of acquaintance. Russell distinguishes 'knowledge by description' from 'knowledge by acquaintance'. The former is characterized as when facts about an object are known only via descriptive concepts ('the surface of the sun is composed of mostly hydrogen and helium'). The latter, by contrast, is characterized as when one is confronted with something. According to Richardson, such things might be particular objects, universals or facts (ibid, p. 18). Thus, one may have knowledge of the composition of the surface of the sun when the proposition, 'the surface of the sun is composed of mostly hydrogen and helium', "has been fully analyzed" such that "all the constituents of it are entities with which one is acquainted" (ibid, p. 18).

It should be mentioned that Russell acknowledges the need for a certain hesitancy with which one approaches an ontological commitment to objects of the external world. It will be helpful to quote Russell (1914, pp. 73-4) at length:

> While admitting that doubt is possible with regard to all our common knowledge, we must nevertheless accept that knowledge in the main if philosophy is to be possible at all. There is not any superfine brand of knowledge, obtainable by the philosopher, which can give us a standpoint from which to criticize the whole of the knowledge of daily life. The most that can be done is to examine and purify our common knowledge by an internal scrutiny, assuming the canons by which it has been obtained, and applying them with more care and with more precision.
>
> [Continues.] Philosophy cannot boast of having achieved such a degree of certainty that it can have authority to condemn the facts of experience and the laws of science. The philosophic scrutiny, therefore, though sceptical in regard to every detail, is not sceptical as regards the whole. That is to say, its criticism of details will only be based upon their relation to other details, not upon some external criterion which can be applied to all the details equally. ...[i]t is not that common knowledge *must* be true, but that we possess no radically different kind of knowledge derived from some other source.
>
> [Finally.] Universal scepticism, though logically irrefutable, is practically barren; it can only, therefore, give a certain flavour of hesitancy to our beliefs, and cannot be used to substitute other beliefs for them.

For Russell then, an ontological commitment to objects of the external world is necessary for philosophical inquiry to proceed at all, but any such ontology does not preclude a certain hesitancy one may hold toward that commitment. Nevertheless, assuming that there are objects about which one can have knowledge by acquaintance provides a set of objects that, given sufficiently robust logical analysis, can be understood in terms of sense data. Richardson notes that this presents the role of logic both to facilitate an interpretation of all objects of the external world in terms of sense data and deduce higher theoretical concepts from sense data. Moreover by successfully translating all external objects into sense data and deducing science from experience, the external world programme thereby vindicates the ontology that was necessary to motivate philosophy in the first place. All science in this instance can be deduced from sensation, while one need no longer be bothered by problems regarding the external world, since all objects can be understood in terms of sensation. Richardson (1998, p. 19) states, "we legitimate our scientific ontology by defining it away in favor of the objects of acquaintance."

For Carnap on the other hand, epistemology has nothing to say about the justification of any prior commitments understood as ontological or, indeed, epistemological. Thus, he remains ambivalent about ontological commitments to objects in the world and offers no justification for the claim that one can know these objects by acquaintance or any other means. Further, and central to the *Aufbau*, he offers no justification for taking sensation as his starting point. While for Russell one must assume an ontology of the external world and knowledge of objects by acquaintance, for Carnap, some form of logical rules and basic observational statements must be assumed before one can raise epistemological questions at all. One cannot specify the scope of what we can know until one first employs conventionally adopted, factual claims and a logical framework with which to interpret relations among them. According to Richardson (ibid, p. 22), the *Aufbau* was more than merely "a more thorough working through of Russell's external world program." If Quine's assessment were right, it might be understood to be a more rigorous undertaking of the attempt to translate propositions about the external world into the language of sensation, thereby contributing to the goal of legitimating a Russellian ontological commitment about those objects. However, Richardson argues that Carnap shared no such goal: the domain of epistemology in the *Aufbau* was in the logical connections between a class of basic observation statements and the higher-level theoretical statements of science. Although Carnap takes Russell's maxim for "scientific philosophizing" as the motto for his own work ("Wherever possible logical constructions are to be substituted for inferred entities."), he did not believe that a constitutional system that he was after need have anything to say about commitments held prior to its construction. Richardson writes (ibid, p. 23), "By the end of the book it is, moreover, quite clear that Carnap takes traditional empiricism and Russell's own views to be as infected with metaphysics as any traditional epistemological project."

Thus, Carnap takes it that the language of sensation as the "starting point" of a constitutional systems is a "scientific fact." That is, he understands empiricist epistemological systems as *assuming* knowledge begins in sensation, which can be understood to be a conventional starting point for him. This is perhaps a contentious claim, but the salient aspect of this point is that Carnap's epistemological project is not enlisted to justify sensation as the starting point for such a project. In so far as sensation as a basis for knowledge is taken to be a 'scientific fact' at all, it is understood as a psychological fact. As such, it has explanatory value only in so far as, in virtue of psychological dispositions of scientific practitioners (or scientific philosophers), one can provide a psychological description of the assumptions motivating them to adopt one or another starting point. As a consequence he is not concerned about justifying that starting point: "Carnap lacks any epistemological vocabulary of acquaintance to undergird the claim that this starting language is epistemically privileged or certain" (ibid, p. 24).

So one difference between Carnap and Russell is that the former has no goal comparable to the latter's acquaintance that will 'undergird' the starting language of sensation as epistemically privileged. A second is that they diverge in their attitudes to the role of logic:

> Russell has an antecedent epistemological point of view given in his adherence to the principle of acquaintance that allows him to have epistemological worries about the basic concepts of logic. This is not true of Carnap. No concerns of an epistemological nature about logic are in evidence in his book. (Ibid, pp. 24-25)

For Russell, given that talk of external world objects can be replaced with successful translation to sense data, the logical tools facilitating such a translation themselves come into question. For Carnap on the other hand, "logic must be in place before any epistemological question can be raised" (ibid, p. 25). Thus, Richardson claims (ibid) that for Carnap, epistemology simply has very little to say about metaphysical aspects of prior epistemological programmes.

Thus Richardson argues that the received view that the *Aufbau* was about a reduction of all scientific concepts via the tools of logic to an epistemically privileged language of basic observations is misplaced. The nature of the problem as understood in this way is whether science can be so reduced and how one would justify the epistemological status of a set of basic observation statements as a starting point for the system of science. On this reading, Quine surely would be correct to claim that the *Aufbau* must fail (which leads to his consequent naturalism). However, because Carnap has sought to recast epistemology with the new tools of logic as the move from subjective to intersubjective statements, rather than rehash an old empiricist program of justifying epistemic norms from experience, the received view talks past the *Aufbau*, ascribing to it goals that it did not have.

As will be discussed below, insofar as the project of the *Aufbau* failed, it is because the epistemological problem presented therein – how to get intersubjective scientific knowledge from immediate, subjective experience – comes to be understood by Carnap as a problem as unformulable and is replaced by the logic of science wherein philosophy is taken to be the logical clarification of concepts.

Two figures can illustrate the nature of the epistemological project of the *Aufbau*.

Figure 1: A comparison of the domain of application for Russell in 1914 and Carnap in 1928

A. [————————————————Russell's epistemology of acquaintance————————————————]

Justification ↓ Justification ↓

B. [External world ———————————— language of sensation ———————— intersubjective language of science]

↑ Justification

C. [————————————Epistemology of the *Aufbau* —————————————]

Figure 1 allows one to compare Russell and Carnap. Line B indicates the domain between the 'world' and science. The domain of application of Russell's epistemology of acquaintance centers on the external world and the language of sensation. In his programme the tools of logic are enlisted to justify appeal to the language of sensation, via his prior epistemology of acquaintance (by translating external world objects into objects known by acquaintance). This raises further epistemological questions about logic itself because it is enlisted in the translation of external objects into objects of acquaintance – why would one be justified in employing *this* logic to provide the epistemological underpinning of objects of acquaintance?

Carnap on the other hand does not seek to justify the language of sensation as a starting point. Rather, it is in virtue of establishing, via convention, a class of basic observation sentences, in conjunction with assuming a logical framework to provide a system of relations among concepts, that epistemological questions can be raised at all. Epistemology, for Carnap then, is the domain of the connection between the assumed starting point of the language of sensation and the intersubjective language of science – how does one get from statements about subjective experience to intersubjective statements of science (and, indeed, natural language generally)?

As Richardson states (1998, p. 27), "The new logic is, thus, not a tool to use in pursuit of a reductive epistemological-cum-ontological project bequeathed to us by the British empiricists, but rather a way of reformulating the whole question of what is at stake in philosophy." The question then arises in Quine about whether Carnap successfully justifies the body of scientific statements with sensation (observation statements).

Figure 2: A comparison of Quine's criticism of the Aufbau *and the epistemological point according to Carnap*

A. [——————————— Quine's interpretation of the point of the *Aufbau* ———————————]
Justification
←
B. [External world ——————— language of sensation ——————— intersubjective language of science]
→ given a sufficiently constructed system
Justification
C. [——————Epistemology of the *Aufbau* ———————————]

Figure 2 locates features for comparison of Quine and Carnap on the *Aufbau*. One might contrast Quine's conception of Carnap's goals from the latter's own with the *direction* of justification represented in Figure 2. The direction of the arrows indicates the nature of justification. From Quine's perspective, if Carnap's constitutional system works, he serves to justify the traditional empiricist claims that scientific knowledge is rooted in sensation. On the other hand, Carnap's justification might be understood to move in the opposite direction: a sufficiently constructed constitutional system can justify theoretical statements of science in virtue of their logical connection to basic observation statements. Any higher-level statements of science are thereby justified relative to the assumed class of basic observation sentences in conjunction with the assumed logical framework specifying deductive relations among constituents within the system.

An important feature of Quine's criticism of Carnap's reduction programme is correct. Regardless of whether one might distinguish Carnap's goal from that of traditional empiricism, Quine's claim that one can deduce only classes of theoretical sentences from observation (and not individual theoretical statements) places a major obstacle in the way of Carnap's ambitions to construct a step-by-step system of deduction between theory and observation. In his preface to the 1961 edition of the *Aufbau*, Carnap expressed that he shifted his view toward a more liberal notion of 'reduction' away from a strict definitional connection (p. viii). This point and its upshot can be made clearer in the outline of the development of his thought throughout the protocol sentence debate.

A second consideration is rather more suggestive and indicates an area that would be fruitful for consideration, though it would go too far astray from the present examination to pursue here. Russell claims that any ontology needs to be adopted with hesitancy. Rather than subscribe to full-scale skepticism regarding the basis for scientific knowledge, which proves unfruitful for any positive account of the possibility of knowledge, one proceeds with reservation, acknowledging that there is no standpoint from which philosophers might position themselves to assess any such account. One may question the extent to which this sort of commitment differs from Carnap's ambivalence regarding ontology. It might be noted that each takes some starting point as a necessity to proceed with their respective positive projects: Carnap with the conventional adoption of sensation and a system of logic; Russell with an ontological commitment to external objects. Without conflating their views, one might ask to what extent are their differences substantive? This question would inform an understanding of what one commits to when committing 'ontologically' to a particular viewpoint and could flesh out the scope of commitments that cannot be legitimated within an adopted scientific methodology itself.

This query might be read as an effort to undermine Richardson's attempt to distinguish Carnap's programme from traditional empiricism: Carnap's conventionalism may be rendered not such a distinct project after all and might be understood to misconceive the nature of ontological commitments in a scientific philosophy. However, one may not feel compelled to take one side or another in this consideration and, as noted, the question is raised to consider what sorts of problems might present themselves from Richardson's interpretation. What should be noted though is the reasons Carnap may have had a problem with Russell's external world programme. Even if one removed reference to the metaphysics of Russell's commitment, Carnap (and his ilk) may still be troubled by the presence of ambiguous or unanalyzed concepts in Russell's account. For example, in his claim regarding the hesitancy with which one needs to employ 'knowledge by acquaintance', Russell (1914, p. 74) writes, "it is not that common knowledge *must* be true, but that we possess no radically different kind of knowledge derived from some other source." A good Russellian may utilize the notion 'true' in this instance with reservation. A good *Aufbau*-Carnapian, on the other hand, might be reluctant to use that term in its pre-analytic form at all on the grounds that it cannot be explicated precisely. It is in virtue of adopting one system or another that one might formulate the concept 'true' in the first place. In strong terms, one might argue that employing a concept for which there is no analytic form can have no meaning in a language. In looser terminology, it is, in part, precisely a notion of 'true' that one is after in the constitutional system. It can be misleading to use the term unless one can specify what the concept means.

A further Carnapian worry would be that utilizing terms like 'true' blurs the boundaries of the actual successes in constructing a constitutional system. That is, one might be led to claim that successes in science somehow vindicate the concept 'true'; that somehow the successes extend beyond the domain within which their success is couched. Claiming merely that the notion is used hesitantly only obscures the scope of a successful construction of Carnap's (or any other) scientific system. 'True' cannot provide explanatory import within a scientific system until it has been specified what that concept is. Then the concept can be understood only relative to the framework within which its meaning has been expressed. This point, too, will be expanded in the discussion below on Tarski's theory of truth.

However there is yet a significant issue that must be taken into account regarding Carnap's anti-foundationalist stance. In his autobiography he writes (1963, p.57), "We assumed that there was a certain rock bottom of knowledge, the knowledge of the immediately given, which was indubitable... This was the picture which I had given in the *Logischer Aufbau*." This quotation indicates the need for further discussion in order to clarify the Carnap's alleged anti-foundationalist orientation in the *Aufbau*. A sufficient discussion would start by looking at Uebel (2007), Friedman (1992) and Christopher Pincock (2005). If Carnap himself was not clear on his commitments, then the Richardson reading stands in need of defense. However, it can be noted that although Friedman (1992, p. 18) claims that in Carnap's reflections in 1963, "It would be difficult indeed to find a clearer statement anywhere of the assumptions and goals of phenomenalistic foundationalism," he also notes that "when we turn to the text of the *Aufbau* itself such an epistemological conception is hardly in evidence." Carnap, it seems, has an unclear conception himself of his former philosophical project.

Perhaps one could argue that Carnap misremembered the point of his work in the *Aufbau*? As Friedman (1992, p. 40) remarks,

> It is not unprecedented, of course, for the character and motivations of an earlier and now rejected philosophical project to be grossly misdescribed – even by the philosopher whose earlier views are in question.

Nevertheless, Friedman (ibid) suggests, and goes on to argue that, "in the passages in question ... Carnap is describing not so much his own motivations when writing the *Aufbau* but rather the way in which the *Aufbau* was initially understood within the Vienna Circle." Friedman also argues that evidence in the *Aufbau*, regardless of Carnap's autobiographical reflections, indicates his affiliation with neo-Kantianism more than any traditional empiricist programme and its attendant commitment to some version of foundationalism.

But the problem does not disappear. In 1930 Carnap writes the following (1930, p. 144):

> The positivist system 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 inter-subjective knowledge possible*. [Italics added.]

The citation suggests that perhaps Carnap was a foundationalist after all. At least, it raises the question about how Carnap understood the point of his programme between 1928 and around 1930.

While this matter too could do with further investigation, the following response suggests itself. Uebel (2007, pp. 129-134, 190-200, 440-449) has argued, with reference also to Carnap's unpublished manuscripts, that Carnap did suffer a foundationalist 'dip' around this time. Carnap was neither always as unconcerned with epistemological foundations as he was in the *Aufbau*, nor as explicitly anti-foundationalist as he was from 1932 onwards. Rather, according to Uebel, it was in the protocol sentence debate with Neurath that Carnap was gradually weaned, as it were, from his foundationalist sentiments (see Uebel 2007, Chs. 6-8). It is to this debate that we turn next.

Neurath's critique of the *Aufbau*: The protocol sentence debate

The previous section serves to indicate the nature of Carnap's empiricism in 1928. It is not because he ignored the problem of the foundations of knowledge that he takes sensation conventionally and without justification. It is, rather, that he attempts to be clear about the limits of normativity with respect to a constitutional system. No system can be employed to justify its own adoption. Rather, it is in virtue of adopting observation sentences and a formal framework that one constructs a constitutional system that affords internal normative force. Carnap is thus not naïve about empiricism in his attention to constructing an account of knowledge. His methodology clarifies what are taken to be the limits of the domain of application of a constitutional system of knowledge: any knowledge claims must be understood relative to an adopted constitutional system. One might make reference to a theme of this thesis: there is a distinction to be drawn between answering a question regarding our knowledge of the world and formulating a methodology with which to answer such a question. Russell may be understood as engaged in the former ambition, while Carnap can be seen to engage with the latter.

This assessment of the *Aufbau* and Uebel's worries about Carnap's foundationalist dip have implications for how to understand the dynamics of the protocol sentence debate, which centered on how to formulate a methodology with which scientifically to answer questions pertaining to our knowledge of the world. Two key published papers of the debate came out in *Erkenntnis* vol. 2 in 1931, as "Sociology in the framework of physicalism" and "The unity of science" by Neurath and Carnap, respectively.[3] Both lengthy discussions, they give an indication of where each directed his attentions. Neurath attends to a more general account of the way in which various inquiries in Sociology can, and ought to, be formulated in the language of physicalism. Carnap provides what he calls (1934a, p. 28), "an example of application of Logical Analysis to investigating the logical relations between the statements of Physics and those of Science in general." Partly in virtue of their different approaches, they disagreed on whether protocol statements ought to be understood as couched in a primitive, distinct language from physicalism or not primitive and with no different status from other non-protocol statements.

A notable aspect of the emergence of the protocol sentence debate is that it did not involve questions of the justification of the purported connection between the language of sensation and the real world (appeals to notions of the 'real world' could not be meaningfully formulated in the language of physicalism). Neither was it over whether there could be – in principle – a successful reduction of scientific language in general to the language of sensation. Rather the bounds of physicalism extend only to the basic observation statements that can be formulated and the debate centered on the status and structure of those protocols in the *Aufbau*. Moreover, this reduction, as noted,

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[3] The articles are cited as follows. Neurath, O. 1931a. "Soziologie im Physikalismus." *Erkenntnis* 2, 393-431. Translated as "Sociology in the framework of physicalism" in Neurath (1983). Carnap, R. 1931. "Die physikalische Sprache als Universalsprache der Wissenschaft." *Erkenntnis*. 2:432-65. Translated as (1934). *The Unity of Science*. London: Kegan Paul. Friedman (2000) cites these publications as 1932, as does Max Black (1934, p. 10) in the introduction to Carnap's "The Unity of Science," because the last part of volume 2, which is marked as 1931, came out in 1932. Here for the purposes of internal consistency, they will be cited as 1931.

was not the solution to the problem of knowledge but was, rather, the problem itself – at that point yet to be worked out.

The debate takes place within a context of relative agreement among Vienna Circle members and other practitioners of scientific philosophy. Neurath (1931a, p. 58) writes that the Vienna Circle attempts to "further scientific work in all fields by means of logical analysis" in an "atmosphere free of metaphysics." They associate their work with that of Mach, Poincare, Russell and Wittgenstein among others and "agree" that the language of physicalism (the intersubjective language of science) provides a framework for all "meaningful statements." Neurath (ibid, p. 65) notes that his own view is closest to Carnap's.[4]

In spite of this agreement, Neurath indicates the need to specify what it means for all meaningful statements to be given within physicalism. He writes (ibid), "some in the circle" [i.e. Carnap] claim that 'philosophy' is given the task of clarifying concepts within the language of science. Neurath finds this problematic both because it gives rise to misunderstandings (about the status of employed protocols as conventionally adopted – they may be understood as somehow *justified*) and because it puts forward a language outside any meaningful framework, which for him is metaphysical nonsense.

For Neurath (1931a, pp. 60-1), physicalism rules out as meaningless any standpoint outside of science from which to assess science:

> It is certainly possible to speak about one part of language with the help of another part; it is, however, not possible to make pronouncements about language as a whole from a 'not yet linguistic' standpoint, as Wittgenstein and some individual representatives of the Vienna Circle seem to do.

[4] Richardson (1998, p. 10*fn9*) argues that the list of "historical predecessors" to the Vienna Circle is "misleading": "it certainly does not apply in any clear way to Carnap."

For Neurath, one cannot assess "language as a whole" from the perspective of "experiences" or the "world." He (ibid, p. 61) writes, "Every statement of the kind: 'The possibility of science rests on an orderly constitution of the world', is therefore meaningless." He (ibid) writes further that the "possibility of science becomes apparent in science itself.... 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." Finally, he notes that science "remains in the domain of statements" (ibid). There is no further "true system of statements" alongside science as a system of statements "even as a conceptual boundary" (ibid). "This system of statements is that of unified science – that is the standpoint that we can call physicalism" (ibid).

Carnap's phenomenal language therefore cannot be meaningfully formulated outside physicalism as was apparently presumed by Carnap in 1930. That language was put forward as distinct from the intersubjective language of physicalism. Protocols are formulated in the phenomenal language and the problem then was how to deduce the intersubjective non-protocols of the physicalist language from the primitive, basic, phenomenal statements. For Neurath however, statements of subjective experience can be understood only when formulated as intersubjective physicalist statements. For Neurath physicalism must be characterized as a singular (if incomplete) system of statements: there can be no meaningful languages that lie outside the realm of physicalism. Friedman (2000, p. 41) notes that for Neurath physicalism is the "essentially interlinguistic standpoint." And metaphysics is best overcome by abandoning any notion of an external standpoint from which to assess the language of science. Thus, one rejects notions of Platonic forms as meaningful, Wittgensteinian elucidations that would relate language to the "world" or the "given" (ibid), or particular ontological commitments that are intended to explain the successes of science, but can be provided no meaningful formulation within the language of physicalism. Moreover, Neurath (1931a, p. 65) writes, "As the views of this paper are above all close to those that Carnap has expressed, it might be stressed that the special 'phenomenal language' from which Carnap tries to derive the physical one, is discarded here," which will suggest some "alterations" to Carnap's constitutional system. Likewise, there is no use for 'methodological solipsism', which "will probably also disappear; it can probably also be understood as a weakened residue of idealist metaphysics [insofar as it might be understood to be justified based on some sort of privileged access to truth about the world] from which Carnap in particular always tries to keep clear." Neurath argues that methodological solipsism cannot be formulated scientifically (that is within the language of physicalism) on the grounds that it is the bounds and framework of physicalism itself that serve to provide the means to give meaning to statements. Methodological solipsism (ibid) "cannot even be used any longer to give an idea of a certain attitude in contrast to another attitude, because there is only one physicalism. It contains everything that can be formulated scientifically" (ibid). Neurath's chief worry is that although Carnap intends to purge the language of science of metaphysics, the phenomenal language could be understood to be metaphysical, since it is assumed in the *Aufbau* to lie outside intersubjective physicalism.

One might be worried about Neurath's insistence that all meaningful statements are formulated within physicalism and that consequently metaphysics is rendered meaningless in virtue of its concepts not being so formulable. An example from popular culture might provide a helpful way to think about what this point means to Neurath (and, broadly speaking, Carnap). Although one may not feel compelled to share their view, it may demonstrate the way in which their position is motivated.

A few years ago the popular English band Keane produced an exquisite music video that starred Giovanni Ribisi. (One need neither know the band nor their cultural sphere to grasp the point of this example.)[5] In it a young family man leaves for work in the morning. His clothes are comfortable. His car is practical. And he seems to share an idyllic suburban life with his beautiful wife, who reminds him to pick up the dry-cleaning, and clever son, who apparently entertains him in the drive to school and work. Entering his office, where he

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[5] For the initiated, the Keane song is entitled "Crystal Ball" and may be found presently on Youtube: http://www.youtube.com/watch?v=uJ0z6g6BLj8.

appears to work as a letting agent, he greets his colleagues, flirts with the receptionist and goes to work surrounded by photos of his family.

After a difficult day, his idyllic life appears to unwind. He returns home to find a strange car in the driveway. When he tries his key in the lock it does not work and when his wife answers the door, she does not know him. Slightly alarmed, she calls to her partner, who looks rather like the protagonist of the story, and they shut the door. He calls to his son through the window and the boy does not know him. The police subsequently arrive and one hears the protagonist protesting that he has been married to the woman for seven years, but she is pretending she does not know him. Feeling ill at ease, he drives around aimlessly, not knowing where to go or who to call, eventually sleeping in his car.

In the morning he returns to work to find the receptionist does not know him and the man from his house is seated at his desk. The same photos around the desk have no trace of the protagonist, but in his place is the other man. His behavior, which up to this point has seemed rational, becomes erratic and he is eventually thrown out by security. Walking to the car park, another woman drives off in his car and he is left in the parking lot rather confounded.

Thematically, the plot runs like an episode of the Twilight Zone. All experiences he held to ground his reality come unraveled in the face of subsequent events. Of course, one concludes that the man is in the end delusional, but the relevant question for present purposes is on what grounds does the viewer come to hold this conclusion? If one starts from the experiences of the protagonist at the beginning, all seems normal until it is not. There would seem to be no grounds on which to make sense of the change in subjective experience and the viewer is left bewildered with the protagonist. It is not until one confronts all the other various accounts of the situation that meaning can be ascribed to the experience. Not just statements of the experience of the protagonist to the police officer, but also those of the wife, son, husband, neighbors or alleged work colleagues need to be considered to give meaning to the experiences of the protagonist.

Indeed, it is precisely in virtue of the broader context of statements about the experience that one can ascribe meaning to the protagonist's predicament at all. His own subjective experience cannot give meaning to what is happening to him until it is formulated against the other body of statements. That body of statements includes, not just testimony of others, but home ownership and employment records, the man's own history (if he has one) with police and possibly mental health facilities, etc. To be sure, it was only because his own experiences fit with his surroundings at the beginning of the story that viewers assume there is any coherent picture to unravel in the first place.

Moreover it cannot help to stipulate that the man must be of sound mind in order for his subjective experiences to be given meaning. For one cannot assess what it means for him to be of sound mind without appeal to this broader sphere of meaningful accounts of the experiences.

This example indicates the way in which Neurath and Carnap can be understood to share a commitment to physicalism (and the way in which their consequent debate was over how protocol statements are understood to fit within physicalism). Certainly each individual has 'basic experiences', but those experiences cannot be given meaning until couched within a shared intersubjective language wherein one can formulate and explicate concepts and statements derived from them (to a greater or lesser extent). It helps nothing in the example to suggest that the grounds for the verity of the protagonists' experience are that such experience is caused (or not) by 'reality'. That concept cannot even have any meaning until assessed against the body of statements within which it is being asserted.

The left wing of the Vienna Circle does not rule out metaphysics based on an ad hoc definition. Rather, its members intend to make clear that metaphysical concepts cannot be rendered sensible within an intersubjective language. Thus one cannot meaningfully formulate the concept 'reality' within physicalism: any talk of reality can only consist in all the statements formulated about it. This is physicalism in the widest sense: not that everything is physical, but every meaningful reference to objects constitutes the ever-growing domain of physicalism, outside of which one cannot assert meaningful access. The subjective nature of the protagonist's experience therefore cannot be given meaning until it is understood against statements surrounding it. Thus, the protocol sentence debate was not over the epistemological justification of basic observation statements as a foundation for science. It rather focused on the form and status of conventionally adopted protocol statements. For Neurath, protocols could not be understood as primitive or rooted in 'subjective' experience, the language of which is external to the intersubjective linguistic framework dubbed physicalism.

What are protocols? Two points can be noted for Neurath. First, protocol statements are basic observation statements, formulated as part of the intersubjective physicalist language, and are the basis for the construction of that language. Second, to determine whether protocols should be accepted, they are compared against the body of statements consisting in the language of physicalism. This is to say that a statement about a basic observation like, 'someone sees a red cube on the table', is checked against other accounts of the cube and corresponding behavioral dispositions that are taken to indicate assent to the claim that 'someone sees a red cube on the table'.

Protocols are the basis on which scientific statements may be made. Since there can be no subjective language outside physicalism, protocols themselves need to be formulated as a part of the language of physicalism (Neurath, 1931a, p. 64): "From the start the perceiving subject will be more closely linked with the perception statement and the object determination than was done before." One cannot separate the "thinking subject" or the "ego" from anything that can be "experienced;" thus, he (ibid, p. 65) writes,

> What we try to detach as 'ego' includes, in the language of physicalism, these processes about which we are not informed through the usual

> 'external' sense. All 'personality coefficients' that distinguish one individual from another are of a physicalist nature!

So, one cannot make reference to *subjective* experience as a foundation for the physicalist framework of knowledge. Specifying the role of the perceiver is of central importance to Neurath's account of protocols, but "perceivers'" protocols are not compared from one subject to another: rather protocols must be formulated from the start as physicalist statements if they are to have meaning at all.

Two interpreted examples of basic observation statements might be the following:[6]

> "The red cube sits on the table."
> "I am hot."

For Neurath there can be no neat, atomic rendering of a protocol statement because any such statement cannot be given adequate translation rules into statements actually made within science. On these grounds there can be no phenomenal language. The examples are incomplete because all protocols must include reference to a perceiver and cannot refer to a subject. This is to say that the appropriate form of the protocol should read,

> Otto states that, 'the red cube sits on the table'.
> Otto is hot.

Further, Neurath says that to be more precise, one might require statements about "Otto" and the time at which he reports the protocol. Thus one may write,

> At time t, the person identified by such and such features and called, 'Otto', reports that at t-1, he stated, 'the red cube sits on the table'.

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[6] For a Neurath's discussion of these examples, see Neurath (1932, p. 93-4).

> At time t, the person identified by such and such features and called, 'Otto', reports that at t-1, he stated, 'Otto is hot'.

This reads as a rather flat-footed account of the structure of protocols to be sure. 'I am hot' is less muddled than Neurath's rather weighty account, while the meaning of the simpler expression seems clear. However the former includes reference to a subject, which suggests, that there is some independent subjective experience forming a basis point. In place of the subject, Neurath makes reference to the name of the person, so that one can compare protocols. The statement 'Otto is hot' can be compared with 'Karl is hot'. Each of the constituents can be further analyzed. Neurath's 'I' on the other hand cannot be compared with Karl's 'I' (see Neurath 1932, p. 94).

As formulated, an adequately specified protocol sentence therefore must include aspects of the broader context within which the statement is made. This precludes any notion that protocols, as the basis for scientific language, can be reduced to primitive – simple and atomic – statements. Neurath's own account thus moves in the opposite direction from Carnap: not toward a neatly specified singular statement, but toward a more tangled account that demands one incorporate the role of the perceiver and other contextual elements into any account of a basic observation statement. Protocols in this sense have no special status over non-protocols; they are selected as a conventional starting point for the construction of a Carnapian constitutional system. Further, this feature characterizes Neurath's notion of *Ballungen* as a central aspect of any account of scientific statements.[7] That is, no observation is primitive, but rather consists in a messy cluster, or congestion, of notions built in. One may always further elaborate or change the context of a protocol sentence in virtue of the fact that there are innumerable levels of specification that one may be inclined to

[7] The concept of *Ballungen* is introduced explicitly in Neurath (1932), but its elements can be seen here and it therefore provides an effective way to account for Neurath's point. An extensive discussion of *Ballungen* and its role in Neurath's account can be found in Cartwright, N., Cat, J., Fleck, L. and Uebel, T. (1996, pp. 167-252).

articulate when accounting for the context of a perceiver's statement of any given protocol.

Carnap's reply to Neurath, "The unity of science," was published in 1931 in *Erkenntnis*, followed in the next volume in 1932, with "On protocols." Friedman (2000, p. 42) notes that in 1931, Carnap held still that there were two languages: the phenomenal language of subjective experience and the intersubjective physical language, while the epistemological problem is how to move from the former to the latter. The former do not require confirmation for Carnap, but function as the basis for all other scientific statements; at this stage they still "express the epistemological point of view of a single subject." The physical language is intersubjective and serves as "a universal language for science," since all statements can be expressed within it (ibid, p. 43). Key for Carnap, as a response to Neurath's worries, is that protocol statements now fall under the physicalist language as "sub-languages" (Carnap 1931, p. 88),[8] thereby removing, according to him, the point of contention with Neurath.

Richardson (1998, p. 198) notes that there is a substantial distinction between Carnap's programme in the *Aufbau* and that in "The unity of science," wherein he "employs a new philosophical perspective," drawing a distinction between material and formal modes of speech. The latter, which he now takes up, consists in statements about language, while the former – employed in the *Aufbau* – is about objects. Philosophy thus becomes solely about language. According to Richardson this sharpens his ability to reject metaphysical statements. Since any meaningful statement must be formulable in the formal mode, metaphysical statements like (ibid, p. 200), "numbers are essentially constructs of the human mind" or "the natural numbers, but not the rational numbers, really exist" cannot be given meaning, since Carnap doubts that they can be given formal expression.

Friedman (2000, p. 43) notes that although Carnap in "Unity" argues that the phenomenal language is now to be viewed as a sub-language of physicalism,

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[8] See Richardson (1998, p. 201).

"From Neurath's point of view... Carnap has still not understood his point," since the latter still refers to two distinct languages. Neurath (1932) is an explicit reply to Carnap's views in the "Unity" paper and presents his worries not just that a language of subjective experience cannot lie outside the intersubjective language of physicalism, but further that Carnap's move toward the logical analysis of concepts cannot be executed because the starting point – the form of any protocol statement – cannot be a neatly reduced atomic statement from which one can employ logical procedures to deduce empirical scientific statements.

Neurath writes (1932, p. 91) that scientific statements are becoming more precise, but "No term of unified science, however, is free from imprecision, since all terms are based on terms that are essential for protocol statements whose imprecision must be immediately obvious to everyone." The notion that there may be an ideal language consisting of "neat atomic statements" is a metaphysical fiction and scientific language cannot be seen to be an approximation to such a language. In scientific practice, one is not given neat, precisely specified observation sentences. Any statement about observation involves necessarily imprecise elements. Rather (ibid, p. 92), "what is first given us is our historical ordinary language with a multitude of imprecise, unanalyzed terms [*'Ballungen'*]. We start by purifying this ordinary language of metaphysical components and thus arrive at the physicalist ordinary language." Although Neurath supports the employment of logic to clarify the structure of "physicalist ordinary language," he argues that a strictly formal representation of natural language statements is not possible because the language used in scientific practice is imprecise. Carnap's formal rendering of protocols therefore cannot be understood to be about any scientific statements that are actually employed in science.

Neurath (ibid) argues thus that a unified language of science utilizes, in practice, terms of ordinary and scientific language, which means that one can never be rid of the imprecision that comes with the necessary inclusion of ordinary language in statements of a unified scientific language. He characterizes his physicalist language as a 'universal jargon', the key for which is that it is purged of metaphysics. It cannot be purged of imprecision because the unified language of science, employed as a 'universal jargon', always consists of both theoretical and ordinary statements.

Referring to Carnap's discussion of 'primitive' protocols that "require no verification," Neurath (ibid) argues that:

> Unified science consists of factual statements. These are either protocol statements or non-protocol statements. Protocol statements are factual statements of the same linguistic form as other factual statements, but in them a personal name always occurs several times, in a definite connection with other terms.

Thus, Neurath indicates that the worry now is not just whether the phenomenal language lies outside physicalism (since by now Carnap has renounced this notion), but that Carnap still wants to suggest two distinct languages, one of neat, atomic protocols and the other of physicalism, consisting in ordinary and theoretical statements. Protocols in the form outlined by Carnap still may give way to metaphysics on the grounds that they may be taken to provide an ultimate foundation from which to build an account of the world. Protocols have the same imprecise status as any other statement in science. They are used as the basis of a systematic account of science and based on a decision – and necessarily may be always further specified through a more thorough descriptive account of the protocols. They may also be rejected.

In spite of the persisting disagreement and apparent mounting tension in the debate, Neurath's tone still concludes in a conciliatory manner (ibid, p. 99):

> What matters in all scientific work is to establish harmony between the statements of unified science: protocol statements and non-protocol statements. For this purpose a 'logical syntax' is needed, which is the

> main issue of Carnap's work; Carnap has created the first preparations for this in his [*Aufbau*].

One can see here Neurath's receptive nature to Carnap's own emerging development away from epistemology to the logic of science. Though he rejects any notion that a study of syntax can bring about a non-protocol language from primitive protocols, he endorses the place that the systematic analysis of Carnap's project has within physicalism. Thus, as the debate becomes more pointed, thereby indicating differences, it may also be seen to suggest the need to more fully articulate Carnap's developing distinction between material and formal modes of speech, which eventually becomes a distinction between psychological and logical aspects of science. The full expression of that distinction, it will be argued below, is both necessitated and motivated by the dynamics of the protocol sentence debate as it develops: both Carnap and Neurath's views require such a distinction to vindicate their viewpoints. Logical aspects are developed and employed methodologically to bring clarity to the structure of scientific language.

The shift in Carnap's view can be seen both in response to Neurath's worries and to his own confrontation of problems that emerge within the *Aufbau*. The latter is considered in the next section. Carnap (1932) develops his line of thought in response to Neurath. He provides a behaviorist account that drops reference to the "first-person epistemological point of view" altogether (Friedman 2000, p. 43), thereby addressing Neurath's worries about appeal to different subjective protocol languages. Yet Carnap still suggests one legitimately might construe the language of science with one of two *forms*: 1.) His suggested form in which language of protocol sentences is distinct from physical language, which requires rules of translation for physical language into the protocol language; or 2.) Neurath's proposal in which protocol-sentences belong to the physical language, thereby making explicit that protocol sentences are not epistemically privileged.

Carnap notes that in the end Neurath's view is to be preferred because it precludes any "residue" metaphysical notions like the "given" etc., but he has not been willing to drop the development of the concept of a distinct protocol language because it stands in accord with his attempts to formulate the intersubjective language in more precise terms. Moreover Carnap's emerging "principle of tolerance" indicates that a systematic account of the language of science in the form of two languages ought not be rejected based on an assertion from the beginning, but ought to be considered in terms of whether it is pragmatically fruitful to adopt it or not. Friedman (2000, p. 45) translates Carnap:

> Not only the question whether protocol-sentences occur outside or inside the system-language, but also the further question of their more exact characterization, is to be answered, it seems to me, not by an assertion, but rather by a convention. Although I earlier left this question open and only indicated a few possible answers, I now think that the different answers do not contradict one another. They are to be understood as suggestions for conventions with regard to their consequences and in testing their practical utility. (Carnap 1932, p. 216.)[9]

Neurath (1932) still contrasts his view from Carnap's, but Carnap's response (1932, p. 457) is, "My opinion here is that this is a question, not of two mutually inconsistent views, but rather of two different methods for structuring the language of science both of which are possible and legitimate." He (ibid, p. 458) holds that the protocol form "affords greater freedom," since it allows one to select protocols of whatever form, while the Neurath's physicalism, "has the advantage of greater unity of system." The protocol form (ibid) has protocol sentences "outside" the "system" language and their form is "arbitrary," while translation rules will be constructed for protocol sentences into the system language. For the physicalist language form, protocols are found within the system language and are "bound" to its syntax. Moreover (ibid), "[t]he questions

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[9] This view can characterize the present proposal for how to understand the point of Hempel's D-N account: it is not assessed as right or wrong, but with the pragmatic consideration of how effectively it explicates explanatory candidates.

of whether the protocol sentences occur outside or inside the system language and of their exact characterization are, it seems to me, not answered by assertions but rather by postulations." This comes to be a central distinction between Carnap and Neurath, but, as will be suggested, it appears to be a methodological difference, not a deeper philosophical one. Neurath rejects the primitive protocol form outright. Carnap prefers to postulate one view or the other and assess them based on their pragmatic benefits.

Further, Carnap (ibid, p. 464) considers the practical application of Neurath's physicalist view and presents for the formulation of the protocol language two options:

> A) with restriction: it will be postulated that concrete sentences of such and such completely specified form shall serve as protocol sentences; B) without restriction: it will be specified that any concrete sentence may be taken under circumstances as a protocol sentence.

Neurath chooses the first option, which precludes the potential for various possibilities of the protocol form. As noted, Neurath suggests that the "name of the protocaller" should be included. Carnap remarks that this *seems* a practical choice for the first option, but that its practicality in the end is doubtful because "it has the defect, from the point of view of syntax, that a sentence which refers to another contains the other as a clause." Here one can identify part of Carnap's motivation for wanting to construct a distinct protocol language: Neurath's formulation of a protocol sentence exhibits a syntactical problem by containing the statement to which it refers. Thus it would appear that the sentence can be reduced further to the clause it contains: 'Otto states, "there is a red cube on the table"' might be further reduced to 'there is a red cube on the table'. Reducing the sentence to its principle clause may function to clarify the structure of the protocol further.

Carnap (ibid) is sympathetic with Popper, who is inclined to advocate option B, that *any* concrete sentence (and not merely Neurath's idiosyncratic formulation) may be a protocol. Every concrete sentence can serve as a protocol in given circumstances. Suppose one employs the statement, 'L is a law' ("universal sentence of the system language"). Concrete sentences relating to "specific space-time positions" are derived from L. That is, the statements derived from L have to have specific space-time positions. One can derive from concrete sentences, in conjunction with other laws and "logico-mathematical inference rules," more concrete sentences "until one arrives at sentences one wants to admit in the case immediately at hand. Thereby it is a matter of decision which sentences one wants to use at various times as such endpoints of reduction and thus as protocol sentences." One is not forced (ibid, p. 466) to stop at any one place: "From any sentence one can reduce still further; there are no absolute initial sentences for the structure of science."

Thus Carnap views his protocol language and Neurath's version of physicalism as suggestions for conventions on which to structure the language of science. They are not assessed in terms of being right or wrong, but in their practicality of application. Carnap concedes that Neurath's view, wherein protocol sentences are not distinct from the system language, is perhaps to be preferred because it avoids "idealist residues" and the worries of metaphysics brought up by Neurath. However he suggests that Popper's view of the application of physicalism is to be preferred to Neurath's in practical terms. The elimination of absolutism is taken by Carnap to be the shared starting point of the discussion, which for him diffuses some of Neurath's worries in his suggestions regarding a distinction of phenomenal language from the physicalist system language.

The protocol sentence debate was not over justifying sensation as a foundation for knowledge. Rather it was over what form a methodology for the assessment of the language of science might take, given sensation as a starting point. Within the debate, that methodology assumes a class of protocol statements that can provide a starting point for the assessment of scientific knowledge – indeed, a methodology for formulating grounds for the possibility of knowledge relative to the framework within which scientific statements are made. Those statements, it is agreed between Carnap and Neurath, are selected on the basis of convention in practice. There is no question of the justification of protocols because they are adopted on the basis of a decision. The question then that is central to the protocol sentence debate is what form the protocols should take and how the methodology shapes up in light of an adopted system of those protocols and the adopted formal framework specifying their relations both to other protocols and non-protocols within the system.

Carnap and the rejection of epistemology: Internal problems within the *Aufbau*

There is a substantial philosophical difference between the *Aufbau* and the 1931 "Unity" paper, where Carnap introduces the distinction between 'material' and 'formal' modes of speech. The former is about objects, while the latter is about language and indicates Carnap's move to develop his methodology toward an analysis of language – not of objects. According to Richardson (1998), the epistemological problem of the *Aufbau* – how to get from subjective experience to intersubjective concepts of science – is thus reformulated in the formal mode as (p. 199), "[t]he terms of physicalist language must be defined in terms of a language of primitive experience [protocol language]." The epistemological problem has now become the extent to which one can specify the relation between two languages, not the relation between subjective experience and intersubjective physicalist language. Richardson (ibid, p. 201) notes that "protocol languages are meant to capture the given in experience in its given form," which leads Carnap to hold that "there is a fact of the matter" about the structure of protocols.

According to Richardson, there are three ways in which Carnap in 1931 sees the protocol language as subjective (ibid): first, each individual has her own language; second, each language is distinct from the physicalist language; third, once translated via rules to the physicalist language, protocol languages are taken to be sub-languages of physicalism. The question then is how to translate protocol languages into physicalism and that translatability is based on "the fact that each agent can discover inferential connections" between the two languages (ibid). Richardson notes that (ibid, p. 202): "Carnap now stresses that it is simply a matter of fact about us that our protocol languages and the protocols that we actually endorse have the right structure for this process to go forward." That is, "ordinal properties" of protocols that are "independent of the agent" provide the necessary connection between physicalist and protocol languages. There is a structural connection and, moreover, an empirical fact of the matter.

Richardson's account now brings out a central tension in Carnap's thought at this stage: a tension which leads to the distinction between psychological and logical aspects of analysis. Carnap now argues that two empirical facts ground the possibility of "objective science" (ibid, p. 203): that agents discover inferential connections between protocols and that there is a fact of the matter of the structure of protocols that allows one to make those inferences. But if the epistemological problem, as assumed by Carnap, is to articulate the conditions for the possibility of empirical knowledge, it cannot suffice to appeal to empirical claims, which surely themselves must be in question. Since Carnap's epistemology is logic-centered, it cannot appeal to empirical facts to ground it: "if we can simply cite empirical matters of fact as answers to epistemological questions, then any and all motivation for Carnap's logical version of epistemology is lacking" (ibid, p. 204). The solution for Carnap, of course, is not to follow Quine. Rather, this problem indicates the ambiguous nature of the epistemological project from the *Aufbau* onward (ibid):

> Throughout the work from the 1928 project to that of 1932, Carnap has been trying to combine the logical, the epistemological, and the psychological. The structure of experience is conceived of as a logical structure, and hence the philosopher can and must use the tools of modern logic. But this structure is also meant to provide the key to the answer to a general epistemological question about the possibility of objective knowledge.... Finally, it is meant to be a psychological structure and to be uncovered in the researches of psychology.

The problem is that Carnap answers the epistemological question regarding the possibility of empirical knowledge by emphasizing the structure of experience. Epistemology then consists in the logical assessment of such structures to formulate the links between protocol languages and the physicalist language. But the structure of experience is known only empirically through psychology. And the question of the possibility of knowledge – if epistemology is a logical discipline – cannot be answered by appeal to empirical facts of psychology. According to Richardson (ibid, p. 205), the problem, Carnap comes to realize, is that to speak of the logical structure of experience is to conflate epistemology, logic and psychology.

Moreover, given Carnap's distinction between formal and material modes of speech and his notion that epistemology is formal, empirical aspects of protocols – i.e. reference to "experience" – cannot be part of the epistemological programme. Epistemology must have nothing to say about experience and therefore cannot, by Carnap's formulation, have anything to say about protocols. Carnap's solution, according to Richardson is to drop the project of epistemology (ibid, p. 206): "the project of the reconstruction of the sciences in formal languages can and should go forward," but it is not motivated (as it was in the *Aufbau*) by the epistemological concern of moving from "subjective experience to objective knowledge." Further, Richardson writes (ibid): "In the end, he cannot succeed in finding a place for an epistemology separate from metaphysics, logic, and psychology. Metaphysics is rejected; psychology is left to the psychologists. What is left to the philosopher is the logic of science." By the mid-1930s Carnap sees the project of philosophy as the logical analysis of languages.

One thus can characterize two features of the development of Carnap's thought. First, he responds to Neurath's worries about the status of protocol sentences: he eventually drops reference to both a phenomenal language that falls outside physicalism and any reference to a subject. For Carnap, this move both appears to address key worries that launched the public stage of the debate and to push the developing tension with respect to Carnap's desire to distinguish between formal and material aspects of the structure of science. Second, he addresses those internal tensions within his programme indicating the way in which, given the 'indispensible' distinction between logical and psychological aspects of a logical empiricist methodology, he finds it necessary to reject epistemology altogether, which he takes to conflate the two.

Thus Richardson argues (ibid) that by the mid 1930s, Carnap had rejected epistemology "in no uncertain terms." At the Paris Congress in 1935 Carnap outlines three stages in the philosophical development discussed in the Vienna Circle: first was purging the language of science of metaphysics; second was the move to epistemology as the problem of how to account for the connection between subjective experience and intersubjective language. This "involved a rejection of the synthetic a priori and the consequent adoption of empiricism in epistemology." Third is the move from epistemology to the logic of science (ibid).[10]

This third stage for Carnap (ibid) is to "purify" epistemology and classify its "constituent parts": the psychological and the logical. According to Carnap, former projects of the Vienna Circle, and notably his own *Aufbau* can be characterized as having conflated these parts. The new characterization of his philosophical programme is the logic of science, which examines the logical connections among statements within adopted language forms. Philosophy for Carnap becomes the logical analysis of any specified language forms.

Carnap had seen the success of logic in bringing clarity to languages. Now he wants to "develop a method for the construction of ... sentences about sentences" (Richardson ibid, p. 209). The point of the formal syntax language is to "provide a precise tool for defining important logical notions such as 'logical consequence' or 'analyticity' for the object languages under consideration" (ibid). Such definitions provide the means to distinguish between aspects of language that follow from the rules of the framework and content statements made with the framework (ibid).

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[10] See Carnap (1936a, p. 36).

Richardson states (ibid) that distinguishing between types of statements is necessary to understand the "rational structure of science." It indicates which parts of scientific language give the logical framework for meaning within that framework. Once the framework has been adopted, one then can – and only then – examine content statements within the framework to see if they are supported by evidence, which itself will be, "sentences expressed in that language." Richardson writes (ibid),

> Thus on Carnap's view, any question of the justification of (or the rationality of belief in) a sentence requires a linguistic system within which that sentence is couched and that provides the inferential relations requisite to make sense of claims about justification or confirmation.

Questions of the justification or confirmation of a statement thus are raised within an adopted framework. One can read Carnap's conventionalist approach to the adoption of one framework or another: with respect to a class of basic observation statements, no question of the justification of the employment of those statements can arise until one specifies the framework which gives the rules of their employment. No epistemologically relevant question can arise for Carnap's methodology regarding whether those are the 'right' statements to use as a starting point. However this does not amount to a naïve scientism. For Carnap, philosophy constitutes the analysis of the structural relations among observation statements and higher-level statements in the edifice of physicalism. Clarification can indicate the extent to which the constitutional system in question is pragmatically viable.

Moreover, Carnap's principle of tolerance holds that the logic of science may use any number of object languages and not just the one actually employed in science. Richardson (ibid, p.209) cites Carnap's account of that principle from Logical Syntax (1934b, p. 52):

> *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.

Thus, Richardson (ibid, p. 210) writes that "the standpoint of a formal syntax language" is that from which one specifies employed syntactical rules, as opposed to rendering a philosophical argument. This highlights a key to the distinction employed in the present thesis between answering questions of knowledge with philosophical arguments and specifying a methodology with which to answer such questions. Given this distinction as demonstrated in Carnap, he may remain ambivalent about which syntax, or formal framework is employed. He merely requires that any such framework be specified. Richardson writes (ibid), "As such, the standpoint of logical syntax provides an infinite variety of new projects in logic for Carnap's bold antimetaphysical and amoralist logicians."

Thus there is no question of the "correctness" of a given logical language as assessed against some logic of the world. One may adopt any formal system desired so long as the syntax with which one makes claims is specified. The correctness of any claims made within a particular logical framework will be relative to the syntactical properties specific to it.[11]

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[11] One might wonder why one should not think that some structures will not admit concrete sentences, but nevertheless can correctly describe the world – i.e. why say *any* will do rather than 'more than one may work'? However, Carnap's claim that any formal framework will do does not preclude the notion that formal frameworks absent content statements may correctly describe the world once applied to it: it is not clear to Carnap at this stage the extent to which formal models might afford empirically adequate statements. But this locates the problem with which Carnap is engaged: how far can formal developments fruitfully account for the world. The formal frameworks themselves might be developed in order to clarify less precise notions like "correctly describe the world." The formal tools are enlisted to render such notions precise, but formal developments are not constrained by empirical considerations every step of the way.

Tying this development to Carnap's position in the protocol sentence debate around 1934, Richardson notes that it leads to certain features of the changes in Carnap's position. First (ibid, p. 211) he has "dropped his attempt to express the motivating epistemological point of the relation between the protocol language and the language of science as showing the relation between the subjective and the objective." Second (ibid),

> There are no facts about the structure of experience to which the protocol language is meant to be true. Thus, in Carnap's eyes, the protocol sentence debate is no longer a debate that involves any facts whatsoever. There is no fact about how the protocol language must look. Rather it is a matter of proposals for formal languages within which to cast the findings of the sciences and thereby render the question of the confirmational status of scientific claims a genuine question.

Thus (ibid) the structure of protocol sentences need not be constrained by "material investigations." Carnap is sympathetic with Popper's proposal merely to select a class of physicalist propositions for any given inquiry that may serve as protocol sentences. The question of the correctness of the propositions as related to the structure of material claims does not then arise for Carnap. He considers there to be pragmatic advantages to it (ibid, p. 212): one need not specify a distinct protocol language outside the physicalist language and it is relatively simpler than Neurath's rather more bewildering account. Richardson writes further (ibid, p. 213) that the,

> reconstructive task cannot possibly wait for and rely on an empirical account of the structure of human experience (or the nature of the primitive language we first learn). The independent epistemological notion of experiential structure is nonsense; the empirical notion is beside the point.

Given Carnap's position on the logic of science, Richardson (ibid) remarks that it was understood by some contemporaries to be an abandonment of empiricism, but for Carnap the opposite was the case: "Only with his adoption of the syntactic perspective does Carnap fully endorse 'logical empiricism' as an appropriate moniker for his view." His assessment of syntactic languages is not based at all on empirical research, since only logico-mathematical resources "are of particular interest to him" as "he is principally interested in these languages as candidates for syntax languages, and only mathematical richness matters for that." Empiricism for Carnap thus is not a "thesis expressed in some language or other ... [nor] a universal constraint on the possibility of well-formed language. It is, rather, a proposal to use certain languages as the languages into which to cast empirical science." Carnap (1937, §27) writes, "As empiricists, we require that descriptive predicates are not to be admitted unless they have some connection with possible observations, a connection which has to be characterized in a suitable way" (in Richardson 1998, pp. 213,4).

The requirement for an empiricist notion of the suitable connection is that (ibid, p. 214) "primitive predicates of the language be observable." An observable predicate is not a syntactical or semantic predicate, but is drawn from empirical science – in particular in this case from behavioral psychology. 'Observable predicates' provide "the starting point for confirmation – the basic sentences of these predicates are accepted independently of any other sentences" (ibid). 'Confirmation', then, is defined "on the basis of observation" (ibid). A statement is confirmable when it can be reduced to a class of "observable predicates."

Comparison and contrast of Neurath and Carnap, 1932

The above discussion moves beyond current considerations of the protocol sentence debate in 1932. However, it indicates the direction that Carnap moves and provides material to consider in examining both the points of convergence and the important distinctions between Carnap and Neurath's views. After the 1931 Unity paper, Carnap had begun to move toward his construal of the logic of science.

Friedman (2000, p. 45) notes that the 1932 discussions are precisely where one can locate the "fundamental difference" between Neurath and Carnap's "antimetaphysical stance." The debate itself develops into a question, not just of the status of protocols, but of the approach one takes to the problem of metaphysics. In Carnap's case, one overcomes metaphysics by adopting (ibid), "the metalogical standpoint of logical syntax... relative to which a plurality of alternative forms for the total language of science is possible and legitimate." For Neurath, one does this by not presuming to go outside the universal language of physicalism. For Carnap, the logic of science (ibid),

> is a fully precise and rigorous subdiscipline of mathematical logic, where our task is not to describe actual linguistic behavior... but rather to investigate in a fully precise way the consequences of adopting one or another proposal for logical form of the total language of science.

However it is worth clarifying the way in which the views of Carnap and Neurath can be distinguished for it is important to emphasize that though Friedman locates the 'fundamental difference' in their anti-metaphysical views, that distinction is not necessarily 'fundamental'. That is, it is not a fundamental philosophical difference as much as a difference in the methodological approach to formulating a language of science purged of metaphysics. This, of course, will not be to obscure that there are indeed differences between Neurath's naturalism and Carnap's logic of science, but conceiving of their positions against the backdrop of Carnap's emerging distinction between psychology and logic indicates that they may be viewed rather as different aspects of the same project. And this will inform an understanding of the subsequent trajectory of Hempel's thought, since Friedman (ibid, p. 39) notes Hempel's later 'turn' from Carnap's project to some form of Neurath's in his later life. Given the distinction at hand between such aspects, that turn may not seem so substantial, which provides a less disjointed account of the corpus of Hempel's work.

Thus, it is worth questioning Friedman's brief rendering of the distinction between Carnap and Neurath's antimetaphysical stances. Friedman (ibid) suggests the following:

> Carnap thus hopes to overcome traditional metaphysics by reinterpreting its 'theses' as logico-linguistic proposals. Neurath, by contrast, will have none of this reinterpretive project but aims rather at a complete dismissal of the metaphysical tradition on behalf of empirical science

Carnap's proposal that the formal analysis of language would give form to any range of protocols might entail that 'metaphysics' can be eliminated by reinterpreting its theses in a formal framework. However, Carnap's stance, as has been indicated, is far stronger, since he is skeptical that any such reinterpretive project is possible. Metaphysics can find no place in the language of physicalism, since its theses cannot be given a formal representation.

Moreover, it will be helpful to be clear on Neurath's view, since he does not propose to dismiss metaphysics "on behalf" of empirical science, as Friedman puts it, so much as to reject any notion that statements lying outside the language of physicalism (which gives the framework for meaning to the statements of empirical science) can be given meaning. As Neurath (1931b, p. 59) writes, "No new world-view is contrasted with an old one, nor is some old world-view replaced by clarification of concepts, but rather now 'science without a world-view' confronts all world-views". Which is to say, that one does not weigh the language of science against metaphysics and opt for empirical science, as might be understood from Friedman's formulation. Rather, with the framework of physicalism one confronts world-views in order to determine whether they provide meaningful factual statements: one uses the framework to give an account of the possibility of knowledge – knowledge that is understood relative to the framework within which it is couched.

It is possible now to provide some comparison and contrast between Carnap and Neurath's views. This chapter began with a survey of Carnap's

Aufbau and follows with the dynamics of the protocol sentence debate. The traditional understanding of the point of the debate obscures what was at stake for its participants. The point of the debate was not to argue that scientific knowledge might be justified via logic on the basis of a reduction to an epistemically privileged language of subjective experience. Rather, the question centered on the very possibility of objective knowledge, given the starting point of statements about experience. Since the *Aufbau* provides the motivation for the discussions in the first place, it has been important to indicate the way in which the received view of its intentions misses the point of the epistemological project. The language of sensation is taken to be the conventional starting point for any empiricist epistemological project – the question then being whether and the extent to which one can formulate connections between subjective experience and the intersubjective language of science.

This feature of the *Aufbau* indicates the following. The protocol sentence debate was not over the justification of the language of sensation as a foundation for knowledge: that role is taken based upon conventional decision. Neither was the debate over whether one may successfully reduce all of science to basic protocols – this point was taken to be open and answerable based upon further research. Rather, the debate centered on how to formulate a methodology with which to account for knowledge – on the form of conventionally adopted basic protocols that serve as a basis for science. It then raised the question about the extent to which tools of logic could connect protocols to non-protocols.

Neurath's principal worry about protocols is that Carnap, in the *Aufbau* attempts to formulate two languages: the phenomenal (subjective experience) and the physicalist (intersubjective science). For Neurath there can be no meaningful sentences outside the language of physicalism and any such attempt smacks of traditional metaphysics, which must be rejected from the start. The form of protocols therefore cannot be taken to be independent from one individual phenomenal language to another and neither can they be distinct from the language of physicalism.

For Neurath, an assessment of protocols begins with ordinary statements purged of metaphysics. Ordinary statements are always given to ambiguity: they consist of clusters of precise and imprecise elements (*Ballungen*). Scientific statements are ordinary statements that are rendered more precise, but all protocol statements consist of both scientific and ordinary elements, which means that no protocol can be primitive or atomic. Ambiguity always remains, but Carnap's attempts to formulate protocols as precise atomic statements obscures this fact and thus is not a tenable means of building up from protocols to higher level scientific statements. Moreover, suggesting that protocols are primitive and atomic might give way to metaphysics since they could be taken to be irrefutable building blocks for science. There simply can be no irrefutable or unrevisable starting points for a language of science purged of metaphysics.

Carnap's view demonstrates some significant changes that bring his view closer to Neurath's (and in his mind puts some of their issues to rest). In the *Aufbau* he stated the domain of epistemology as the problem of moving from subjective experience to an intersubjective language. By 1931 after three years of discussions in the Vienna Circle meetings, he published (in the same issue as Neurath's first publication on the issue) "The unity of science" wherein he comes to suggest that phenomenal languages can be viewed as distinct from physicalism, but as sub-languages within it. In 1932 he removes reference to the first-person point of view, indicating a further concession to Neurath. By the mid-1930s, Carnap has given up on the epistemological project initiated by the *Aufbau* altogether, acknowledging that epistemology itself cannot be given an adequate formulation within physicalism, since it conflates its tasks with logic, psychology. He thus turns his view toward formulating a logic of science wherein the sole task of philosophy is the logical clarification of concepts of any language. The class of statements consisting in the language under consideration is given by behavioral psychologists.

Neurath's naturalism holds that any protocol statement must include a broader context of the agent's role in formulating such a statement. There can be no precise atomic statements because any statement within the language of physicalism cannot be given meaning without any such inclusion. This leads to his persistent worry about Carnap's project of the logical clarification of any languages on the grounds that any such methodological approach might be misunderstood to provide some irrefutable building blocks to the edifice of science. Science for Neurath can never be purged of imprecision and to suggest such a notion that the building blocks of science might be conceived as primitive gives way to giving some statements unwarranted status in the system of science. Furthermore, purely logical statements can never say anything about the world: thus a logically reconstructed protocol can never be about statements made in science. It is only when the context of a perceiver is introduced with all its imprecision that one can be talking about scientific statements.

Carnap's logic of science on the other hand takes the task of philosophy as the logical analysis of language. His principal of tolerance allows for the presentation of various forms of protocol sentences that can be assessed based on pragmatic considerations. He takes it as an open question, to be determined through further inquiry, which system one intends to take up. Moreover his emerging distinction between material and formal modes of speech and the subsequent psychological/logical distinction wherein the division does not fall within factual statements – but between the factual and formal, suggests that the developing division between them breaks down into different aspects of inquiry into the language of science.

Lest this point be taken to miss the significance of their differences it can be made more fully in the discussion on their views of Tarski's theory of truth in the next chapter. However for the point of the present intentions, one may say that on the face of it, their differences might be understood to be getting evermore deeply entrenched – Neurath continued to criticize Carnap's view throughout the 1930s and into the 1940s. Nevertheless, as will be seen below, the emerging distinction articulated by Carnap regarding the psychological and logical aspects of the analysis of science necessitates and vindicates their two methodological approaches to developing an adequate account of science.

The next chapter will serve to introduce Hempel into the discussion. Schlick's contribution to the debate (1934) will remind readers that the Vienna Circle was not unified on what was at stake for their interests. In particular he worries that the entire protocol sentence debate obscures what is really at issue: the foundations of scientific knowledge. He thus raises the question of the connection between language and the world. Hempel (1935) marks his first publication and his response to Schlick's objections. Notably it is a paper about which Hempel came to hold some regret.

Chapter Three: Truth and confirmation: Carnap's distinction in the protocol sentence debate

In 1935, Hempel weighed in on the protocol sentence debate, attempting to synthesize Carnap and Neurath's respective positions. Two difficulties with particular historical readings of the development of Hempel's thought should be considered. First, Richard Jefferey suggests that Hempel regretted his 1935 publication because of the scorn poured over it by Russell (1940). Second, Friedman (2000) notes that he saw Hempel's last two public statements wherein he "joyfully described his own conversion from the point of view of Carnapian 'explication' or 'rational reconstruction' to the point of view of Kuhnian historical sociocultural naturalism as a return to Neurath's original of the ineliminable necessity of '*Ballungen*'" (p. 45). Neither of these views are intended to amount to developed philosophical arguments and it is surely the case Hempel both regretted his 1935 paper (he redresses his [1935] reading nearly 50 years later in Hempel [1983]) and moved from Carnapian analysis to something more akin to Neurathian historico-sociological naturalism. However, the important question is, how ought philosophers understand this regret and this move?

The worry is that, though Jeffrey and Friedman make minor points, they indicate a subsequent disjointed trajectory of Hempel's thought. That disjointed trajectory could be understood to vindicate the attitude that Hempel's philosophy of science, though an important historical moment in the development of 20th century philosophy, is not a resource to be taken seriously in contemporary philosophical questions. Since a key point of this thesis is to indicate, conversely, that an appropriate understanding of Hempel's thought may fruitfully change the way in which certain contemporary philosophical questions are posed, and consequently what sorts of answers one might expect, it will be helpful to suggest the more consistent development from the protocol sentence debate to Hempel's D-N account of explanation.

Rendering Hempel's view as such will afford the means to reformulate the problem confronting the logical empiricists. As has been discussed, the protocol sentence debate arose over the status and form of protocol sentences. Protocols are adopted by a conventional decision, are revisable and are taken as a starting point from which to analyze the language of science. Early on, the principal difference between Carnap and Neurath is, roughly, that protocols are either basic, atomic and primitive for the former or consist of *Ballungen* for the latter– they are ever-reducible and always contain ambiguous concepts. Initially the distinction Carnap sought was between protocol sentences and non-protocol sentences: that the former could be neatly specified and provide the building blocks for the edifice of science which consists of both basic sentences and non-basic sentences derivable from them. Throughout the protocol sentence debate, that distinction became less and less formulable until by the mid 1930s Carnap had moved his view closer to Neurath's in terms of the character of protocol sentences. One consequence was that protocols themselves were understood to be no different in status from any other statement in science.

Yet Carnap's distinction between formal and material modes appears to be emerging as entirely necessary for his (and arguably Neurath's as well) philosophy of science. Once he embraces Tarski's theory of truth the distinction comes to be drawn between factual statements and their formal structure. Neurath's worry with Carnap's logic of science then comes to be whether any such formalization of a factual statement might come to be understood as somehow immutable and an unrevisable foundation for factual statements. Furthermore, the question is whether one can bridge the divide between logic and observation – whether formalizations can be linked adequately to empirical observation. Neurath's metaphysical worries aside, one may be able to reformulate what becomes the problem to be dealt with once the strict distinction between logic and psychology is assumed. How can a formalization of the structure of factual statements have any connection to a particular factual statement? How can Carnap's logic of science say anything about empirical science? If one takes the formal features of a protocol sentence as a starting point, how can those formal features logically connect to the empirical statements to which they are intended to refer? Or put another way, how can an idealized representation of a statement be about an actual statement? These appear to be the relevant questions once one avoids conflating factual statements with their logical features (which Carnap understood much Vienna Circle philosophy to have done). They present a reformulated understanding of the philosophical problem confronting logical empiricism by the late 1930s. And they provide a launch point from which to understand the point of Hempel's subsequently developed D-N account of explanation.

The next chapter addresses Jeffrey and Friedman's remarks and the question of whether the protocol sentence debate can find resolve (in Hempel's work in particular). The present chapter looks at Schlick's reply to Neurath in 1934 and Hempel's emergence into the public form of the debate. It will show that the Vienna Circle was by no means in agreement about what was at issue in the problem of scientific knowledge. Moreover it will indicate just what the problem was for Hempel's (1935) rendering of the issue. It considers Russell's 1940 critique of what he calls the Neurath/Hempel view, suggesting that Russell really misses a key point of the protocol sentence debate and the views that emerged from it. Moreover, this chapter examines the role of Tarski's theory of truth and the way in which that motivates, for the left wing of the Vienna Circle, Carnap's strict distinction between the logical and psychological that is arguably shared by Carnap, Neurath and Hempel toward the end of the 1930s. This analysis will suggest the context within which to formulate the present reading of Hempel's D-N account of science in the final chapter.

Schlick's criticism of the protocol sentence debate and Hempel's response

Schlick's response in 1934 to the protocol sentence debate indicates that the Vienna Circle was by no means unified over what was at stake in the debate and the problem of scientific knowledge more generally. Schlick holds that the dispute itself obscures what is crucial in the problem scientific knowledge, arguing that it fails to articulate some foundation in truth or certainty. The problem with the way the left wing of the circle addresses the issue is that their view amounts to a coherence theory of truth that would allow any fairy tale that exhibits a coherence of its statements the same status of alleged knowledge produced in science.[1]

Friedman (2000, p. 46) writes that Schlick (1934) worries that the Neurath/Carnap conception leads to a coherence theory of truth, the problem for which is that *any* logically consistent set of sentences can stand equally as a candidate for a 'true' set of sentences. He argues that empirical science requires some fixed statements rooted in immediate experience. Otherwise statements deemed true in science that nevertheless contradict one's own view, would have to be accepted over more certain subjective experience. However, argues Schlick (ibid, p. 379), "It is theoretically conceivable that the statements made by everyone else about the world should be in no way confirmed by my own observations."

Schlick (ibid, p. 370) writes that radical empiricism considers the function and structure of protocol propositions as "the ultimate ground of knowledge." Originally the notion, 'protocol propositions' meant basic factual propositions of the simplest form and on which more complex propositions could be based. If it is possible to produce (ibid) "raw facts quite purely in 'protocol propositions', then the latter seem to be the absolutely indubitable starting-points of all knowledge." However, inquiring after the foundation of knowledge is to inquire after a criterion of truth (ibid). Protocol propositions thus are meant to be able to provide a set of basic foundation points from which to construct the language of science, while the truth of these propositions could provide a "yardstick" by which to measure the truth of all other propositions. But

[1] For a discussion on and defense of Schlick's position, see Oberdan (1996).

according to the "Neurath camp," this yardstick is itself relative (ibid): "And that view with its consequences has been commended, also, as an eviction of the last remnant of 'absolutism' from philosophy." The problem is that one is left with a coherence theory of truth (ibid, p. 375), in which propositions derive their 'truth' status in their "mutual agreement ... with one another." Schlick argues this "doctrine is wholly untenable" because non-contradiction among propositions determining truth is inadequate as an account of material truth (ibid, p. 376).

Schlick argues, conversely, that when one assents to knowledge claims in history or the natural sciences, this is done based not on the coherence of the statements in question with a broader body of statements, but because "we know precisely in what way such factual statements ordinarily come to be made, and this way inspires our confidence" (ibid, p. 378). We consequently order the propositions according to their origin. One should always test the truth of a world picture against one's own experience: "What I see, I see!" (ibid, p. 380). Immediate experience – 'affirmations' in Schlick's terminology – is afforded prime status among propositions.

Friedman (2000, p. 46) remarks that Neurath (1934) is a response to Schlick in which he rejects any demand for "fixed and absolutely certain assertions against which all others are to be tested"; he rejects any talk of "affirmations," or a comparison of statements with notions like "experience" or "reality" on the grounds that these concepts simply cannot be given any meaningful formulation. For Neurath, unified science necessarily is a "comparison, and consequent mutual adjustment, involving sentences with one another" (Friedman, 2000, p. 46). According to Friedman (ibid, p. 47), for Neurath all meaningful terminology is historically and sociologically conditioned such that any use of terms needs to be fleshed out according to how they are understood at their point of application. For example, the terms "sentence" or "language" are not compared with "reality," but need to be understood against the system of statements in which they have been employed. Practical constraints then limit the options of alternate logically consistent systems, which for Neurath indicates that Schlick's problem of mere logical coherence of a system does not arise: protocols are established in scientific practice, which, it will be seen, suggests a notion of causality underlying Neurath's conception. However any such notion of causality is pragmatic and not philosophical for Neurath. This can be understood as a result of Neurath's view on the limits of methodology. Certainly one would want indubitable foundations for scientific knowledge, but Neurath rejects the notion that any such foundation is available. Simply appealing to notions of 'truth', 'subjective experience', or 'reality' cannot serve its task because these terms, for Neurath, resist any meaningful interpretation. This does not amount to faulty formulation of the foundations of knowledge, but is indicative of Neurath's skepticism regarding the extent to which one can establish certain or indubitable foundations: it is the state of human knowledge and the question surrounds not how to change that state, but how to proceed with a positive account of the structure of science in light of limits restricting human knowledge.

Lest one misread the orientation of Neurath's view, it should be clarified that he does not presume to *impose* limits on scientific knowledge. His is not a restrictive programme lacking in creativity. Rather, he intends to be clear about what exactly the expansion of scientific knowledge amounts to. And for Neurath, the growth of knowledge points to the ever-expanding domain of physicalism, which consists, in widest sense, in everything that can be meaningfully asserted about 'reality'. Suggesting that somehow the advancement of science underwrites claims to capturing reality is fiction and a vestige of what he takes to be the conceptual bonds of theology and meaningless metaphysics.[2] Human knowledge is bound by certain limits. The wider concern of the protocol sentence debate is about how to formulate an empirically grounded and systematic methodology employed in science to characterize the nature and limits of that knowledge.

However Friedman (ibid) argues that Schlick has rejected "such a historico-sociological perspective on science" since he would never be willing to

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[2] The point here for Neurath is to emphasize science as affording the scope of 'everything that can be meaningfully asserted', rather than emphasizing it as 'capturing reality'.

give up his own observations in light of a radically different logically consistent set of sentences adopted by a given community of scientists. Friedman (ibid) writes,

> And it is in this essentially individualistic conception of science, according to which all demands of intersubjectivity can, at least in principle, be sacrificed on behalf of one's 'own' subjective experience, that Schlick's version of 'empiricism' fundamentally diverges from both Neurath's sociological naturalism and Carnap's logico-linguistic pluralism.

Schlick's worries could be adapted into the Neurath/Hempel framework. Employing the body of scientific statements – that Neurath holds is adopted for practical reasons – could be backed by the fact that it does not require individuals within the scientific community to reject its observations (or more precisely statements regarding those observations). As will be indicated below, this effectively amounts to the way Hempel replies to Schlick's criticism: that as a matter of empirical fact, individuals are not forced to sacrifice their own affirmations. Of course, since this is a foundational question, Schlick's worry is that the Neurath view allows for the possibility that it can.

From Schick's perspective, of course, it would not be possible to incorporate his view into the Neurath/Hempel framework since it is precisely one's individual experience that provides the foundation for a developing system of science. However, given the Neurath/Hempel view that statements are checked only against statements, Schlick's position could be interpreted in their terms as follows: one may hold up one's statements regarding personal experience against the larger body of physicalistic statements to determine the way in which they relate to that body. If one's own experience does not line up with that larger body, it is of course possible – in principle – to reject the current body of scientific statements, but it may be more prudent to consider that one's own claims regarding personal experience are problematic. This point was illustrated in the example in the previous chapter of the man whose idyllic life unravels in a day.

A further illustration might prove helpful. A relatively recent popular movie series echoes a classic philosophical thought experiment. "The Matrix" serves as a similar, but more developed example of Putnam's brain in a vat thought experiment.[3] One principal difference between the two is that Putnam's example is meant to illustrate that we cannot determine the veracity of our experiences, since we may be a brain in a vat, stimulated by various probes in a scientist's laboratory. Putnam's example likewise echoes the classic Cartesian doubt about whether our experiences are authentic or whether we are being deceived by an evil demon. The movie itself presents pop-philosophical questions regarding authenticity and identity. However it also presents for current consideration an example to illustrate a Neurathian response to Schlick's critique. The movie starts from the assumption that the matrix is a computer-simulated world in which humans experience their lives. They are, in 'reality', being farmed by a race of computers for their energy. Neo, the protagonist of the story is offered a choice to be removed from the matrix and live an authentic existence in reality or go back to his life in the matrix with his memory erased of the experience of his exciting offer. His choice is to take the green pill to escape the matrix or a blue one to return.

Now the movie presupposes that Neo is presented a choice over authenticity or constructed experiences. However, and this ties now to the question of the protocol sentence debate, Neurath would argue that there is no such position outside the scenario in which the protagonist finds himself from which to make such a decision. Neo has no position outside his set of experiences in which his life consists. He sits in a room across from the Laurence Fishburne character. He is presented two pills. The movie gives this as a choice between a real and fabricated set of experiences. However, what more does the character have to go on to base his decision other than precisely the experiences he has had up to that point? Those subjective experiences include his alleged life

[3] See Putnam, H. (1982).

in the matrix and the very experience of being presented the choice to leave it. In the story the context presupposes a standpoint from outside his own experience by which to make the choice. But it is precisely that standpoint to which he has no access. In Putnam's example, the brain in a vat can be understood, in part, to indicate epistemological worries about using experience as a basis for knowledge. In the movie's rather more pedestrian motives, it is about human authentic existence. For the present purposes, it indicates the way in which one can argue that there can be no reference to any world outside the language of science (which functions as a systematic set of statements about our experiences) by which to make a decision between authenticity and fabrication. The decision, according to Neurath, falls to convention.

One should clarify of course, that the example as presented has referred to one's 'experience' in a subjective sense. Neurath, as has been indicated, can find no way meaningfully to formulate 'subjective' notions of experience. So to be more precise the example should be construed as "Neo has been presented with a choice between pills." What is at stake according to a physicalist formulation is that Neo cannot make a decision about outcomes of the pills based on any account outside the framework of statements with which he, or anyone else, can formulate an account of this experience. Moreover, there is no systematic way for him to make the choice between authentic and inauthentic existence because there is nothing in the system of statements about the world that would afford him a criterion to determine what is authentic and what is not. To be cynical, the green pill, after all, offers him escape from his mundane world of nine-to-five computer programming into one where he plays a Christ figure saving all of humanity, beloved by a beautiful action-engaged woman and respected and admired by his male colleagues. If anything, the premise sounds like a comic-book lover's dream.

Thus, a Neurathian reply to Schlick might be that there is no criterion outside the language of physicalism with which to determine the authenticity of one's own experience. Schlick of course would appeal to the immediacy of the experience itself, but that immediacy can only be understood in light of the framework with which the broader community of scientists operates. 'Immediate experience' cannot tell Neo whether his normal life or his potential life as a Christ-figure is 'true' because that experience gains meaning only in the intersubjective realm of the language comprising physicalism (even as a rejection of the conclusions of that language – any such rejection raises the question of how to interpret the distinction between one's own experience and that described in the statements of physicalism, which echoes the claims Putnam used the brain-in-a-vat example to make vivid.)

Hempel (1935) is his response to Schlick. Friedman (2000, p. 47) notes that it is a "hurriedly condensed" version of a talk Hempel gave at the request of Susan Stebbing in London. It marks his entrance into the public forum of the debate and proves problematic within the canon of his thought. Clarifying the problem indicates how to understand the development of his subsequent thought and the overall nature of the trajectory of his body of work. Hempel's paper agrees with Schlick's coherence characterization of Carnap and Neurath's theory of truth, though he notes that it is a coherence theory of a restrained sort. He traces the development of Vienna Circle discussions away from the correspondence theory of truth in the *Tractatus* to a coherence theory based upon Carnap's emerging distinction between material and formal modes of speech, thereby indicating that Schlick's worries, which are provided in the material mode of speech, are pseudo problems.

Hempel (1935, p. 9) asserts that the Carnap/Neurath position amounts to a coherence theory, but of a "restrained" sort. And he (ibid) appeals to Carnap's distinction between material and formal modes (logic of science) of speech as an "explicit statement… of the coherence theory of truth." Given this distinction between modes of speech, the logic of science is to be differentiated from empirical science. The former consists in statements about formal features of scientific statements – about "certain properties and relations of scientific propositions only" – thereby affording a crude concept of truth as the "sufficient agreement between the system of acknowledged protocol-statements and the logical consequences which may be deduced from the statement and other statements which are already adopted" (ibid). Empirical science consists of statements in the material mode of speech. These latter statements comprise the domain of empirical inquiry, while the logic of science makes assertions regarding the formal structure of those statements and their relations to one another. The logic of science thus exhibits an ambivalence regarding questions of the truth of empirical statements except insofar as may be characterized in the formal mode of speech. Which is to say that questions of the truth of statements are not answered by appeal to statements corresponding to the 'world', but by their logical relations to the body of statements (protocol and other) against which the question of truth is raised.

Hempel replies to Schlick's main worry over mutually incompatible, yet internally, logically consistent ("true") systems of sentences in science – which Friedman notes amounts to an attempt to synthesize Carnap and Neurath's respective views. Hempel (1935, p. 18) writes that there is "no logical difference between ... two compared systems." The difference is "an *empirical* one" (ibid). As an empirical fact, protocols generated by various scientists contribute to a set of coherent statements and theories. Certain protocols, as an empirical fact, are called 'true' insofar as they are "sufficiently supported by that system of actually adopted protocol statements" (ibid).

Hempel (ibid) thus classifies, roughly, Schlick and Neurath as advocating a correspondence theory of truth and a coherence theory of truth, respectively. For the former, truth arises from a correspondence between statements and facts (or "reality"). For the latter, truth [ibid] "is a possible property of a whole system of statements (i.e., a certain conformity of statements with each other)"; worries about an absolute foundation for truth amount to pseudo problems – as discussed below.

Hempel then indicates the way in which the logical positivists developed their theory out of the correspondence notion in Wittgenstein's *Tractatus* (1922). That text marks the "logical and historical starting point" of discussions in the Vienna Circle (Hempel, 1935, p. 10); it is "characterized by a correspondence theory of truth."[4] A statement is called 'true' in the *Tractatus* if "the fact or state of affairs expressed by it exists." The theory of facts takes facts of the world "to consist ultimately of certain kinds of elementary facts [atomic facts] which are not further reducible to other ones." The logical form of molecular facts, which are made up of atomic facts "reflects the formal structure of facts," so that the existence of molecular facts depends on the existence of its atomic constituents – and likewise the truth or falsity of such facts. "That is to say: each statement is conceived to be a truth-function of the atomic statements" (ibid).

Hempel remarks that early on the Vienna Circle adopted Wittgenstein's view (ibid), but Neurath, and shortly after, Carnap came to challenge the correspondence theory of truth. The chasm between theory and facts, or scientific statements and some 'reality' renders the formulability of that 'reality' impossible. For Neurath one can only compare statements with other statements, which for Hempel (ibid, p. 11) implies a coherence theory.

The first stage in the development of Vienna Circle thought away from Wittgenstein's *Tractatus* was to reformulate atomic facts as protocol statements. This represents the notion that the domain of scientific discourse does not extend past statements to some notion of the 'world' or the 'real'. The second stage (ibid, p. 11-12) was to reformulate the "formal structure of scientific statements." For Wittgenstein, "laws of nature" were not statements, since they cannot be "entirely verified," but give only instructions for establishing meaningful statements. Carnap considered Wittgenstein's criterion for meaningful statements too narrow, recognizing that scientific language includes empirical laws and singular statements, the combination with which one may derive predictions. Empirical laws are "general implicative statements" and are tested through an examination of "singular consequences." However, since one can derive an infinite number of singular statements from a general law, and therefore the law cannot be fully verified, the latter cannot be taken to be a *function* of the former (in the way the molecular facts are taken to be a function of atomic ones). Rather

[4] The protocol sentence debate emerged over discussions of Carnap's *Aufbau*, but as Hempel notes here, the early Vienna Circle meetings discussed Wittgenstein, among others.

(ibid) it "has in relation to [singular statements] the character of a *hypothesis*." Thus because one cannot deduce a general law from a finite set of singular sentences – each set "allows an infinite series of hypotheses" – the selection of hypotheses falls to a conventional decision (ibid).

Moreover, singular statements themselves exhibit the character of hypotheses: "even the singular statements, which we regard as true, depend upon which of the formally possible systems we choose" (ibid, p. 13). That conventional choice is "logically arbitrary," but is "practically restricted by psychological and sociological factors" – a point made by Neurath. Thus, since singular statements themselves are seen to be hypotheses based upon conventional decisions, rather than truth-giving atomic sentences, one cannot determine the truth of a statement based on the correspondence theory advocated in the *Tractatus*.

Thus there is a relaxation of the concept of truth: "In science a statement is adopted as true if it is sufficiently supported by protocol statements" (ibid, p. 13). At this stage, he remarks that there is still agreement between the Vienna Circle view and that of Wittgenstein, which is "the principle of reducing the test of each statement to a certain kind of comparison between the statement in question and a certain class of basic propositions which are conceived to be ultimate and not to admit of any doubt." This principle itself is rejected subsequently since one can always reject a protocol statement. He writes further (ibid, p. 14), "there may be attached to any empirical statement a chain of testing steps in which there is no absolute last link. It depends upon our decision when to break off the testing process...." Science is not likened to a pyramid of knowledge wherein all concepts are reducible to a set of basic propositions, but to a boat in Neurath's famous metaphor. Thus, the effective synthesis of Carnap's and Neurath's respective views is that protocol statements are always revisable, determined based on a decision and are understood as historico-sociologically determined. Their coherent 'fit' within the broader intersubjective system of protocol and non-protocol statements – that is, their acceptance as 'true' – is clarified by the logical explication of the form and relations among statements.

Hempel employs Carnap's distinction between formal and material modes of speech to suggest that Schlick's worries amount to pseudo problems. According to Hempel (ibid, p. 16), Schlick argues that abandoning the notion that certain basic statements remain "unalterable," leads to relativism by depriving science of "the idea of an absolute ground of knowledge." Hempel's reply is that "syntactical theory of scientific verification" cannot provide an account of anything that does not exist within its system: "And indeed, nowhere in science will one find a criterion of absolute unquestionable truth." Thus in order to be meaningful, propositions must be formulated within the logic of science – that is, in the formal mode of speech, but "absolute truth" can not be so formulated and must be understood to be meaningless. Schlick's worries are thus pseudo problems. Any degree of certainty will require one to look to the set of selected protocol statements, but those statements are revisable. The argument that one must start from "absolute truth" as a criterion therefore "starts from a false presupposition."

According to Hempel, Schlick's principal objection (ibid, p. 17) is that in abandoning a notion of an absolute starting point, one may construct any number of systems of cohering statements: "For any fairy tale there may be constructed a system of protocol statements by which it would be sufficiently supported; but we call the fairy tale false and the statements of empirical science true, though both comply with that formal criterion." The problem then is how to distinguish between true and false protocol sentences. For Carnap and Neurath there is no formal way to make this distinction, but, according to Hempel, there is an empirical one. Protocols attributed the status of 'true' are those that are accepted as true in practice and there is "fortunately" widespread agreement among scientists about what is empirically accepted as true. "True" protocol sentences are those that are accepted by convention (ibid). He writes (ibid, p. 19) that the "evolution of the concept of truth we considered is intimately allied to a change of view concerning the logical function of protocol statements."

However, Hempel's argument is factually problematic. Neither Carnap, nor Neurath endorsed a strict coherence theory of truth. Moreover it came to be understood that this discussion of truth conflated formal properties of a notion of truth with a criterion of acceptance of statements within the language of science.

Hempel redresses a problem

The problems in Hempel (1935) were significant enough that he redressed the issue almost fifty years later (Hempel 1983, p. 181). In particular, the problem dealt with is "the question under which conditions – given which reasons – should we accept empirical statements, especially the statements of the empirical sciences." And that problem is addressed, in part, by emphasizing the indispensability of the distinction between truth and acceptance – a distinction Hempel obscured in his (1935).

Hempel (1983) outlines the views of Schlick and Neurath, attempting to indicate how their respective positions are both informed by a causal element to the "advancement of protocols" – that is, protocols are advanced based on experience. Though it retraces some features of the debate already covered above, it will be prudent to cover the development of his argument. Neurath held a roughly coherence theory of acceptance, but his theory neither pertained to a definition of truth, nor precluded the source of protocols as lying outside the edifice of science. It allows that experience plays a role in the production of protocol sentences. This feature renders Neurath's view – given appropriate clarifications regarding whether he provided a theory of truth or one of acceptance – immune to Schlick's charge that he relied exclusively on coherence with the rest of that which is accepted by science for the admissibility of basic observations.

Schlick and Neurath are empiricists (ibid, p. 182): knowledge comes from experience, based on the "immediately given. This sets the limit for the content of legitimate science." Hempel notes that this statement is "vague and programmatic" and Schlick and Neurath give two different interpretations of that principle. The disagreement can be discovered in Schlick (1934) and Neurath (1934), but (Hempel 1983, p. 183) writes, "the disagreement between these two basic approaches has not been clearly resolved."

Schlick sought an account of facts rooted in certainty or truth. The criterion of truth (ibid) "referred to the data of immediate experience." One deduces (with a hypothetico-deductive model of testing) consequences from hypotheses. Deductions yield statements about observable events that can be checked against results in relevant experiments, "like the position of the indicator on a measurement instrument or the change in color in some chemical reaction." Conflict between hypothetical deductions and "directly advanced observation sentences" indicates the deduction is falsified. Agreement yields a partial confirmation, though not verification.

Since Schlick (1934, p. 370) is looking for a criterion of truth or falsity of observation sentences Hempel (ibid, p. 183) writes, "the question suggests itself whether the testing of scientific hypotheses can be founded on a certain class of observation sentences that, once advanced, were immediately certain." Determining this class of sentences provides a basis on which to have all other scientific statements agree. Error may always be present, but one may order sentences based on a level of certainty. The most certain of these would form a subclass and are called 'affirmations' by Schlick: they express individual perceptions or experiences. For Schlick, affirmations are not protocol sentences, which can be written down. The latter have the status of hypotheses, while the former motivate the formulation of them. Affirmations provide the foundations of protocols (Hempel 1983, pp. 185-6): the question of foundations will transform to affirmations and the (Schlick 1934, p. 387), "unshakeable points of contact between knowledge and reality."

Hempel (1983, p. 186) argues that Schlick's view conflates "two incompatible views of 'affirmations'." First, he takes them to be observation sentences and attempts to justify the notion that they are true by virtue of their being one's immediate experience. However, second, he argues that they cannot be written down or put into words, which makes it difficult to grasp how they could be construed as true or false. An affirmation in this sense is a "psychological event," but either "it occurs or it does not." There is no sense in which the event can be said to be true or false. Hempel (ibid) notes that such an event can play a "causal" role in the expression of a protocol sentence.

Hempel writes that Neurath asserts that one never tests scientific statements against facts or the "experiences," but against other statements, which stems from his stance against metaphysics (ibid, p. 187): notions like "reality" lead to "subjectless pseudoproblems." Neurath's empiricism then is that pursuing knowledge is not that of a certain link between knowledge and reality, but a pursuit of "agreement between the statements of science and as many protocol sentences as possible" (ibid). Though protocol sentences are the basis for testing empirical statements, all accepted statements, protocol and non-protocol alike, are subject to revision. Non-protocol sentences are revisable on the basis of Duhem's thesis: no hypothesis can be tested in isolation. Hempel (ibid, p. 189) notes that Neurath takes this further in regards to protocol sentences themselves, which are revisable and have the status of hypotheses as well. This amounts to Neurath's holism, which Hempel notes (ibid), "even if he did not always formulate or justify it with the utmost precision ... by now has pretty much succeeded.... To an individual scientific hypothesis, we can assign neither unequivocal testing methods nor a definite empirical content."

According to Hempel (ibid, p. 190), Schlick took Neurath's to be a coherence view, which can amount merely to "logical consistency." However, logical consistency is not the sole criterion for scientific acceptability. Neurath's version of empiricism plays a role – namely that non-protocol sentences must be in agreement with as many protocols (that is, with as many observation statements) as possible. Thus one accepts a non-protocol sentence based on the scope of agreement with accepted protocols. Simplicity may also play a role. Schlick's worry, as mentioned, is that Neurath's view admits the possibility of a fairy tale as much as scientific claims rooted in observations, but Schlick's worry would appear to be unfounded given that Neurath would argue acceptance of a class of scientific observations would agree with a larger class of accepted protocols.

Though Neurath's additional criteria indicate the way Schlick's worry is misguided, his view nevertheless admits of many different sets of systems of hypotheses that are equally acceptable. For Neurath this indicates the role of decisions within the scientific community: at some level there is no systematic way to determine which system of hypotheses to adopt. Moreover, Hempel (ibid) notes in Neurathian fashion, that there is no "one true picture of the world."

Neurath characterizes sentences as true, not in virtue of their agreement with facts, but when they can be incorporated into the larger body of scientific statements. Hempel notes that this confuses the semantic notion 'truth' with the epistemological notion 'acceptability'. That is, a statement is not 'true' by virtue of its agreement with a body of statements, but it is 'acceptable'. Neurath put forward his account in response to a correspondence notion of truth, but 'true' seems to have been a term he rather accepted reticently and felt that the business of inquiring after scientific knowledge could have been carried on without it. Hempel notes that logical positivists considered the term "either superfluous or metaphysical." That view changed, he (ibid) notes, upon the consideration of Tarski's theory of truth.

Hempel argues that the differences between the semantic notion of truth and epistemological notion of acceptability are as follows (ibid, p. 192). First, a sentence is true or false "timelessly" and "independent of whether it will ever be tested or not." Confirmation or acceptance of that sentence depends upon existing test results at any given time; confirmation and acceptance can thus change. Second, true sentences may in principle not be acceptable, or only "weakly" so, while false sentences may be acceptable depending on existing evidence in support of them. Third, for any sentence, it may be said to be true or false, but both a sentence and its negation may be not well supported by testing.

Hempel (ibid) notes that the "semantic notion of truth is indispensable in logic" and cannot be supplanted by an epistemological notion. Neurath's sociological description of the acceptability of hypotheses cannot be regarded as a theory of truth, but rather a "pragmatic interpretation of the acceptance and the acceptability of scientific systems of statements."

According to Hempel, the principle problem for Neurath then is the concern over how to choose a scientific theory. He argued (ibid, p. 193) there can be "no precise methodological rules" suggesting the contrary of any such assertion to be 'pseudo rationalism', which ties to his criticism of Carnap's logic of science and its connection to empirical statements. The logical analysis of languages can proceed successfully, according to Neurath, in the domain of mathematics, but when confronting the choice between two empirical theories, a strictly logical analysis cannot determine the outcome (ibid, p. 193): "two scientists can rely on different considerations that will not lead to clear agreement." Hempel notes that Neurath advocates the notion that "procedures of empirical science" can fall under a precise set of rules, but such rules may apply over a more narrowly specified domain.

Is Neurath's view then limited to descriptive theory or can there be a normative element? Could it (ibid) "establish norms for correct and rational scientific procedures?" If 'true' statements on Neurath's view are those accepted by scientists as agreeing with the body of scientific statements, can one "criticize behavior of scientists" if they make up results of experiments (ibid, pp. 194-5)? No theory of scientific knowledge can be restricted to description.

Hempel argues that Neurath emphasizes the descriptive aspect of scientific methodology (ibid, p. 196), but that he "builds certain epistemological norms into his description of empirical science." For instance, he argues that acceptable systems of sentences must be "logically consistent and deductively closed." Thus, acceptance of a statement is based upon whether it agrees with the larger body of statements. (As stated, even that large body of statements may in principle be rejected.) The problem, however, is that Neurath does not specify what that agreement relation is. Strict deductive connections among statements would appear to be too strict, given the problem of connecting logical structures of statements to the empirical statements they are intended to represent. So something must be said about the connection between formal representations of statements and the actual empirical conditions within which such statements are made, which presents the need to formulate more clearly Carnap's distinction between logical and psychological aspects of science. And this presents the further problem of when they can be understood to be connected. Neurath's opinion is that there is no systematic means by which to determine when that connection is sufficient, but it will rely on a decision. Furthermore, it might be argued (with Carnap) that such a formal representation of the structure of statements is indispensible to begin to formulate the question of agreement between statements at all. Neurath locates the role of decision in accepting a connection, but is rather ambiguous about what acceptable connections will look like. Carnap's formalizations can be understood as an attempt to render suggested connections more clearly.

Hempel questions whether Neurath may be considered a coherence theorist. As mentioned, he argues that Neurath's theory is not a theory of truth, but of acceptability. Part of the problem of understanding Neurath's view is that (ibid, p. 197) "he himself used the words 'true' and 'false' in the statement of his view," which conflates the question of truth with that of acceptance. In so far as Neurath might be understood as a coherence theorist, he gives a coherence theory of acceptance, not of truth.

However, Hempel (ibid) argues that Neurath is not even a strict coherence theorist of acceptability if the coherence view were that "the only demand on an acceptable system of hypotheses is that the sentences of this system stand IN certain sorts of agreement-relations to each other." As mentioned, the notion of 'agreement' is vague in Neurath's work (ibid). In addition to agreement with existing statements, Neurath advocates his empiricist maxim that sentences within a system should agree with as many protocols (the basic building blocks for the system) as possible. The upshot is that, although, the nature of relations among sentences is left ambiguous (ibid), "It just demands that the whole system contain sentences that are advanced under appropriate circumstances by competent observers." Protocol sentences must be included in any system and protocol sentences cannot be characterized based on their relations to other sentences – "at least not exclusively." Hempel (ibid) proposes thus that there is a causal element to Neurath's advancement of protocols. Moreover, he compares these features with the causal element found in Schlick: affirmations are "occasions for framing protocol sentences." Experience affords occasion to formulate protocols by which one makes meaningful assertions about that experience. Hempel concludes that both these views "go beyond" coherence because they intend to emphasize the role that experience plays in the generation of protocol sentences. The causal element in experience behind the occasions to put forward certain protocols indicates that Neurath is not a strict coherence theorist: his empiricism indicates that. Hempel (ibid, p. 198) states furthermore,

> In order to take the empirical character of Neurath's protocol sentences into account, one has to support and to refine the idea of directly advanced protocol sentences using an empirical theory of human observation and concept formation. Studies that go in such a direction have been undertaken for some years now. Neurath, without doubt, would have heartily welcomed these more recent attempts toward a 'naturalized epistemology'.

It will be fruitful to return to Hempel's (1935) misconstrual of Carnap and Neurath as coherence theorists in light of a consideration of Tarski's theory of truth. Hempel (1983) has argued that he and Neurath conflated notions of truth with those of acceptance and this can be understood more clearly with attention to how Tarski's theory of truth impacted the protocol sentence debate.

Tarski, truth and confirmation

By the mid 1930s the protocol sentence debate had brought about the centrality of the distinction between formal and material aspects of an analysis of the structure of science. Although Neurath still worried about the possibility of application of formal representations of material statements in scientific practice, he had influenced Carnap away from attempting to formulate protocols in terms of a subjective language situated outside the language of physicalism. Carnap, for his part however, did not retreat from his view that a distinction between formal and material modes of speech was crucial for assessing the structure of science: it marks an indispensible distinction. Rather he sought to push that distinction further and utilized Alfred Tarski's definition of truth to underwrite his own developing view.

Friedman (2000, p. 49) notes that Carnap invited Tarski to deliver a paper (Tarski, 1936) on the semantic definition of truth at the Paris Congress in 1935. Carnap understood Tarski to provide a definition that clarifies certain confusions related to the protocol sentence debate and epistemological worries more generally. Carnap himself took Tarski's definition to afford the expression of the practical importance of distinguishing between logical and empirical aspects of science.

According to Tarski (1936, p. 401), semantics is the domain addressing connections between languages and the objects or states of affairs referred to by expressions within languages. For example (ibid), concepts like 'denotation', 'satisfaction', 'definition' or 'truth' are all semantic concepts. 'Truth' is a semantic concept in its classical interpretation, wherein truth (ibid), "signifies the same as 'corresponding with reality'." Though semantical terms can be understood with relative ease informally, any precise rendering of such concepts has in previous discussions led to paradox (ibid). Here and in particular with reference to the protocol sentence debate, by Tarski's account, 'corresponding with reality' would be understood informally.

According to Tarski, semantical concepts must be *related* to a linguistic framework and are not in that framework (ibid, p. 402). Thus there ought to be a distinction drawn between the "language *about which* we speak" and the "language *in which* we speak." Because any language understood as containing its own semantics leads to inconsistency, semantical concepts must be provided by a metalanguage – some linguistic framework external to the object language that provides the semantic rules governing the application of that language to its objects of reference. One starts with observation language and formulates a meta-language to make explicit the way in which that observation language is intended to apply to events or facts.

Tarski writes (ibid) that to lay the "foundations of a scientific semantics" one must follow certain steps: "characterizing precisely the semantical concepts and [...] setting up a logically unobjectionable and materially adequate way of using these concepts." First, one must describe the language (ibid): "enumerate the primitive terms of the language and give the rules of definition by which new terms distinct from the primitive ones can be introduced into the language." Second, one must separate axioms from the rest of the deduced sentences, and third, "formulate rules of inference," that indicate which theorems may be derived from the axioms.

Once a description of the language is provided, one must construct the meta-language that provides "logically unobjectionable," semantical concepts for application of the original language (ibid): "The most important point in this construction is the problem of equipping the metalanguage with a sufficiently rich vocabulary." That vocabulary must furnish the semantical concepts with appropriate references to relations between the object language and the objects to which it is intended to refer. Tarski thus writes that (ibid), "The metalanguage which is to form the basis for semantical investigations must thus contain both kinds of expression: the expressions of the original language, and the expressions of the morphology of language" (the description of the structure of the language couched in the terms of that language).

One then needs to indicate the conditions under which the semantical concepts can be understood as "materially adequate and in accordance with ordinary usage" (ibid, p. 404). Tarski articulates this with reference to the concept 'truth' in so far as that concept is understood as 'correspondence with reality'. 'Corresponding with reality' is itself vague, but can be specified in Tarski's definition as follows (ibid): "the sentence 'it is snowing' is true if and only if it is snowing." More generally, Tarski gives this phrase the form, "the sentence x is true if and only if p, where 'p' is to be replaced by any sentence of the language under investigation and 'x' by any individual name of that sentence provided this name occurs in the metalanguage" (ibid). If the metalanguage is robust enough to account for the expression, 'the sentence x is true', "in such a way that every statement of the form discussed can be proved on the basis of the axioms and rules of inference of the metalanguage" then one can establish the material adequacy of the truth claim. If such a concept of truth can be introduced as a (partial) definition, then the partial definition itself can be understood as 'materially adequate'. (Further conventions may be established regarding usage of the materially adequate concept, which Tarski notes [ibid, p. 405] is formulated in the metametalanguage; this follows from his notion that no language can provide its own rules of application without leading to inconsistency.)

Given these foundations, a clear and exact characterization of a language is only possible within formalized languages, in which one can specify all the statements within the object language formally and all the semantical concepts specifying how the object language applies to the objects it is about. Tarski (ibid, p. 403) writes that a clear characterization is possible "if we employ in it only those concepts which relate to the form and arrangement of the signs and compound expressions of the language." But not all features of a given language (like those used in science) can be so formalized (ibid), which raises the question of how far one can construct an exact characterization of the language of science.

Thus Tarski sets up the machinery with which to deal with the "chief problem" (ibid): how to establish "a materially correct way of using the semantical concepts in the metalanguage." He notes that one of two procedures is thus required. One possible procedure is semantical concepts are introduced to the metalanguage as basic concepts, the properties of which are established axiomatically. Moreover, certain axioms give the materially adequate usage of the concepts of the object langauge. There are certain challenges to this procedure. Namely semantical concepts are chosen "accidentally" and based upon "inessential factors" like the current state of knowledge. There are further difficulties regarding the consistency of the semantics and the appropriateness of adopting concepts used in ordinary language that led to inconsistency and misunderstandings in the past (ibid, pp. 405-6). In addition there would be a problem of fitting this semantical conception in with the unity of science, since science employs concepts that are "neither logical nor physical" (as in the case of unobservable entities).

The second procedural option is that (ibid, p. 406), "the semantical concepts are defined in terms of the usual concepts of the metalanguage and are thus reduced to purely logical concepts." Thus the semantical concepts are formally construed – as part of the "morphology of language." Tarski notes (ibid) that the question then arises regarding whether the method may be at all applicable (since the semantical rules are given in purely formal terms), but this question he takes to be answerable. A metalanguage can provide materially adequate and methodologically correct definitions of semantical concepts for the language in question "if and only if the metalanguage is equipped with variables of higher logical type than all the variables of the language which is the subject of investigation." Thus the metalanguage can provide sufficient semantical definitions for the language in question if it contains additional variables *about* the variables of the language in question. However, Tarski argues that no justification of this claim is possible. Rather, it can serve a *useful* purpose for a number of reasons. For example, starting with the concept of 'satisfaction' provides "few difficulties" (ibid), and "the remaining semantical concepts are easily reducible to it" (ibid, p. 407). Moreover one may derive general theorems from such definitions of semantical concepts. From the definition of truth, one can (ibid), "prove the laws of contradiction and of excluded middle." Thus the metalanguage is adopted on pragmatic, not justificatory, grounds.

Having thus established scientific semantics, Tarski leaves open the extent to which it may contribute to epistemological considerations, but he is optimistic. Importantly for Carnap, Tarski's definition of truth clears up certain ambiguities that have traded in epistemological considerations and worries about establishing a sufficient account of the structure of science. For example, Tarski's definition of truth motivates for Carnap (1936b, p. 119) the notion that there is an important distinction to draw between 'truth' and 'confirmation' and that the failure to draw such a distinction leads to confusion with regards to epistemological questions and those over the structure of science. 'Truth' is a "time-independent" term such that what is true is always true. It can be established within formal languages in which all the constituents of the object and meta-languages can be specified. 'Confirmation', has two conceptual types. The first is the pragmatic conception, which is time-dependent and says, "such and such a statement is confirmed to a high degree by observations ... at such and such a time." The second is the semantical concept of degree of confirmation, which is, like the concept of 'truth', time-independent. It is understood, "*with respect to other statements* which formulate the evidence" (ibid). Using the concept in this way asserts an analytic truth such that the status of the degree of confirmation of a statement follows from the "presupposed" definition of the 'degree of confirmation', which is given in light of evidence of related statements.

Thus, Carnap outlines three terms with respect to this distinction (ibid):

1. Semantic definition of truth: time-independent – the statement x is true if p.
2. Pragmatic confirmation: time-dependent – a statement is confirmed to a certain extent at a given time.
3. Semantic confirmation: time-independent – asserts analytic truth in the logical relations of statements given a definition of degree of confirmation.

Semantic confirmation provides the means to understand how, for Carnap, the semantic view can introduce notions of truth to empirical claims.

Like Tarski, Carnap argues that although the concept of truth largely can be understood when used informally "in conversational language," it nevertheless can lead to contradictions. Some, therefore, have avoided its use altogether, while others, given the difficulty in defining truth, have conflated it with the concept 'confirmed'. Using the concept 'truth' in the latter way leads to abandoning conventional applications (ibid): "Thus one would find it necessary to abandon, e.g., the principle of the excluded middle." That principle would hold that for any statement, it is either true or false, but in science there are many statements that are neither confirmed (accepted) nor denied (rejected).

However, Tarski's definition of truth (ibid), "explicates adequately the meaning of this word in common language." That definition makes it clear to Carnap that 'truth' ought then to be distinguished from 'confirmation' because the definition of the former is limited in its range of application to formalized languages in order to avoid contradiction. Consequently, acceptance criteria for empirical statements pertains, not to notions of truth – since truth can only be defined in a fully formalized language, but to degree of confirmation. Epistemological notions like justification can only be articulated based on criteria of confirmation. This is to say that the definition of truth cannot be understood to contribute to a criterion of confirmation in epistemology: the definition of truth cannot provide the basis for a theory of knowledge, since the definition gives 'truth' only trivially (ibid, p. 120) – it "consists in the statement itself." Carnap therefore states that Tarski's definition, 'the statement, "snow is white" is true if and only if snow is white', establishes a definition of truth, but it does not thereby answer the question of confirmation –i.e. the degree of confirmation with which one can say snow is, in fact, white.

This distinction between truth and confirmation is crucial for Carnap to indicate what, exactly, one might be after in a theory of knowledge. When one claims to know something, this may be understood in two ways. First, one could mean (a) 'perfect knowledge', which could never be refuted. Second, one could mean (b) 'imperfect knowledge', which asserts a degree of (but not absolute) certainty. Carnap employs knowledge in the imperfect sense (ibid).

To indicate the distinction between truth and confirmation he lists four sentences.

1. The substance in this vessel is alcohol.
2. The sentence 'the substance in this vessel is alcohol' is true.
3. *X* knows (at the present moment) that the substance in this vessel is alcohol.
4. *X* knows that the sentence 'the substance in this vessel is alcohol' is true.

He remarks that statements (1) and (2) are logically equivalent because (ibid, p. 121) "they are merely different formulations for the same factual content" – as are (3) and (4). Statement (1) refers to the "object part of the language" and (2) refers to the "meta-part" (semantical). Thus, making the statement in the object language is logically equivalent to making the statement in the meta-language, even though it is in a different form. Carnap insists however that because of the ambiguities of natural language – in this case the term 'true' – logical equivalence can "only be made with certain qualifications." So, logical equivalence of (1) and (2) can be understood to hold true in the semantical sense (the object statement is included in the semantical version, which states that the object sentence 'is true') and it is this sense which Carnap and Tarski understand to be assumed in everyday and scientific use. He adds that whether it is actually used, however, is a "psychological or historical question" and not relevant to the establishment of his semantic methodology. He simply uses the notion of truth in the semantical sense. However the content of statements (2) and (3) is distinct, which, Carnap notes, has not been adequately appreciated by authors like Peirce, Dewey, Reichenbach and Neurath (ibid): this "seems to be the source of many misunderstandings in current discussions on the concept of truth."

What is the difference, then, in the 'content' of the truth statements and the knowledge statements? Since, assuming the semantic definition of truth, sentences (1) and (2) are logically equivalent, as are (3) and (4), Carnap notes that one might be led to conclude all true propositions just are those which are accepted (Neurath). If one argues, as Carnap does, that knowledge of the truth of propositions may be reversed with future inquiry, then what is deemed true effectively would be what is accepted. But Carnap argues that it is crucial that the content of (1) and (2) and of (3) and (4) is distinct. The former pair is about 'the substance in the vessel', while the latter is about 'X's knowledge'. That is to say that the definition of truth of a proposition is distinct from knowledge of the truth of the proposition: one would claim truth of a proposition based on the semantic definition of truth and knowledge of the truth of a proposition when one accepts it as true.

To expand on this point, Carnap addresses Felix Kaufmann's criticism of his own view.[5] On Carnap's account (ibid, p. 121), Kaufmann argues that Carnap's view is not compatible with the constitutive principle of empirical procedure that "rules out invariable truth of synthetic propositions." That is, empirical propositions are always subject to revision and therefore cannot be said to be true invariably at any time. Empirical procedure would indicate that one can never confirm "invariable truth of synthetic propositions" because 'invariable truth of synthetic propositions' is a contradiction of terms. Carnap's reply has two aspects: first that this reasoning incorrectly identifies truth with perfect knowledge; second, 'invariable' should be understood more adequately as 'time-independent' or 'non-temporal'.

First of all he writes (ibid, p. 122), "Kaufmann's reasoning seems to me based on the wrong identification of truth with perfect knowledge, hence, in the example, the identification of (2) with (3) in interpretation (a)." (Recall that interpretation (a) asserts perfect knowledge; interpretation (b) asserts imperfect knowledge.) What does this mean? Kaufmann wants to deny the admissibility of (4) which is a claim about knowledge of the truth of a sentence. For Kaufmann no synthetic sentence can be 'invariably' true and therefore, one cannot have any degree of confirmation of such a true sentence. By extension, one can never have

[5] See, Kaufmann (1942) and (1943).

knowledge of true synthetic statements because 'true synthetic statements' is a contradiction.

Carnap agrees that scientific procedure rules out perfect knowledge, but he refers to imperfect knowledge (b), not perfect knowledge (a), as Kaufmann's criticism suggests. Nevertheless, scientific procedure does not rule out truth, since the semantic form of the object language statement is logically equivalent to that statement. And one can admit such object language statements. To assert (2) is the same as to assert (1), and (1) is understood to be empirically meaningful. If the observational statement is taken to be empirically meaningful, then the semantic formulation is also empirically meaningful. To assert (2) is merely to assert an object statement (1) according to adopted semantic rules. One cannot, of course, confirm perfect knowledge of truth with scientific procedure, but to confirm imperfect knowledge of truth is to arrive at a degree of confirmation for acceptance or knowledge of the semantical version of the truth statement in question.

Carnap writes (ibid), "When Kaufmann declares that even imperfect knowledge of truth is unobtainable, then this means that even imperfect knowledge of (2) is unobtainable and hence that an event as described in (4), even in interpretation (b), cannot occur." However, he notes that no one thinks that sentence (3) is inadmissible. So when (3) occurs, (4) is said to occur also since these two are logically equivalent and both describe, "a certain state of knowledge of the person X." To repeat, the scope of the claim of (3) for Carnap is bounded by an imperfect knowledge that may be revised: it is a knowledge claim based on, and relative to, a specified degree of confirmation.

So, Kaufmann's objection, as Carnap understands it, denies that one can have knowledge of truth. He denies the admissibility of statement (4). However, since (3) is admissible and, given the semantic definition of truth, (4) is logically equivalent to (3), Carnap argues that one can meaningfully employ the terms 'knowledge of truth'. That knowledge, as has been pointed out, is not given by the notion of truth itself, but is understood relative to a degree of confirmation.

So, "*X* knows that the sentence 'the substance in this vessel is alcohol' is true" can be meaningfully asserted because the truth of the claim follows from the form expressed in the object language. "Knowledge" is understood relative to the specified degree of confirmation of the true sentence in object language: it is assessed against existing sentences that provide the framework with which to assign a degree of confirmation.

Second, Carnap proposes to discuss the problem another way to examine the underlying presupposition of the objection. Kaufmann uses 'invariable' truth to indicate an independence of truth from persons or states of knowledge, "and hence of time" (ibid, p. 122). Kaufmann thinks the semantical conception of truth should be abandoned with regard to synthetic statements about physical things because one can never determine the truth or falsity of a synthetic statement with absolute certainty. Carnap agrees, but argues that it does not follow that the term 'truth' is not admissible. That inference would be based upon the premise that a term should be rejected if one can never decide with absolute certainty whether the term applies in a given instance. However, if one accepted this premise it would rule out use of all physical terms – i.e. the term 'alcohol' since one could never determine with absolute certainty whether the term applies or not in an empirical situation (that is, whether a substance is alcohol or not). No physical term can be determined to apply with absolute certainty, which would rule out all physical terms as inadmissible.

Carnap acknowledges that of course no one actually accepts this principle formulated as such, but rather some milder version of it: a term is a legitimate scientific term if it can be determined to some degree whether it applies or not in a given instance. Statement (1) can be confirmed to some degree and therefore so can statement (2). Thus it is admissible. Meanwhile, articulating a criterion of confirmation involves both a description of scientific testing procedures (ibid) and the specification of the conditions in which a statement can be said to be confirmed to varying degrees – "i.e. scientifically accepted or rejected" (ibid, p. 124). Description of procedure in this sense is not a logical matter, but an empirical one, and therefore the domain of psychology and sociology. This, in part, is to say that scientific procedure is not systematically deduced exclusively within logic, but is motivated by decisions of inquirers themselves.

Carnap (ibid) distinguishes two types of statements relevant to confirmation, the directly and indirectly testable. Though he argues that they differ not in principle, but in degree, the distinction can serve to indicate two sources for testability of statements. Directly testable statements are those, which would be accepted or rejected "outright" on the basis of a few observations. Thus, the conditions for the statement 'the key is on the desk' would be those in which one sees a key on the desk (ibid). Indirect testing would involve testing statements that "stand in specifiable logical relations to the statement in question" (test-sentences). One may confirm indirectly testable statements by testing the set of statements from which the statement is deduced – for example, Carnap (ibid) notes existential sentences – i.e. 'there is a key'. Scientific laws on the other hand can only be confirmed to an ever-larger extent since one cannot deduce laws from particular occurrences.

Two operations are relevant to directly testable statements: (1) "confrontation of a statement with observation;" and (2) "confrontation of a statement with previously accepted statements." For the former, Carnap writes (ibid), "If, e.g., I see a key on my desk and I make the statement: 'There is a key on my desk," I accept this statement because I acknowledge it as highly confirmed on the basis of my visual and, possibly, tactual observations." The second operation (ibid, p 125) relates to the system of statements against which the first operation is held. An observation statement will be accepted if it satisfies the first operation and if it does not contradict statements that have been established by confirmation. In this case, either the observation statement in question, or one of the previously accepted statements must be abandoned, the decision of which is determined by established methodological rules. The first operation is crucial for confirmation; the second one is "auxiliary" and functions regulatively to eliminate inconsistencies within the body of scientific statements (ibid).

This assessment of the two operational procedures suggests a position in the question of whether statements can be compared with other statements, or must be compared with facts. Carnap states that if 'comparison with a fact' is understood in terms of the first operation, then statements can indeed be compared with facts, although he argues that 'confrontation' is more appropriate than 'comparison'. The latter suggests a comparison between two objects in terms of colour, size or other properties, whereas the former (ibid) "is understood to consist in finding out as to whether one object (the statement in this case) properly fits the other (the fact)." That is to say, 'comparison of a statement with a fact' is to ask whether the statement is (semantically) true – i.e. whether the "fact is such as it is described in the statement." 'Confrontation' indicates that the relation between a statement and the event or fact is established through the conscious effort of the observer. Moreover, he (ibid, p. 126) suggests formulating the relation in terms of a 'comparison' with 'facts' or 'reality', "easily tempts one into the absolutistic view according to which we are said to search for an absolute reality whose nature is assumed as fixed independently of the language chosen for its description." One would not understand the relation as somehow revealed between an object and a sentence, but that the sentence is constructed according to an assumed structural framework and then used to confront the 'object' to determine the degree to which formal relations can be established between that structure and the object. Carnap writes (ibid),

> 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. In translating one language into another the factual content of an empirical statement cannot always be preserved unchanged. Such changes are inevitable if the structures of the two languages differ in essential points.

Thus, the notion 'confrontation' indicates the role of the inquirer in formulating a statement and articulating the structural connections intended to hold between the statement and the object or fact: the causal antecedent leads one to formulate the statement about it.

However, Carnap's argument is not against the "content" of the expression 'statements therefore may be compared with facts'. Rather he argues against the form of that expression. Consequently, he is not to be understood as denying the claim that some relation between statements and facts can be forged – as denying the operation of confronting observation with statements (ibid): "Nor must the significance and indispensability of such confrontation be overshadowed by exclusive attention to the second operation." On the contrary, confrontation of a statement with observation is crucial for Carnap's empiricism.

He sums up his argument by emphasizing two points. First, one must distinguish questions regarding the definition of truth from those pertaining to a criterion of confirmation. Second, there are two operations relevant to confirmation: formulating observations and confronting statements with each other (ibid, p. 127).

Tarski's theory of truth had an impact on Carnap's work and facilitates the point at which Carnap rejects the sort of synthesis sought by Hempel (1935): 'coherence' for Carnap in 1934 could not characterize the scientific philosophy with which he took himself to be engaged. Friedman (2000, p. 48) notes that in 1934, - prior to Tarski's theory of truth – Carnap denied that, "the concept of truth, as applied to empirical propositions, is a meaningful component of *Wissenschaftslogik.*" 'True' for Carnap was not a syntactic concept and could not be characterized formally (ibid): "Better so: there is no theoretical criterion of truth in the domain of synthetic sentences." For Carnap a coherence theory of truth is one in which coherence is the sole criterion of truth. Also, Carnap at the time writes (1934a, pp. 268-9; in Friedman 2000, p. 49),

> *Truth and falsehood are not genuine syntactic properties;* whether a sentence is true or false cannot generally be seen by its design, that is, by the kinds and serial order of its symbols. (This fact has usually been overlooked, because one has normally been dealing not with descriptive, but only with logical languages, and in relation to these 'true' and 'false'

in fact coincide with 'analytic' and 'contradictory', respectively, and are thus syntactic concepts.)

Carnap effectively "undercuts" Hempel's synthesis as the science of logic being a coherence theory by stating that 'true' is not a syntactic concept.

However, by 1936 Tarski provided a sufficient definition of truth for Carnap. Though Hempel's (1935) notion of coherence misapprehended Carnap and Neurath's views in this respect, Carnap reformulated his account to indicate the way in which 'truth' functions within his methodology. As mentioned above, Carnap (1936b) distinguishes 'true' from 'confirmed' or 'scientifically accepted': 'true' is a logical ("timeless") concept, while 'confirmed' is empirical. Friedman (ibid) writes, "From this point of view, the Neurath-Hempel characterization of 'true' as applying to those sentences currently accepted by the community of scientists, is thus seen to rest on a fundamental confusion."

Friedman (2000, p. 51) notes moreover that Neurath's initial response to the Tarskian definition of truth at the Paris Congress of 1935 was that it was not reconcilable with an anti-metaphysical, properly empirical stance because of the notion of "timelessness" of that concept that separates scientific statements from those accepted at any given time. Nevertheless, a short time later Neurath advocated the legitimacy of Tarski's definition. However according to Friedman (ibid), in a letter to Hempel, Neurath suggested it is not clear that Tarski's definition "captures the unambiguous meaning of the traditional conception of truth" and Neurath's own characterization of true sentences agreeing with the body of scientific statements "is also a perfectly acceptable clarification of the traditional conception—which, moreover, has perhaps even better claims to historical continuity." Neurath, and likewise Hempel, considered Carnap's defense of the "semantic concept rather as a *suggestion*" (ibid). Hempel goes on to consider notions of truth in terms of confirmation or verification (ibid). The problem with the early Hempel view is precisely that he conflates of a notion of 'truth' with confirmation. He later acknowledged that Tarski's concept itself ought not be equated with confirmation, but is timeless. However each concept, 'truth' and 'confirmation', locate certain distinct aspects of a "rather vague notion of truth" (ibid and Hempel 1937, p. 227). The notion of a timeless (semantic) truth appears acceptable at that stage by both Carnap and Neurath. The question at stake then is over the material adequacy of such a definition in the actual practice of science.

For Carnap in 1936, this notion of truth is not merely a suggestion: one *must* sharply distinguish between these two concepts in order to clarify the structure of scientific statements (Friedman 2000, p. 52). This distinction leads to Carnap's articulation of the distinction between logical concepts, like 'truth', and psychological concepts like 'confirmation'. As Friedman states (ibid), "The main point of Carnap (1936b) is that such a sharp distinction between logical and psychological considerations is absolutely central to fully clarifying the nature of properly scientific philosophy, especially as it has been practiced within the Vienna Circle." Friedman (ibid, p. 53) notes that Carnap in 1936 insists on the importance of this distinction to clarify epistemological issues. 'Directly testable' is linked to his definition of 'observation' in his first English publication and both of these are psychological concepts. 'Indirectly testable statements', or assessing statements against the body of scientific statements marks a logical function.

It is at this point that Friedman (ibid, p. 54) notes, "the fundamental tension between Carnap's conception of *Wissenschaftslogik* and Neurath's has become intolerable." For Neurath there is "no room ... for a metalanguage or syntax language describing the process of empirical testing from some idealized point of view outside the language of empirical science itself." The semantic definition of truth is not materially adequate.

Friedman writes that the emphasis for Neurath is as follows (2000, p. 54):

> we describe how science, considered as an actual social system, operates with empirically and factually given real sentences and utterances (as opposed to mere 'serial structures' belonging to 'pure syntax').... there is

> a limit to the precision that can be required or attained in the actual historical-social process. We can certainly introduce logical precision into our actual scientific methods, by axiomatization, for example, but it makes no sense either to represent or to replace our actual procedures by a fully precise logical version.

One simply cannot eradicate language of *Ballungen* and it is that language actually employed in science – not syntactical formalizations – that must be assessed.

Friedman's narrative of the features of the debate at this stage place Hempel in October 1935 as torn in the middle between Neurath's 'naturalism' and Carnap's *Wissenschaftslogik*. That Hempel is torn at this stage may suggest a way to understand his subsequent stance. By 1938, Hempel seems resolutely on the side of Neurath, while Friedman (ibid, p. 55) indicates Neurath's view is motivated, in contrast to Carnap, by Tarski. Citing a letter of Hempel to Neurath, Friedman indicates the source and nature of this motivation (ibid):

> T[arski] thought, of course, that the Wittgensteinian idea of complete verifiability for empirical hypotheses is entirely naïve; but also that, in his opinion, Carnap's logical theory in Test[ability] and Meaning, based on much more liberal principles, did *not* achieve what was desired: in fact he is acquainted with no single example of a reduction-sentence that actually reduces a concept, say of physical theory, to concepts of observation-language in materially correct fashion (i.e., so that the empirical investigator would agree).

Hempel notes that Carnap provides something like schematizations (ibid), which are empirically inadequate – i.e., Carnap's example of solubility is not adequate because "in fact it can happen that a material is put in water does not disappear and yet is soluble.... 'exceptions' are always thinkable." The worry here then is over the logical connection between theory and observation. Tarski's view is that no logical connection may be constructed, but Carnap and – indeed – Hempel himself are at this stage holding out that one may be constructible (ibid): "it is at the very least not excluded that no such bridge can be forged in an adequate matter."

Here then is a glimpse at how to understand Hempel's view. He aligns himself with the Neurath position that empirical knowledge is historically-sociologically oriented, but holds out that the logical aspects of inquiry may provide some bridge between theory and observations – at least it remains to be discovered whether any such formulation can be constructed. Put another way, it remains to be developed how far one can push the limits of logical constructions toward material adequacy.

Friedman remarks that some of Hempel's defining papers of the 1940s (ibid, p. 58),[6]

> show Hempel as the master of Carnapian 'explication,' dedicated, above all, to finding a precise and explicit characterization in purely formal-logical terms of the crucial relationship between scientific theory, on the one side, and observational statements, on the other. Whether we look at this relationship in terms of confirmation (of theory by observational evidence) or explanation (of observational statements by theory), the central ambition is that it be reconstructed as perfectly precise and explicit, and, therefore, as 'objective'.

Given the survey above, one may locate keys aspects of Tarski's view and orient Carnap, Neurath and Hempel with respect to that view.

1. Semantics articulates connections between languages and objects; all semantical concepts for a given object language must be given by a meta-language, which, to be complete, must contain all terms of the object language plus semantical concepts telling how object terms apply in order to avoid inconsistency.

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[6] Hempel (1942), (1945), Hempel and Oppenheim (1948).

2. The goal of developing the foundations of semantics is to establish logically unobjectionable and materially adequate ways of using these concepts; this involves specifying the object language and its rules of application in a metalanguage:
    a. Describing the terms of the language, giving rules of definition.
    b. Locating axioms from which all other sentences can be deduced.
    c. Formulating rules of inference for all deduced sentences.
    d. Specifying a metalanguage which gives rules of application for the terms in the object language.
3. The goal of semantics *can only be achieved in formal metalanguages and with regard to formal object languages*, wherein a metalanguage may be formulated that provides all rules for the application of terms in an object language. One problem is that not all features of a language can be adequately formalized.
4. The biggest challenge is formulating a metalanguage with a sufficiently rich vocabulary to indicate how the object language applies to the objects it is about.

Thus the semantic definition of truth can be given only with respect to formal object languages, and only when a sufficiently robust metalanguage can be formulated that adequately encompasses the terms of the object language and rules of its application to objects or events.

One thus can map the respective positions of Carnap, Neurath and Hempel in light of Tarski's work. For Carnap, definition of truth can be given only in formal languages. A sufficiently robust metalanguage then can be understood to be provided only in formal mode so that all of the variables in the object language and the semantic variables in the metalanguage are contained within it. Problems in epistemology make assertions in natural language, the variables of which are too ambiguous to formalize, which leads to the confused notion that 'truth' (ambiguously asserted) necessarily informs questions of knowledge. Distinguishing between 'truth' and 'confirmation' indicates the way in which the semantic definition of truth (the most compellingly sufficient definition of truth at the time for Carnap) can have nothing to say about knowledge. That distinction does, however, bring Carnap back to the connection between theory and facts at the point of directly testable observations. If one can produce an adequate definition of confirmation with directly testable observations – that is, if one can produce a sufficiently robust metalanguage consisting of the relevant variables of statements in the observation language and the semantic rules telling how they can apply – then one can have a criterion of empirical truth. Neurath, of course, will reject this notion outright, as will Tarski express skepticism – but Carnap and Hempel believe that this remains a question for which the answer is yet to be determined. How far can one enlist the formal tools of modern logic to forge connections between scientific theory and observation statements?

Neurath's principal worry is that no formalization can adequately render the statements of science. *Ballungen* are always present and any attempt to overly constrain scientific statements with what came to be understood as "schematizations" would misrepresent the "congestions" of concepts present in any of the even basic statements of science. This is to say that Neurath rejects the notion that one may construct a sufficiently robust metalanguage that will account for the *Ballungen* present in the observation language. Put another way, if Carnap produces schematizations, or idealized models of object language, Neurath is worried that Carnap's formalization will employ the wrong models.

Hempel's corpus can be seen as developing the logical aspects of Carnapian analysis, while he remained explicitly cognizant of Neurath's worries. As will be discussed in the final chapter, schematizations – as Carnap has employed the term – can be understood as idealizations in Hempel's D-N account of scientific explanation. They are not directly about empirical science, but provide a framework with which to make statements about it. Is Hempel's methodology the right one? Like Tarski, one might be inclined to suggest that this is the wrong sort of question. Is it fruitful? Can one recommend it pragmatically? Yes.

Chapter Four: The outcome of the protocol sentence debate and Hempel's emerging philosophy of science

This thesis surveys the protocol sentence debate in order to explore the roots of Hempel's thought. Chapter Two examined the catalyst for the debate and Neurath's subsequent criticisms: Carnap's *Aufbau*. Richardson argues that Carnap's programme needs to be distinguished from Russell's external world programme on the grounds that Carnap did not share a commitment with Russell to the notion that causality, with respect to basic observation statements, was an epistemological notion. However, Carnap's ambivalence with respect to ontology or epistemology outside the framework of science does not reflect a naïve scientistic approach to the foundations of scientific knowledge. Rather, it is because he takes skepticism as a legitimate starting point for an inquiry into the structure of science, that Carnap believes his constitutional system, which Quine dubbed 'the best attempt to reduce scientific statements to observation', can have nothing meaningful to say about ontological or epistemological commitments prior to its adoption. According to Carnap, it is only in virtue of assuming any such constitutional system that epistemological questions can be raised at all. Basic observation sentences are adopted on the basis of convention. Thus, the second part of Chapter Two considers that the protocol sentence debate was not over justification of adopting one or another set of basic observation statements. Rather, given that protocols are taken, by convention, as the starting point for an empiricist system, it centered on the form and status of those protocols. The question of the extent to which one could derive all other concepts in science remained a problem to be worked out for Carnap and Hempel (Neurath denied this was possible).

Chapter Three examined Schlick's criticism of the left wing of the Circle and Hempel's subsequent reply. It indicated that all Vienna Circle members were not unified in the debate. Schlick believed that the debate missed what was crucial for scientific knowledge – namely, that it must be rooted in certainty or truth. Meanwhile, Hempel's attempt to synthesize Carnap and Neurath's views attributed to them views that they did not hold. Furthermore, the third chapter indicated the way in which Carnap's move to draw a distinction between formal and empirical aspects of science came to be of central concern in the debate. He argued that failing to distinguish between them led to much confusion in the assessment of the structure of science. To be sure, Hempel and Neurath themselves conflated these two notions until Hempel's recognition of its point in 1938 (Neurath's acceptance of it is less clear, but that will be addressed more thoroughly in the latter part of the present chapter).

The development of the debate raises two points for consideration that are taken up in the present chapter. First, there is a question of the credibility of both Hempel's emergence in the debates and his subsequent development of logical empiricism from the 1940s onward. Richard Jeffrey (2000, p. 7) writes, "In 1955 Hempel had still found it excruciating to recall those essays [1935], which were then twenty years old – perhaps because of the scorn and ridicule Bertrand Russell had heaped upon them fifteen years earlier [1940]." This may be so. However an analysis of Russell's paper indicates that, in so far as Hempel may have been bothered by the way in which his contribution to the debate influenced what came to be the mistaken received view of the Vienna Circle discussions, he need not have been bothered by the substantive criticisms given by Russell. As discussed, two key problems for Hempel's (1935) account are that he claims erroneously that Carnap and Neurath espouse a coherence theory of truth and, moreover, in doing so he (and as well as Neurath) conflated notions of empirical confirmation and logical truth. However Russell's own criticisms do not adequately address these problems as they fit within the philosophical project that the Vienna Circle took themselves to be engaged. Second, the examination of the protocol sentence debate begets the need to grasp how, if at all, the debate can be understood to have been resolvable. The protocol sentence debate provides a cornerstone of the history of logical empiricism, but, with a few notable exceptions, a general understanding of its outcome has been, as yet, rather oblique.

If Carnap and Neurath's views are understood as irreconcilable then one can frame two philosophical stances that emerge from within the left wing of the Vienna Circle. Friedman (2000, p. 39) has stated that it was interesting to watch Hempel turn in later life from the project of Carnapian explication to a form of Neurathian socio-historical naturalism. This claim somewhat obscures what may be viewed as a coherent development of Hempel's thought: it suggests that somehow the logical investigations into explanation, with which he largely made his name in the mid-twentieth century, were abandoned in favor of a more historical bent later on. Accounting for the conceptual resources emerging from the debate suggests that Carnap and Neurath were not at philosophical odds by the end of the 1930s, but that they attended to different methodological aspects of a common philosophical project. Against this backdrop Hempel's thought in his logic years can be understood to relate rather coherently with his later emphasis on Neurathian (and Kuhnian) ideas of the importance of historical investigations for understanding the structure, scope and limitations of scientific knowledge. Indeed, without obscuring his distinctive attentions to logical and historical aspects of science at different points in his career, one can see in Hempel's overall body of work a unifying methodology indicating the role of both technical (or formal) aspects and historico-sociological aspects of science.

This chapter examines the Russell/Hempel issue and attempts to flesh out resources for understanding the outcome of the protocol sentence debate. Those resources indicate that in spite of a lack of resolute conclusion to the debate, Carnap and Neurath need not be understood as engaged in distinct philosophical programmes. It considers, first, the problem with Russell's assessment of Hempel (1935). Second, it examines Hempel (1939) on vagueness and logic to indicate the way in which Hempel employed Carnap's distinction between formal and empirical aspects of a philosophical methodology. Third, it assesses the possibility of a resolution between the Carnapian and Neurathian approaches to philosophy of science.

Hempel and Russell's 'scathing' commentary

Russell (1940) treats the Hempel/Neurath view in critical and even denigrating terms. It can be broken down into three parts: (1) his statement of the problem of basic propositions; (2) his account of the Neurath/Hempel view of basic propositions; and (3) his criticism of their view. Russell (1940, p. 137) argues that basic propositions (protocol statements) are a "subclass of epistemological premises," which "are caused, as immediately as possible, by perceptive experiences." He suggests there are two parts to a discussion of basic observations (ibid, p. 138): first, "it is necessary to argue, as against opposing opinions, that there are basic propositions;" and second, "it is necessary to determine just what sort of thing they can affirm, and to show that this is usually less than common sense asserts on the occasions on which the basic propositions in question are epistemologically justifiable." A basic proposition has two properties (ibid): 1. It is caused by a perception. 2. It cannot contradict other basic propositions. Regarding the notion of 'cause', Russell (ibid) appeals to a loose conception of that term, suggesting that a proposition is caused by a perception if "it will be defended by the argument 'why, I see it!' or something similar."

Russell gives what he calls an epistemological definition of a basic proposition (ibid, p. 139): it "arises on occasion of a perception, which is the evidence for its truth, and it has a form such that no two propositions having this form can be mutually inconsistent if derived from different percepts." For Russell this is an epistemological definition because the truth of a basic proposition is based on its being caused by a perception. The perception and the proposition to which it gives rise provides an epistemological foundation for a system of knowledge based upon it. Recall from the discussion on Carnap in Chapter Two above, that in the *Aufbau* and Carnap's subsequent work neither basic propositions, nor the perceptions that cause them to be formulated are epistemological notions. Rather, they are pragmatic and are adopted conventionally: they must be in place within a constitutional system before epistemological questions can be raised meaningfully at all. This difference is important for considering the effectiveness of Russell's criticisms of Hempel and Neurath, but it will be helpful to continue a survey of Russell's paper to identify this distinction more clearly.

Russell (ibid, p. 137) argues against the notion that no knowledge can be gained from a single event. That view would hold that knowledge can only be arrived at based on "inductions from a number of more or less similar experiences," which Russell argues, "makes history impossible and memory unintelligible," since one would not have any origin for a set of basic propositions against which to assess the introduction of new propositions. The truth of a basic observation *can* and must be dependent upon a single perception.

Thus, according to Russell there are such things as basic propositions, which are caused by singular events. The subjective experience of a perception is evidence for the truth of the basic proposition. Truth is not determined based on the coherence of a basic proposition with a body of statements. It is crucial for Russell's perspective that basic propositions are understood to be empirical, since they are not determined logically, and epistemological, since their truth is determined on the evidence of singular perceptions and that truth affords an epistemological foundation for what one can claim to know. Logical rules are then formulated to ensure that basic propositions do not contradict one another.

Russell (ibid, pp. 139-40) remarks that Neurath and Hempel "deny that any set of propositions can be singled out as 'basic', or as in any important epistemological sense premises for the remainder." 'Truth' in their sense is strictly syntactical and not semantic: "a proposition is 'true' within a given system if it is consistent with the rest of the system." Moreover, a proposition deemed 'true' in one system can be deemed false in another. One cannot derive 'truth' from experience or perception of the world: according to Russell (ibid), for Neurath and Hempel, "the world of words is a closed self-contained world, and the philosopher need not concern himself with anything outside it."

Russell (ibid, pp. 140-41) lists what he takes to be key ideas found in Neurath (1934) and Hempel (1935).

- An assertion is called right when we can fit it in.
- Assertions are compared with assertions, not with 'experiences'.
- There are no primary *Protokollsätze* or propositions needing no confirmation.
- All *Protokollsätze* should be put into the following form: 'Otto's protocol at 3:17: [Otto's word-thought at 3:16 (In the room at 3:15 was a table perceived by Otto)]'.

He notes that in mathematics and logic, truth is a syntactic concept because "syntax guarantees the truth of tautologies." However, by espousing a coherence theory of truth, Neurath and Hempel conflate notions of empirical and logical truth. The problem with a coherence theory of empirical truth is that one is led to the view, "so it is, if you think so." In short, it gives way to a radical relativism that needs to be rejected if one is to account for objective aspects of scientific knowledge.

Most of these statements of the Hempel/Neurath view are wrong. More gracefully, one might say that certain Neurathian or Hempelian replies may suggest themselves. Before attending to those, it will be fruitful to consider Russell's criticism of what he takes to be their views.

Russell argues that Neurath is not a scientist; all knowledge begins with subjective experience; and Neurath's coherence view amounts to a basis in memory of past perceptions, rather than in immediate perceptions. Russell writes (ibid, p. 143),

> I think Neurath and Hempel may be more or less right as regards *their* problem, which is the construction of an encyclopaedia. They want public impersonal propositions, incorporated in public science. But *public*

> knowledge is a construction, containing less than the sum of *private* knowledges.

By Russell's interpretation (ibid), Neurath is not an empirical researcher so he can attend to compiling the research of "the best authorities." His encyclopedia can attend to opinions, but he does not address direct-observations because that is not the scope of his interest. Russell writes (ibid),

> The individual men of science, however, whose opinions are the encyclopaedist's premises, have not themselves merely compared other investigators' opinions; they have made observations and conducted experiments, on the basis of which they have been prepared, if necessary, to reject previously unanimous opinions.

Russell argues further that "all theory of knowledge" starts from the question, 'what do *I* know'? It does not start with 'what does mankind know'? The only way one can determine intersubjective knowledge is via "personal observation of what it says in the books it has written" and assessing evidence for the truth of claims in those books (ibid). All my knowledge must be based on my observations "through which alone I can ascertain what is received as public knowledge" (ibid, p. 144).

Further, according to Russell, Neurath's account of protocols is based upon memories of past events and that one must check the protocol against what others say. He writes (ibid, p. 146-47) that for Neurath,

> all empirical knowledge is based upon recollections of words used on former occasions. Why recollections should be preferred to perceptions, and why no recollections should be admitted except of thought-words, is not explained. Neurath is making an attempt to secure publicity in data, but by mistake has arrived at one of the most subjective forms of knowledge, namely recollection of past thoughts.

Thus, Russell allows that for the project with which Neurath is engaged, observations are not necessary. Neurath in this sense is not a scientist and is therefore not dealing with observations. By not dealing with observations, he ironically promotes an entirely subjective knowledge.

Moreover, any notion that empirical truth is based upon the coherence of basic observations with existing statements renders empiricism meaningless and history impossible. Russell examines a number of statements made by Neurath and Hempel (ibid, pp. 140,1):

1. Statements are compared with statements, not with experiences (N).
2. A protocol-statement, like every other statement, is at the end adopted or rejected by a decision (N).
3. The system of *Protokollsätze* we call true ... may only be characterized by the historical fact, that it is the system which is actually adopted by mankind, and especially by the scientists of our cultural circle (H).
4. Instead of *reality* we have a number of mutually incompatible but internally coherent bodies of propositions, choice between which is not logically determined (*logisch ausgezeichnet*) (N).

Russell then outlines objections to these claims (ibid, p. 147). How can one know that a claim like 'Neurath says so-and-so' is true? It could be that one knows it by reading words on a page, but this cannot be the ground for such knowledge on the Neurath/Hempel view, since, according to Russell (ibid), one first must know "the opinion of mankind, and especially of my cultural circle, as to what Neurath says." How does one know that more general opinion? If one asks opinions of other scientists, how does one know the truth of these opinions about the truth of whether 'Neurath says so-and-so' is true? One would have to ask further of opinions of other scientists and so on without ever having some account from which to assess the truth of not only the original claim in question, but all the claims after which one seeks answers regarding their truth status. Russell writes (ibid), "If eyes and ears do not enable me to know what Neurath said, no assemblage of scientists, however distinguished, can enable me to know."

Given the coherence view that Neurath is understood to hold, he writes (ibid),

> If we choose to attribute to him opinions completely different from those which he in fact holds, it will be useless for him to contradict, or to point to pages in his writings; for by such behavior he will only cause us to have experiences, which are never a ground for statements.

Hempel notes that Carnap and Neurath do not intend to say that there are only propositions and no facts, but that protocols are themselves facts, but Russell remarks (ibid, p. 148) that this "makes nonsense of the whole theory," since it suggests that empirical facts can have a different truth status for different bodies of statements: "owing to this, Neurath is an exile. He remarks himself that practical life soon reduces the ambiguity, and that we are influenced by the opinions of neighbours. In other words, empirical truth can be determined by the police." This fact is evidence for Russell that Neurath has abandoned empiricism altogether (ibid), "of which the very essence is that only experiences can determine the truth or falsehood of non-tautologous propositions."

Consequently, on Neurath's interpretation empirical statements are reduced to meaninglessness, but for Russell empirical propositions are about things other than just words. The meaning of an empirical statement cannot be understood merely as it fits in with a class of other statements, but refers to something external to language (ibid): "If I go into a restaurant and order my dinner, I [...want...] to bring about the presence of food."

Absent some grounding in perception there just is no connection between propositions and the world, for Russell. There is no way in which to account for the formulation of any basic propositions. So one has no propositions to start with against which to assess a new basic observation statement.

Russell seems to be making two points. Clearly he intends his argument to be about the causal origin of basic propositions in the world. However his final example indicates something slightly different (though related). We want our statements (and not just basic statements) to be true or false of the world. We want them to be about the world and not merely constructed relative to some system of statements. The epistemological concern regarding basic propositions then is how to secure some notion of truth of these propositions. On the other hand, the restaurant example is not about basic propositions but is couched in a more complex framework wherein one orders food. This scenario incorporates any number of basic and non-basic propositions. Russell wants his example to indicate what (he takes that) Neurath and Hempel's view cannot do: it cannot assess the truth of empirical sentences because they do not have an adequate basis for which to account for the truth of such sentences, since they hold a coherence notion of empirical truth. So Neurath and Hempel cannot make empirical claims at all, much less have a criterion of truth against which to assess if a basic proposition is true or not. Implicit in Russell's argument is that, absent *epistemological* worries about the relation between basic propositions and experience, Neurath and Hempel can have no criterion by which to assess basic observation.

There appear to be two principal arguments in Russell's conception of the problem of protocols. First, the basis for basic propositions is in perception and second, we want our basic propositions to be true about the world. If we articulate his worry in everyday language, we want our statement, 'it is warm out' to be true or false based on whether or not it is – loosely speaking, in fact, warm out. And it is precisely our experience that it is warm out that causes us to utter 'it is warm out'. When Russell orders dinner at a restaurant, he wants it to result in bringing food to the table for him to eat; he does not want to assess the way in which it fits into a coherent system of statements that could equally result in him getting a new car.

Perhaps the following example in support of Russell's worries might seem compelling. Children provide great illustrations. Assume your eight-month old has had a series of ear infections over the past few months and you want to be sure there has been no damage to the ear drum. You go in for tests at the Ear, Nose and Throat hospital on Gray's Inn Road. Your child sits in your lap facing a mirrored window and is fed audible and visual stimuli. When the child responds appropriately – that is, when the child's behaviouristic responses map to those taken to indicate that the child has heard the sound – a box in the direction of the sound illuminates a toy that dances around, providing a form of reward for looking in the right direction. Sometimes the sound precedes the illumination of the boxes, sometimes it is at the same time, sometimes it follows and sometimes there is no corresponding sound to the light. A series of tests may indicate that the child's behaviouristic responses do not map appropriately to behaviour that indicates he can hear and the technicians determine that there may be a problem and the child should come back for more tests.

On the other hand, as an engaged parent you may have constructed auxiliary hypotheses indicating a different explanation for why the behavioral responses did not indicate a proper correlation – that is, they suggest your child cannot hear. You might suggest that your son is curious and was not looking in the direction of the sound because he was anticipating that he was going to see a light come on from the other side. He therefore ignored the sound because he did not really care about it. You might feel that he is a bit nervous because he is in a strange environment and responds to your movements more readily since he is aware that you are a comfort to him. Perhaps he cares more about your approval as an indication that he is safe so he ignores all the stimuli around him, sucks on the plastic toy and looks back at you. Furthermore, he is only 8 months old and the behavioral indications he provides cannot be analyzed against responses of older children.

In this example, what one wants to know is whether the child can really hear. One does not want an interpretation of his behaviour that is assessed against a set of existing statements that might suggest he cannot hear, when in fact he can. This is one aspect of Russell's worry: without a causal account in perception of the origin of a basic proposition, one could conceivably make a statement deemed true within a language system, but empirically false.

This example is considerably more rich than worries about *basic* observations like "red, here, now" and presents a more complex scenario in which to consider the problem. In addition, the question posed by Russell surrounds the causal relation between one's own subjective perceptions and corresponding propositions about them. This example therefore might appear to be not entirely relevant because the concern is over whether the statement 'the child can hear' is true or not, and not any basic statement caused by a subjective perception. Nevertheless it illustrates an important aspect of Russell's worry and underscores why one might share the sorts of epistemological worries expressed by him.

Another aspect of the Russellian point is that one will not deny one's own experience in the face of the scientific experts challenging that experience – that the child is exhibiting behavior inconsistent with publicly acknowledged correlations between behavior and attributed physical occurrences inside the child's body –i.e. he can't hear. Furthermore, what one is worried about is whether the child can hear. You want to know if his hearing is actually functioning properly.

Two comments might be made about Russell's criticisms of Neurath and Hempel. These comments indicate why Russell's claims about Hempel and Neurath are incorrect. First, he seems to have missed a key point of the protocol sentence debate by failing to recognize that their empiricism does employ a notion of causality to generate basic observation statements. But that causality is in no way an epistemological notion for them because perceptions are not utilized to undergird claims to epistemic privilege of protocols. Second, though Russell is correct to argue that their conception of truth conflates notions of truth and acceptance, by 1940 when his paper was published, that error had been rectified *in virtue* of the development of the protocol sentence debate itself.

Hempel repudiated his confusion explicitly and, as will be seen in the subsequent section, employed the distinction between logic and psychology in his philosophical arguments in 1939. Russell's paper, at best, arrives too late. At worst, it is beside the point.

It will be helpful to expand on these points. First of all, as noted, Russell claims he must argue that there are basic propositions. Moreover these propositions must employ a notion of causality. And that notion is an epistemological one: perceptions that give rise to basic propositions are evidence for the truth of those propositions, which undergirds Russell's empiricism as a justification for scientific knowledge rooted in sensation. On the other hand, in the protocol sentence debate, basic propositions are adopted conventionally, and as part of their empiricism are causally rooted in experience. But there is no attempt to justify rooting scientific knowledge in sensation, or to legitimate, on the basis of some prior epistemic commitment, the primacy of the selected class of basic protocols. As a conventional fact, selecting experience to generate protocols characterizes their empiricist inclination when constructing a system of knowledge. As identified in Chapter Two above, this distinction is one of the key features brought about by Richardson who distinguished Carnap's from Russell's epistemological programmes. For the Vienna Circle, one could not give meaning to worries outside an adopted framework. Epistemological concerns thus could not be raised until a framework was in place that included a class of basic protocols and a system of logic, which would give rules of inference for non-protocols in the scientific language.

Neurath and Hempel, following Carnap, deny that the domain between experience and the basic sentences one might form about that experience is in any way an epistemological domain, but a pragmatic one. As discussed above, Hempel (1983) clarifies that a causal element is necessary in the generation of protocols, but that any causal connections employed between experience of objects in the world and protocol sentences are adopted on the basis of convention. Experiences come from somewhere (the 'world'), but Neurath and Hempel deny the claim that there is anything meaningful to be said about the world beyond what can be formulated within the framework of physicalism. There is no question for Hempel and Neurath whether there "are" basic propositions. The entire protocol sentence debate is premised on the notion that there are protocols. The question they were interested in centered on the form and status of those propositions. Any question in the example of whether the child can hear cannot be answered with respect to appeal that he "really" can or cannot hear. Assessment is given by weighing empirical statements for or against the claim in light of the larger body of evidence.

Second, as is now evident, one of the issues of the protocol sentence debate was Carnap's developing distinction between logic and psychology – and the distinction between truth and acceptance that results from distinguishing formal from empirical questions of scientific knowledge. Between 1935 and 1940, it became clear that Hempel and Neurath had conflated truth and acceptance. Hempel came to share Carnap's explicit employment of the distinction. Although Neurath did not share Hempel's explicit recognition of Carnap's distinction it was clear that he was happy to do without using a notion of 'truth' at all, except that to employ it was an attempt to do justice to how he understood the term to have been used traditionally. As discussed above, Tarski's definition of truth came to be a pivotal feature of the developing logical empiricism at the time. Thus, Russell's criticism of Neurath (1934) and Hempel (1935) appears to have arrived late, since Hempel, certainly, would have agreed that he and Neurath had been formulating a theory of acceptance, rather than a theory of truth.

Moreover, the protocol sentence debate was a vehicle by which logical empiricism came to formulate the distinction between truth and acceptance. If one takes Carnap's view seriously, then Russell himself appears to continue to conflate truth and acceptance by ascribing epistemological relevance to the causality underlying the source of basic propositions. For the Vienna Circle, causes that bring about protocols reside in the domain of psychology: psychologists describe a class of statements that are employed to account for that causal connection. (For example, in general individuals say the colour of an object is 'red' when they see an object exhibiting certain colour characteristics.) Assuming, via convention, that class of statements and a logical framework, one then worries about the form of those statements and how they relate to non-protocols in the physicalist language.

For these reasons, Hempel cannot have been troubled by Russell's paper – at least he had no reason to be troubled based on the paper itself. To be sure, Russell was correct that Hempel's view in 1935 was problematic, but his criticisms came long after those problems had been made explicit within the protocol sentence debate itself.

Hempel on Vagueness and Logic

In 1939 Hempel published a paper entitled "Vagueness and Logic," wherein he aimed to examine what follows from the presence of vagueness in scientific and everyday concepts with respect to questions of logic. He lays out three goals (1939, p. 163): (1) outline the meaning and logical status of the concept 'vagueness'; (2) question whether logical terms are free from vagueness, and whether vagueness influences the validity of logical principles; and (3) look at the possibility of reducing vagueness of scientific concepts with appropriate logical tools.

Examining this paper will serve two purposes for the present interest. First, it indicates the way in which Hempel's work already by 1939 embraces both Carnapian logic of science and Neurath's notion of *Ballungen*. It embraces the former in the sense that it confronts the question of precision in employing tools of logic, and the latter by endorsing that all statements of science are necessarily imprecise, or vague, on some level because of the different usage of terms among various user groups. Hempel looks at the question of imprecision in logic as a tool, while asking how logic itself can be used to reduce imprecision (problems of the logical aspects of science). And he indicates the source of ever-present vagueness in concepts by accounting for the role of users of those terms (problems of the historico-sociological aspects of science). Under Hempel's employment, one can argue that Carnapian and Neurathian programmes are not two fundamentally different philosophical standpoints, but different methodological aspects of an inquiry into science. Second, it vindicates the claim that Russell's scathing criticisms in 1940 could (or at least should) not have bothered Hempel terribly because those criticisms both missed a key point of the protocol sentence debate and wildly failed to acknowledge the dynamics of the debate that took members of the left-wing of the Circle far beyond the question of whether theirs was a coherence theory.

This section first surveys Hempel (1939), and second, discusses its relevance to the Carnap and Neurath positions that emerged from the protocol sentence debate.

Hempel takes his cue from Max Black (1937) as his starting point for a definition of 'vagueness', where that concept is distinguished from (Hempel 1939, pp. 163) "generality and ambiguity."[1] A symbol for an object is vague when it is not possible to determine whether or not that symbol applies to an object in a given instance. Vagueness (1939, pp. 164) arises from different usages of a term among various user groups of a language. Thus, he notes (ibid), a deep-sea organism not clearly belonging to plants or animals may be designated either category depending on members of user groups asked: "If several observers are asked whether the term 'plant' does or does not apply to that object, there will be a certain number m of affirmative answers and a certain number n of negative ones."

Hempel (ibid) notes that Black attempts to formulate a "numerical determination" of a term's relative vagueness through the "consistency of application" of a term T to some object x [C(T,x)]. Briefly, the term 'plant'

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[1] For Black (1937, p. 430), 'generality' is characterized by the application of a symbol to a number of objects, while 'ambiguity' is characterized by a symbol having a number of meanings. Vagueness thus differs since a vague symbol will have only one meaning, but it is not clear when that symbol applies or not to a particular object.

applies to an object with a relative consistency that can be defined by the "limit of the ratio m/n, when the group of observers is more and more extended, and the number of decisions made by its members is indefinitely increased." When a symbol T may be applied to several objects, then these objects may be ordered along an axis according to the decreasing consistency of application of T – instances in which groups affirm T on the left (assign 'plant') to those denying T (assign 'animal') on the right of the horizontal axis. This produces a curve that, according to Hempel (ibid), Black "calls the *consistency profile for the application of the vague symbol T* to the given series of objects." The steepness of the curve in the middle of the ordering gives the relative consistency (ibid, p. 164): "the steeper the drop, the smaller the number of doubtful cases, and therefore the smaller the vagueness (the greater precision) of the symbol."

Hempel (ibid) notes a gap in Black's designation of 'vagueness' because the notion of steepness of curve has no metrical scale against which to place the axes of the system: for the horizontal axis, "the objects of the series have simply been put into what may be called a topological order.... But there is no additional criterion stipulating under what conditions two objects in the linear arrangement are to represented as equidistant." One knows only that one object precedes the other and not the extent to which the vague term is applied to each relative to the others. Steepness therefore cannot function to determine vagueness in Black's account.

However Hempel's worry is not over the general idea of Black's designation, but his particular technical expression. Hempel (ibid) claims to avoid the problem with Black's account by modifying the initial definition of C (T,x) as the "(limit of the) ratio m/m+n." C is thereby restricted to values between 0 and 1, with vagueness of a symbol falling around ½. Hempel defines precision (pr) then as the extent to which a symbol differs from ½ and vagueness (vg) therefore is, according to his definition, 1 – pr. Hempel's expression of precision as applied to a series S is not important for the present discussion. However, for the initiated it looks like the following:

$$pr = \frac{4}{n}\sum_{r=1}^{n}[C(T, x_r) - \tfrac{1}{2}]^2.$$

Hempel (ibid, p. 166) argues that the study of vagueness belongs to a general theory of "the process in which something functions as a sign" – or 'semiotics'. He (ibid) notes that according to C.W. Morris (1938), 'semiosis' is the process in which something comes to function as a sign and involves three correlates: "the sign, the subject matter it refers to, and those who use the sign or respond to it." Branches of semiotics may involve exclusive aspects – i.e. syntax, which refers to the formal properties of a language, or semantics, which examines rules for the relations between symbols and designata. However, a consideration of a concept like 'vagueness' must "take into account all the correlates involved in semiosis." 'Vagueness' thus does not designate merely formal syntactic properties of a language, nor rules that link symbols to objects, but designation of such concepts requires (Hempel, 1939, p. 166), "reference to the symbols, their users, and their designata."

Hempel (ibid) argues that 'vagueness' is thereby an empirically grasped concept, since it is an empirical question how user groups apply a term (ibid, p. 167): "Statements about the degree of vagueness with which a term is used by a certain group of persons, are obviously empirical in character; they refer to the speaking or writing behaviour of certain individuals; their establishment requires empirical investigation." This feature is important to emphasize in Hempel's paper because it aids to clarify certain aspects of the protocol sentence debate.

It will be fruitful to consider Hempel's example of weight and vagueness and examine whether logical symbols are characterizable as vague. He suggests that a set of balances indicating weight can function as an analogy, but also count as a case of genuine vagueness. A set of balances produces cards indicating weight – one weighs an object and the scales print a card giving the measured weight. Hempel (ibid, p. 167) writes, "These balances may be regarded as a group of individuals speaking a very simple common language. The terms of this language are vague, just like those occurring in any historical language." They are vague in two ways: first, if a scale uses sufficiently small units of measurement, and a load is weighed several times, there may be random variations. Second, if a load is weighed by different scales in the set, there will be likewise a variation in results. Taking the scales as an analogy he writes (ibid),

> Some of the 'speakers' of that group will perhaps 'apply to the given load the term "60 kgs"', others, by yielding a different statement, will 'assert that that term does not apply'. Thus, a certain number m of positive and a certain number n of negative 'answers to the question as to whether the term '60kg' applies to that load' will be obtained....

Vagueness in this analogy is exhibited by deviations of results, which is (ibid, p. 168), "common to all measuring instruments, including living organisms." The point one might take for the present concern with respect to the protocol sentence debate is that '60 kg' may be defined precisely. Moreover, rules of application may likewise be formulated that tell precisely when an object weighs 60 kg (when it is placed on an accurate scale under certain conditions etc.). However, to articulate the precision of a particular statement 'this bag of flour weighs 60 kg', one must account for the context within which the statement has been applied according to specified semantic rules. That is, to account for random variations of measurement among various scales, one must account for the conditions within which the scale was used etc. Consistency of application of the term '60 kg' as spelled out by Black and clarified by Hempel will indicate the extent to which '60 kg' is a vague term applied in a given context.[2] If the scales are taken to be an analogy for human individuals, the degree of vagueness of a concept applied in a given context, one needs to articulate the conditions under which some term T has been applied to an object. This will of course involve an account of (what Carnap calls psychological aspects of) beliefs and assumptions of individuals employing that concept.[3]

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[2] One may also put it in terms of precision: consistency of application of the term '60 kg' renders it more or less precise in its particular application.

[3] Hempel is not attempting to argue the Neurathian line that vagueness is present in all scientific statements. That ineliminable *Ballungen* are present throughout science (via the employment of an unclear mix of vague and precise terms) is a

Can logical symbols be characterized as vague or do they constitute a class of precise symbols? Are descriptive symbols the only sort that exhibits a relative degree of vagueness? Hempel proposes that one must examine vagueness further to provide an answer (ibid, p. 169). He has characterized vagueness to this point as variations in use of a term: sometimes it will be applied to an object and sometimes not. Certain variations of use would depend upon (ibid) "observers' perceptual apparatuses." For example two individuals under the same circumstances may apply the term 'yellow' to different objects, but this is not an instance of vagueness because the individuals may have different perceptual apparatuses. On the other hand, he notes (ibid, p.170), 'yellow' may be employed as a vague term if one has learned to apply it "in a rather liberal way so as to cover, say certain shades which the other observer has been to term 'green'." Vagueness here arises in the "speaking habits" of users, since there is no strict line distinguishing yellow from green.

Descriptive terms assigned to objects would fall into this latter category, since they are mostly learned by "ostensive definitions" which allow for variations in the application of defined terms. Moreover (ibid), for the same reasons, logical terms like, "not," "and," "if—then," are not "absolutely precise." Usage of these terms are learned with examples and "illustrative comments which do not prevent the occurrence of variations in their application" (ibid). No terms therefore in interpreted languages are free from imprecision.

However, Hempel (ibid, p.171) remarks that artificial languages[4] are immune from such vagueness because they have not been given any interpretation: "the question of vagueness does not even arise since one of the correlates of the semiotic relation of vagueness is missing, namely the designata of the linguistic terms." Thus artificial languages are immune to vagueness, but

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starting point. The question for Hempel (ibid) is the extent to which "suitable logical devices" can reduce vagueness.

[4] These artificial languages would be of the sort developed by Carnap in his attention to logical relations within formally constructed languages.

also they are not about anything empirical. To be empirical they must be interpreted in one form or another, but that is to introduce vague concepts to these languages. The vagueness of the interpreted language does not entail the vagueness of the abstracted, artificial language (ibid, p. 176): there is,

> no place for a purely semantical concept of vagueness.... Thus the question as to the influence of vagueness upon the validity of the principles of logic does not arise on the purely syntactical and semantical level of investigation, and no modification of the logical symbolism is necessary.

Can modifying the logical structure of a given language decrease vagueness? Hempel argues 'yes' (ibid, p 177): by the introduction of gradable concepts ("harder than"), which are more precise than natural language terms like "hard." Names which designate properties, like 'hard', can be replaced by those which designate relations, like 'harder than'. Such concepts exhibit a different logical structure than those designating properties. Hempel writes (ibid, p. 178), "This is the sense in which vagueness may really influence logic: it may suggest (though not require) a modification of the logical structure of scientific concepts."

In this section Hempel notes that 'vagueness' is an empirically grasped concept because it belongs to the domain of semiotics, which involves syntax, semantics and an account of user groups' applications of terms applied according to the semantical rules. This underwrites both Carnap's claim of the indispensability of the formal/empirical distinction and Neurath's claims about ineliminable imprecision of scientific statements at their point of application. Hempel (ibid, p.174) assumes for his analysis Carnap's crucial distinction: "the question of logical principles arises, strictly speaking, only on the abstract level where language is dealt with as a theoretical semantico-syntactical system; the questions concerning vagueness, on the other hand, refer to language as a form of behaviour." It is because of this distinction that one may accept Tarski's theory of truth, while indicating Neurath's point that its usefulness to actual scientific practice is negligible.

Hempel's scale example demonstrates the point of the formal/empirical distinction. The weights printed onto cards by the scales indicate vagueness, since given small enough units of measurement they will produce varying results. From a contemporary perspective a scale example may seem rather primitive, since it is now possible to make incredibly precise measurements of weight. However, this is perhaps the point: how can one make existing tools more precise? Can one expand the systematic power of technical tools to attempt a broader application of their conclusions? Carnap's attention to the logical aspects of science can be understood in precisely this fashion: refining the logical machinery to indicate how much can be done with it. Thus, Carnap can start from Neurath's view that all statements consist of *Ballungen* and leave an open question the extent to which one may continue to clarify those *Ballungen*.

As an instance of refining technical machinery, Hempel (1939) asks whether logical statements themselves can be vague and whether the introduction of vague concepts affect the validity of logical operations. His analysis suggests that logical structures are not vague: that vagueness only emerges upon the interpretation of those structures into the empirical domain. Thus, according to Hempel, validity itself is not affected by vagueness, but logical formulations may aid in reducing vagueness of empirical statements on the grounds that it provides a precise inferential framework with which to clarify the structure of empirical statements. One can never be rid of vague statements in empirical science, but in the attempt to clarify them, Hempel argues that vagueness does not reside in the inferential framework.

Hempel's analysis indicates a further way in which he might contribute to an understanding of Carnap and Neurath's rather confusing connections at the end of the protocol sentence debate. Carnap, of course, was interested in the extent to which one might construct a robust formal language to highlight the structural dynamics of empirical science. One can see an application in Hempel's

1939 paper: if one can establish a consistency of application of a term to an object to a relative degree of precision, one may be able to systematize that application in a purely formal structure, and thereby afford an objective criterion for its application. The structure would, of course, be revisable given subsequent empirical studies of usage. However, in this sense a term T may be applied to an object x with a high degree of consistency such that it is fruitful to systematize this application by representing it formally. In the formal language one may call Tx a true statement. The robustness of semantical language (which gives rules of application for the object language) will determine the extent to which the true statement may be applied empirically. However, the formal representation of the application of the concept T to object x, would be generated from its consistency of empirical application among various user groups.

Resolving the protocol sentence debate

One may now consider the outcome of the protocol sentence debate. This section takes up Thomas Uebel's claims that the dispute between Neurath and Carnap can be resolved – or at least a resolution is not ruled out in principle. This affects how one understands the difference between their views. Thus this section will have three parts: (1.) survey Uebel's attempts to reconcile Carnap and Neurath's disputes; (2.) assess Uebel's claims; (3.) suggest a reading that builds on Uebel's work to understand the relation of their respective views.

Uebel (2001, p. 211) writes that the emerging differences in the debate between Neurath and Carnap call into question whether they ought to be considered as still sharing a common programme. For Uebel (ibid) this is an important question, not as "a case of sweeping a particularly dusty and obscure corner in the ramshackled mansion of logical empiricism that might as well remain undisturbed," but because he is interested in the nature of the legacy of logical empiricism. For the present inquiry, this appears to be a crucial question for which to forge some clear answer because the historical legacy of Hempel and its reinterpretation in the following chapter rides on both the relative connections one can locate between the Neurathian and Carnapian views and how Hempel may have understood those connections. Uebel's exemplary paper provides occasion to articulate how Neurath and Carnap can be understood as connected, without glossing their important differences.

Uebel examines three differences between Carnap and Neurath's views (ibid). First, the rational reconstruction of Carnap "recognized no bounds," other than logical incoherence, to conceptual considerations, while the naturalism of Neurath restricted inquiry to "humanly realizable languages and schemes of inference." Second, he notes that the "hierarchical model of the relation of the individual sciences" employed by Carnap stood in opposition to Neurath's encyclopedic model, which emphasized the interconnectedness, and consequent incompleteness, of the whole of individual sciences. Third, Uebel points out a distinction between "Carnap's enthusiasm for Tarski's theory of truth" and Neurath's "increasing scepticism" regarding both the possibility of semantics (given Tarski's definition of truth) and formalism in general.

Uebel (ibid) contrasts Carnap's rational reconstructionism from Neurath's naturalism. First however, he notes that the latter's view is distinguishable from Quinean naturalism, which is characterized in part by a rejection of the analytic-synthetic distinction (ibid, p. 212): "Neurath accepted Carnap's treatment of analyticity in [Carnap (1937)], going on to deny both that analytic statements are epistemologically foundational or 'certain' and that we ourselves could ever reach a 'final verdict' about whether a statement was analytic or not."[5] Uebel (ibid) concludes that Neurath then accepts the analytic-synthetic distinction "as a concept of logico-linguistic analysis." For Neurath it was not understood as a concept meant to bolster a foundationalist epistemology, as it was for Quine.[6]

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[5] See Neurath (1934, p. 104).

[6] In the same passage, Uebel notes that Neurath embraced the notion of analyticity in Carnap's syntax definition, holding it to be methodologically sounder than Carnap's own later semantic definition. What this suggests is that Neurath perhaps did not understand the problems with Carnap's syntactical programme as discussed above. Moreover, even though Neurath eventually accepted Tarski's semantic definition of truth as correct, he failed to recognize

Uebel (ibid) thus raises the question about what sort of naturalism Neurath espoused, noting more precisely some differences with Quine. First, in rejecting a hierarchical conception of the sciences, Neurath rejects the primacy of physics, taking the "social sciences seriously." Quine, Uebel (ibid) notes, sided with Carnap. Second, Neurath, in agreement with Carnap, "does not subscribe to the realism that typically characterizes Quine's, even more so the contemporary correspondentist naturalism." Third, as mentioned, in contrast to Quine, Neurath accepts the Carnapian notion of analyticity as a codification "of linguistic conventions." Uebel (ibid) remarks that this is a "carefully circumscribed acceptance of apriorism."

As Uebel (ibid) notes, Neurath's naturalism thus may be characterized (perhaps remarkably) by an acceptance of apriorism, since there must be some set of initial concepts from which to launch a methodology of analysis of science. Those concepts are given ultimately by a decision: there are no ontological commitments to reality underwriting any such selection. The present thesis indicates that for logical empiricists the decision to select a methodological approach to the analysis of science must be based, first on the formulation of a language purged of metaphysical concepts and second, on practical considerations for the fruitful application of such a methodology. How well does the methodology do justice to the concepts employed in the sciences? Is it governed adequately by logical constraints given according to the logical framework within which the methodology is applied? Does it lend itself to metaphysics (a step backward in the developments of the logical empiricism, according to Neurath)? There is no claim in Neurath's philosophy of science that a priori principles are underwritten by appeal to any ontological commitment regarding the state of the world. For Neurath, there can be no ontologically justified claim to such a relation between the selection of a methodology (and the a priori principles therein) for analysis of science and any commitment to the

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(or, perhaps, simply rejected) the way in which that definition leads to the semantic programme developed by Carnap.

way the world is.[7] Nevertheless, it is evident that a starting point is necessary, and although that starting point may be revised, it is taken to be a priori in the methodology within which it is employed. Protocol statements just are the basic building blocks for a logical empiricist methodological approach to analysing the structure of science. This is perhaps a contentious claim and, as seen in Uebel, raises the question of whether Neurath's view would even count as naturalist. Neurath certainly does not advocate naturalism of the Quinean sort. Uebel writes (ibid), "Just as it is a mistake to read Carnap as an epistemological foundationalist and more or less traditional empiricist, so it might accordingly be

[7] One might argue, as Nancy Cartwright (2007) does, that one's methodology cannot be separated from one's metaphysics – or that metaphysics and methodology 'go hand in hand'. However, the present statement of Neurath's view need not contrast with such a view, given that 'metaphysics' is a rather ambiguous term. To assert that metaphysics goes hand in hand with the selection of a methodology could be to argue one of three things. First, it could be to argue that one cannot have methodology without metaphysics: that one simply cannot be without metaphysical commitments and purport to construct a methodology without acknowledging that there are commitments that influence such a methodology. This view is rather strong and would fail to recognize that refining a methodology of analysis may be akin to sharpening a tool. One may sharpen an ax, and be very good at it, without necessarily being committed to the notion that it is necessarily the right tool to be using in a given scenario. The second conception might be to say simply that insofar as metaphysics must inform methodology, there are reasons one has for selecting one methodology or not and those reasons cannot be given justification by the methodology itself (or in the particular historical debate with which this thesis thus far has been concerned, by the physicalist language employed in science). However, this is not to disagree with Neurath or Carnap, but to employ the notion 'metaphysics' in a different manner. For the Vienna Circle philosophers, 'metaphysics' indicated a domain of concepts that could not be given adequate formulation in the language of physicalism, but traded as though such concepts provided explanatory import within physicalism. To argue that metaphysics and methodology go hand in hand in the sense that there must be reasons for adopting one's methodology is to point to the psychological aspects of theory acceptance. For example, one may in fact have realist commitments about science, but those commitments cannot explain the success of science for Neurath and Carnap. Rather they can explain why a given set of individuals accepts the explanatory adequacy of a scientific theory over certain domains (even as yet unempirical domains). A third argument might be more generally that one cannot justify the employment of a methodology without philosophical reflection upon the grounds for such employment. Philosophy in this sense would involve an exploration of the limits of employed methodology in science. As such it characterizes a significant aspect of Carnap, Neurath and Hempel's approach to scientific knowledge.

a mistake to read Neurath's naturalism as a mere anticipation of Quinean, even less of contemporary philosophical naturalism."

Uebel (ibid) is quick to note that though Neurath accepted Carnap's notion of analyticity, it "does not mean, of course, that Neurath's own theorizing made much use" of it. On the other hand, Uebel notes that Neurath's view leads one to see the difference between his naturalism and Carnap's rational reconstructionism "more along the lines of a division of labour than as an outright opposition." Whereas Carnap focused on the construction of logico-linguistic frameworks and constraints the rules of such frameworks might place upon acceptance of a given theory within them, Neurath "explored patterns of theory acceptance," in the history of scientific practice – the historical (cultural, sociological) conditions under which a particular theory actually came to be accepted. That is, Carnap attended to the logical aspects of science, while Neurath to historico-sociological aspects. And Uebel (ibid) reminds the reader that both Neurath and Carnap explicitly stated the need "for both types of metatheoretical approaches to science."

One may wonder still how 'naturalism' might be an appropriate moniker for Neurath's thought – that perhaps to emphasize an a priori element in his thought is to miss a defining aspect. However Uebel (ibid, pp. 212-13) points out that in Neurath's appeal to a priori concepts of a methodology, "no substantive a priori determinations of the nature of any subject matter are involved." Neurath rejects metaphysics and (ibid), "the Kantian project of proving the possibility of knowledge from some transcendental condition,"[8] arguing (in a familiar phrase in Neurathian literature), "The possibility of science becomes apparent in science itself" (ibid, p. 61). There are no substantive first principles from which to assess science. One elects to utilize certain principles on the grounds discussed above. Moreover (ibid, p. 213), any epistemological questions cannot be assessed from "substantive a priori determinations of what justification and knowledge are *supposed to be*" [italics added].

[8] See Neurath (1931a, p. 67).

In short, both Neurath and Carnap accepted the latter's concept of analyticity and "his related conception of the relativized a priori," but these concepts related to conventions, to which "the attributes 'true' and 'false' do not properly apply" (ibid). Uebel writes (ibid), "Neurath's acceptance of the analytic/synthetic distinction thus pertained to a tool for the analysis of scientific theories, not to some set of unrevisable conceptual truths." He concludes, "Joined with the denial of ontological realism, naturalism in Neurath's hands retains much of the conventionalist impulse that also characterizes Carnap's famous principle of tolerance in language construction. What we find so far is not so much opposition than potential for the fruitful division of labour."

According to Uebel (ibid), the second difference between Neurath and Carnap was their views concerning the hierarchy of the sciences: "all-important for [Neurath] was the connectability of theories of different disciplines for purposes of prediction," while Uebel attributes to Carnap a more hierarchical view.[9] Thus, unity for Carnap was understood to be characterized by the reduction of empirical statements to a formalized metalanguage robust enough sufficiently to account for definitions and semantic rules involving those statements. For Neurath unity was characterized by the encyclopedia: joint efforts of all the natural and social sciences operating under a universal jargon,

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[9] One needs to be clear here on Carnap's view: in what way did he advocate the "primacy of physics?" Does Uebel adequately characterize Carnap's view? For example, if Carnap's was a conventionalist view, he might be seen to take the structure of successes in physics and attempt to see how they work in the sciences more generally. In that sense his 'hierarchical' view may be understood as adopted for pragmatic reasons. There is a clearer methodological structure in physics and the way logic and mathematics have been successful there, they may likewise function in other aspects of science with an adequate metalanguage detailing the rules of syntax and semantic rules of application of that syntax. Also, Uebel cites Carnap (1995), which is a reprint of Carnap (1934a), but that is not the middle-to-late Carnap to whom Uebel is comparing Neurath. Richardson has shown that Carnap changed his view substantially after the 1934 publication and after accepting Tarski's theory of truth. By the late 1930s Carnap had rejected epistemology in favor of the logic of science, but how does this rejection connect to his alleged view of the primacy of physics? Perhaps it is that physics provides a domain wherein one can determine the successful application of the logic of science. The fruitful application there provides a basis from which to construct a robust metalanguage that possibly can apply to ever-expanding domains within science.

wherein all concepts could be given physicalist expression. The difference here ties to their respective views on the explanatory power of formalized languages to explain theory acceptance in science. Of course, Carnap held out that the tools of mathematics and logic, given a sufficiently robust metalanguage, could possibly account for the language of science generally (and thus the acceptance of theories in the development of empirical science). Uebel (ibid, p. 214) points out that for Neurath, Carnap's logic of science was too counterfactual to account for empirical science. For Neurath, to account for empirical science, one can never appeal to primitive exact formalizations: science in practice always operates with some level of imprecise concepts that will be missed by precise formalizations.

The difference thus is tied to their respective conceptions of unification of science: Carnap's in a potential reduction of the sciences to mathematics and logic employed in physics, and Neurath's (ibid), "via the partially procedural conventions of theory acceptance." Thus, Uebel claims that Carnap and Neurath may not be understood only as differing methodologically, but over their fundamental conceptions of unity in science. Uebel (ibid, p. 214) writes that given Neurath's view, unity is not to be found via mathematical and logical formalization of natural language, but in both coherence of a statement with the body of scientific statements and the behaviour of scientists, who provide the "conventional determinations of the epistemic goals for the theory in question" (ibid).

It should be noted here that both Carnap and Neurath emphasized the constructive nature of their methodologies. As Uebel writes (ibid), "the point of their conventionalist take on theory construction was the denial of epistemological realism, the pre-givenness of epistemic norms and standards." Unity is not given by some pre-existing state of the world. It is found in the constructed methodologies with which they attempt to account for the world. Moreover, they both held that their programmes were subject to change, revision or rejection. As Uebel points out (ibid, p. 215), "Neither of them would have to claim that the features they were concentrating on were the ones that *really* accounted for [unity of science] all along, nor that they would hold with equal weight of all scientific disciplines."

Uebel here suggests that their different approaches "threatens to be a serious faultline" between their respective projects (ibid, p. 214). However, he responds that because they were not attempting to find a pre-given notion of unity, but rather are understood as formulating criteria "that we might be persuaded to accept," there is no principled objection to viewing them as part of the same project (ibid, p. 215).

Uebel's argument is worth clarifying here. He clearly lays out the way in which Carnap's programme is neither appealing to, nor available to Neurath given the latter's emphasis on procedural conventions of science. Uebel lays the two views out as distinct, but then holds that they are not incompatible because of the open-endedness of their viewpoints and the notion that what they were doing was attempting to construct a methodology. On the other hand one might argue that they are focusing on different aspects of the same notion, but Carnap is searching for a systematic approach that, among other things, could render Neurath's holistic orientation more precise. In a Carnapian vein one might ask whether one can locate formal properties of the social and pragmatic conditions of adopted procedural conventions in science. If so, can these properties be generalized to apply across a sufficiently wide domain, such that one may take a systematic approach to the analysis of those conventions? The answer here should be a qualified yes: yes because Carnap believes it is not ruled out in principle, and qualified because it is determined based on the successful applications of the formal frameworks in empirical domains.

Thus one might argue something slightly different from Uebel. He suggests their views are compatible-in-principle because Carnap and Neurath are open to revising their positions. On the contrary, one might not worry about compatibility at this level: about whether the Neurath and Carnap views can be brought together – as a historical fact, Uebel agrees, they were not. Rather one might argue that they are compatible because they attend to different aspects of a common philosophical programme. Carnap is working on a "more systematic" account of something like Neurath's social and pragmatic aspects of science (more systematic in virtue of his attempts to formalize the social and pragmatic – for better or for worse).

Uebel (ibid, p. 215) locates a third difference around "Carnap's championship of Tarski-truth and Neurath's vehement opposition." According to Uebel, the differences here would preclude seeing them as working on two aspects of the same programme. And Uebel argues that it is not enough to "rectify" Neurath's "blunders" (i.e. his conflation of 'truth' and 'acceptance') to overcome their differences. The issue of Tarski's theory of truth has been discussed above, and the view argued in this thesis has been spelled out there. For the moment it will be prudent to continue the present survey to indicate how Uebel treats the issue so as to facilitate an assessment of it.

Uebel is sympathetic to certain revisions of Neurath's thought that do justice to his position (ibid). However Uebel does not think such revisions necessarily serve his task of forging reconciliation between their apparent disparate views on truth and consequent views regarding the nature of an appropriate methodology to assess the language of science. He suggests that on the basis of their alleged stances on Tarski's theory of truth, their views may not be reconcilable after all. (Of course, Carnap endorsed Tarski's definition of truth from 1935, while as has been seen, Neurath initially rejected it outright.) Uebel's question then regards Neurath's first worry regarding the programme of semantics (whether he accepted Tarski's theory of truth) and concludes that he was worried principally about a retreat to metaphysics.

Next Uebel locates Neurath's second criticism that semantic terms apply to "precise logical calculi" but not to the language used in empirical science. Carnap's semantics cannot apply to actual scientific language, since the rules of the formal construction (of an idealized representation) of a language, are routinely violated in the empirical domain. Thus, Carnap's logic of science cannot tell about empirical science. Of course, as discussed above, this reflects their respective views on the usefulness of formalizations: Carnap held that the potential success at representing empirical science formally was not ruled out in principle.

Uebel (ibid, p. 216) confronts a third worry that their two views reflect different conceptions on the nature of language: as a calculus or a "universal medium." Neurath would be understood to have held the latter view, holding that one cannot step outside language to assess it. Carnap's semantic view on the other hand would hold that one could apply a given language via a metalanguage that gives both all the definitions and rules internal to the language in question and further rules for how that language is meant to be applied. Uebel (ibid) writes, "The universalist view would seem to forbid metalinguistic semantic talk, the calculus view allows it."

Uebel's reply to this worry is that one may hold a mixed view. And Neurath appeared to. Uebel (ibid) writes that Carnap was a calculist with respect to constructed formal languages, while Neurath was a "universalist *vis-à-vis* natural language." On the other hand (ibid), Uebel notes that Neurath advocated Carnap's logical syntax programme, which suggests he was also a calculist with respect to formalized languages. The "crucial question" for Uebel is whether Carnap was a calculist with respect to natural language, to which he replies 'no' (ibid, pp. 216-17): Carnap always focused on specific formal languages. Moreover, notes Uebel (ibid, p. 217), Carnap "shared Quine's doubts whether natural languages contained anything like the precise concept of analyticity his explications were aiming for.... All along it was not natural languages that he set out to analyze in model-theoretic fashion."

Uebel concedes that though he has forged something like the conditions wherein one can view a certain agreement in the respective programmes of Neurath and Carnap, the nature of such an agreement is by no means clear: "the methodological demarcation of formal and empirical inquiries does not yet determine the strategic role and relative weight carried by either." That is, though both Carnap and Neurath would recognize the legitimacy of the other's programme, it is not clear the weight they give to either. Does one rely more heavily on the systematic attempts to forge a formal construction of languages or on the more general historico-sociological analysis that emphasizes the necessary ambiguities inherent in actual scientific practice? Moreover, Neurath may express worries about the point of (ibid) "producing ever more subtle edifices of formal confirmation theory ... when we are faced with actual contradictions between our best physical theories, never mind the vagaries of knowledge of the social world." On the other hand, Carnap may wonder about the point of "discouraging formal inquiries when rigorous clarity concerning our basic analytical tools themselves is imperative." Any reconciliation between Carnap and Neurath given this assessment seems problematic at best.

Moreover Uebel still worries about Carnap's global aspirations and the effect this might have on an understanding of the relative conformity of his to Neurath's viewpoint. If his target was to formalize natural language in order to provide a deductive link between scientific theory to observation, then perhaps his view contradicted Neurath's after all. Carnap of course intended to apply his descriptive syntax to observation terms used in the practice of science. The origin of those terms comes simply via an appeal to the class of terms used conventionally in biology and psychology (ibid, p. 218). This would relativize the a priori nature of the formalized observation clauses, rooting them in convention and practice. However Neurath's further worry would be that testing observation sentences requires an examination of the application of those sentences within the particular historico-sociological contexts that they are made. Any such testing cannot "be enlightened by the idealizations that Carnap entertained" (ibid). To be sure, Uebel writes (ibid), "Carnap was far from denying that he traded in idealizations. Indeed, his logical reconstructions were 'ideal types' precisely because they sought to approximate the calculus conception of language."

Uebel (ibid) concludes then that Carnap's "conception of scientific language was a different one" from Neurath's. However he claims that there is nothing in his analysis that indicates Carnap intended to tell "the 'whole' story about 'the' scientific language." So nothing about his view precludes their respective programmes being understood as different aspects of a common methodology. The main difference appears to be in their respective "estimation of how far the formalist agenda of the logic of science can help to elucidate science and how much of it will have to be elucidated by the contextual and pragmatic features investigated by Neurath's 'behaviouristics' of science."

Thus the potential compatibility between Carnap's logic of science and Neurath's historico-sociological naturalism (ibid, p. 219) "is unstable until the competition of priorities between the logic and the pragmatics of science is resolved." That resolution, from a contemporary perspective, indicates that their respective views "stand in need of reformulation." What Uebel claims is needed is a "systematic account of the philosophical implications of ... pragmatical questions of language choice." Finally, Uebel writes (ibid),

> So is Carnap's and Neurath's reconstituted collaborative project doomed to ultimate failure? It is perhaps still too early to tell: to investigate this would be to start down a path that was, after all, not taken in the actual history of logical empiricism. Yet that at least in principle it could have been taken is what I hope to have shown here.

For Uebel, a *rapprochment* between Neurathian and Carnapian philosophy is not ruled out in principle. Though their apparent differences regarding Tarski-truth and semantics threatens a deep rift, Uebel attempts to salvage their division by suggesting that they each hold a mixed view of the nature of language. That is, they both, to different extents, were 'calculists' with respect to formalized languages. He argues that Carnap was not a calculist about natural language: he did not hold that one could formalize all empirical language in science. This raises the need then of philosophical reflection regarding the extent to which one ought to rely upon the formal considerations articulated and pursued by Carnap and the socio-pragmatic considerations of Neurath. At bottom, it does not rule out that they may fall under a common philosophical umbrella.

However, one may take pause over Uebel's points in the hopes of contributing to a still clearer and stronger reading of how Carnap and Neurath's programmes may be reconcilable – or at least how they may be related under a common methodological umbrella. First of all, Uebel's claim that Carnap did not seek to formalize all of natural language is partially correct, but the present thesis indicates that framing the issue this way does not suitably capture Carnap's view. Certainly, his starting point was not the claim that logic could be enlisted to formalize all of scientific language. However, he did not take it to be ruled out *in principle*: if one can construct a sufficiently robust formalized language containing all definitions in the natural language and a meta-language giving semantic rules to apply definitions within that (conventionally adopted) language, then one could in principle formalize the language in question. As Uebel notes, Carnap traded in idealizations that sought to approximate language. This indicates that he did not think these idealizations encompassed all the features of that language, but it does not preclude the possibility that a sufficiently robust calculus may be so constructed. It remained an empirical question the extent to which such idealizations might approximate natural language to an ever more precise extent.

This is to say that constructing formalized idealizations of language is not understood as Carnap's solution to the problem of the connection between theory and observation. Rather it amounts to the problem itself. And that problem can be addressed through an application of the formal structures one enlists to represent natural language. Recall the discussion above regarding the *Aufbau*. Richardson's detailed historical analysis indicates that Carnap was not after a reduction of scientific theory to subjective experience as a solution to the problem of scientific knowledge. Rather the problem itself was, given one assumes subjective experience as the starting point for an empiricist epistemological programme (which he took to be the generally agreed upon starting among empiricist epistemologists), how does one go from subjective experience to knowledge in science? Similarly, given that formalizations function to clarify relations among variables in constructed languages, to what extent can formalizations clarify constituents of empirical statements?

So Uebel is correct to assert that Carnap was not after the formalization of natural language in the sense that the possibility of such a task was not his starting point. However, given the successful application of the tools of logic and mathematics, the question regarding the extent to which the ambiguities of empirical science and the natural language utilized therein might be systematized through those tools was an open question. It was answerable through the endeavour of pushing the limits of the applications of idealizations.

Second of all, one might revise, slightly, Uebel's characterization of the problem facing Carnap and Neurath with respect to the 'nature of language'. At the risk of being pedantic, one might argue that given their mutual rejection of any sort of epistemological or ontological realism and, as Uebel points out, their consequent aim of formulating a methodology which has no *systematic* roots in any 'reality', they cannot be understood as disagreeing about the 'nature' of language. To appeal to such criteria would surely count as falling back toward metaphysics, since it would suggest there is a 'real' nature of language that they are attempting to formulate. Rather, their disagreement was pragmatic: what was an appropriate starting point from which to formulate a methodology for the assessment of language?

Moreover, the worry about their respective conceptions of the 'nature of language' may be misplaced because Neurath came to accept that Tarski's theory was an adequate definition of truth. Friedman (2000, p. 51) states that Neurath reversed his initial opposition to Tarski at the Paris Congress of 1935. In part, it is based upon this acceptance that one can understand Neurath's receptiveness to Carnap's formal endeavours. However, he continued to be worried about whether formal languages have anything to say about the empirical domain, which marks the problem left at the end of the protocol sentence debate.

This ties to another consideration. Uebel is correct to point out that Neurath worried about the potential retreat of formalism to metaphysics, wherein formal idealizations would be taken to have some foundational role that they cannot be afforded given the limits of empirical knowledge. Neurath exhibited a consistent criticism throughout the protocol sentence debate. But one might argue that it is in virtue of Neurath's constant worries about metaphysics in Carnap's attention to technical issues that proved his undoing in the subsequent development of logical empiricism. Richardson (1998, p. 207) writes that at the Paris congress of 1935, Carnap laid out what he took to be three stages of philosophy. These stages signify Carnap's own development from epistemology to the logic of science and they can illustrate for present purposes the way in which Carnap viewed worries about metaphysics. Richardson (ibid) writes,

> In this essay, Carnap invited his audience to view current developments as a move to a third stage of scientific philosophy. In the first stage, scientific philosophy had rejected metaphysics. This ushered in a 'transition from speculative philosophy to epistemology'. The second stage had involved the rejection of the synthetic *a priori* and the consequent adoption of empiricism in epistemology.

The third stage marked Carnap's move from epistemology to logic of science, wherein epistemology is (1936a, p. 36), "purified and decomposed into its constituent parts," which were the psychological and logical (Richardson 1998, p. 208).

Carnap's development has been discussed above, but the point in identifying his position thus is to indicate that his programme developed from the first stage of purging scientific language of metaphysics. It moved on to an attempt to "purify" and decompose the language of science into parts. A scientific language must certainly be devoid of metaphysical concepts that can find no meaningful expression in the language of physicalism, but then what are the positive features of formal developments in logic and mathematics that might be applied to formulating a methodology for the analysis of science? This sort of question motivated a move within philosophy to explore more technical aspects of philosophical analysis, while the rejection of metaphysics was taken to be a launch point for the move. And though Neurath supported developing logical calculi, his ambiguous position on how far formal pursuits may function to clarify some of the very features he argued for in his physicalism rendered him ill placed to push historico-sociological aspects within the developing logical empiricism. This can be demonstrated below.

George Reisch suggests how logical empiricism came to be received as an 'apolitical' and 'trivialized' philosophy into the 1950s and beyond. In part this due to the absence of Neurath. Reisch's paper (2001, p. 199) explores Neurath's ideas about unpredictability (that unpredictability-in-principle ought to be recognized as a constraint upon scientific endeavours). Reisch argues that, "with some tweaking, they amount to a principled rejection of what would become one of the standard (if preliminary) models of explanation in philosophy of science, namely, Hempel's Deductive-Nomological model [DN] and its extension to history and sociology."[10] Reisch argues that his reading of Neurath's unpredictability principle (ibid) "opens a window on some larger questions about the history of logical empiricism." Why did logical empiricism "at first, a vital, creative, and politically engaged project" transform into an "apolitical" and "trivialized" area of philosophy? And why were Neurath's views not recognized in what came to be the received view of that philosophical tradition? Reisch claims that the two questions are related, suggesting it is in virtue of Neurath's ideas falling out of favour that logical empiricism came to be understood as it did.

He writes (ibid), "Assume that Hempel's work on DN explanation stoked the fires of what would become the explanation-industry, a large component of professional logical empiricism as it thrived in the 1950s and 1960s." Because of this Reisch views Neurath as trying to steer logical empiricism in a different

[10] This will certainly suggest a problem for the reading of Hempel in the following chapter, since it is argued there that Hempel ought to be understood as sympathetic to Neurath's worries. However this apparent problem will not prove resilient given the historical picture.

direction than he understood Hempel to be taking it. Reisch (ibid, p. 200) shows that Neurath (1931) presents a "vague outline" of what would become a DN model of explanation: sociologists attempt to establish laws that may be applied to individual situations. In the end such an application may afford certain types of prediction. By 1943, Reisch indicates, Neurath came to re-evaluate the very possibility of prediction (ibid): "He came to believe that the world is much more unpredicatable than he had suggested earlier, and he supplies his readers with nearly all they need to conclude that Hempelian explanation will generally not work for History or historical Sociology."

Reisch lays out a general historical context within which Neurath expressed his opinion regarding Hempel's work (ibid). He lived in Oxford and was establishing his ISOTYPE projects there. He lectured at All Soul's College and was editor-in-chief of the *Encyclopedia.* Moreover he developed a concern over the notion of unpredictability in empiricism: that unpredictability-in-principle – chance – precluded notions that one could predict empirical events at all. And Neurath felt that his colleagues, like Charles Morris, a co-editor for the *Encyclopedia*, were too soft on thinkers Neurath would dismiss outright as metaphysicians. Reisch writes (ibid), "he was surprised by Morris who published a book called *Paths of life: Preface to a World Religion* (1942) in which Morris used not only the R-word ("religion") but also the S-word ("Spengler") in respectful, non-dismissive ways. Neurath wrote to Morris and urged him to look at his own, early essay against Spengler's mischief."[11] Neurath appears to have been having less an influence on the intellectual culture within which his colleagues operated.

Moreover, Neurath felt he was being unfairly attacked, and worse, ignored by fellow philosophers. To wit, he felt that Russell (1940) was unfair and confused. It has been argued in the present thesis that Neurath's view is justified – that, though there were difficulties with Neurath's overall expression of his view, Russell did not seem to understand key points of the discussion. Furthermore, when Neurath rushed out his 1943 manuscript for the next edition

[11] See Neurath (1921).

of the *Encyclopedia*, Carnap removed his name from the editorial board of Neurath's submission. Reisch writes (ibid, p. 202) that because the monograph was rushed into production and Carnap received only the proofs, rather than a draft, he relinquished editorial responsibility: "Carnap's maneuver, Neurath believed, signalled that Neurath was a figure whose efforts did not even need to be taken seriously." Neurath complained about disregard of his philosophical work by other colleagues and younger scholars, being represented as "merely an organizer and promoter for logical empiricism" (ibid). Reisch (ibid) cites a letter from Neurath to Carnap: "I think that I, to a certain extent, am one of the pillars of this movement, not only its 'promoter' as people sometimes like to treat me."[12] Furthermore, he complained that in attacking certain Neurathian views, others had failed to even mention the views as his own. Schlick did not mention Neurath by name, while Schlick's student, Herbert Feigl listed him merely as a sociologist (ibid, p. 203).

Overall, Neurath appears to have become marginalized among the logical empiricists during the period of the early 1940s. Intellectually this surely must have been because he failed to identify with the shift in emphasis within scientific philosophy toward a technically oriented systematic approach to the problems of empirical knowledge. The technical aspects of Carnap's work that worried Neurath so much, with respect to metaphysics, reflected the direction technically oriented philosophers were headed. For Carnap, logic and mathematics proved indispensable in clarifying problems within physics and they presented potentially fruitful frameworks with which one might clarify the language of science more generally.

One should draw a distinction between two claims that might be made here. First, there is the historical question of the trajectory of scientific philosophical thought – the direction they actually took as an intellectual community toward refining technical apparatuses of analysis. Second, there is a question about whether this direction was justified. What the present analysis

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[12] Cited as, Neurath to Carnap, September 22, 1945, Wiener Kries Archive, Noord-Holland, folder 223.

points to is the first claim: as a historical fact, Neurath seems to have been left out of the cultural dynamics within scientific philosophy at the time. Logical empiricism was emerging – for reasons both internal and external – as a technically organized discipline of the analysis science. Though Neurath has been shown to support the technical use of logical tools for analysis, his objections, and perhaps his own lack of understanding of the point of the technical programme (in Carnap's work in particular), obscure his relationship to that programme. It cannot help that he came to be known as rather non-receptive to criticisms of his position (ibid).

Reisch rightly suggests that part of the reason logical empiricism came to be characterized as rather trivial was precisely because the historico-sociological worries that dominated Neurath's thought came to be sidelined. However, Neurath does not appear to have placed himself in adequate proximity to the technical projects of logical empiricism to remind his colleagues of these broader contexts within which technical frameworks must be understood to have anything to say about the empirical domain. This, of course, is not for lack of trying, but Neurath's idiosyncratic methods and modes of engagement clearly did not resonate in the way he intended among logical empiricists like Carnap and Feigl, and scientific philosophers more generally, like Russell. (Furthermore, Neurath died early in the development of logical empiricism – 1945 – and resided in Oxford, which left him absent in the development of scientific philosophy in America throughout the 1950s and 60s when academics had to maintain a certain level of ideological discretion in the McCarthy era.)

A claim of the second nature is beyond the scope and intention of the present consideration. That may involve refining Neurathian views in the manner Uebel suggests to indicate how Neurath and Carnap's views might be reconciled. However, given the historical analysis afforded by Uebel and Reisch, one may be inclined to forgo the sort of reconciliation proposed by Uebel and take the approach suggested here. Neurath and Carnap shared the philosophical viewpoint discussed at length in the sections above. They agreed that the language of physicalism ought to be purged of metaphysics, which precludes any notion that epistemological questions (in so far as either of them would characterize their endeavours as epistemological) cannot be aided by any claims regarding 'the way the world is'. Scientific language is constructed from conventionally adopted formal frameworks (logic and mathematics) and empirical statements (taken from psychology and biology). Their differences lay in the extent they thought formalism could contribute an analysis of the empirical domain.[13]

Thus, philosophically speaking, Carnap and Neurath can be characterized appropriately under the same umbrella. Methodologically speaking, their views appear to be distinct – not because there is no methodological place for both historico-sociological and logico-mathematical aspects of the analysis of science – but because, as a historical fact, they disagreed on the extent to which one can utilize formalizations to tell us anything about the empirical domain. That failure to come to such a methodological agreement, in conjunction with the broader social currents confronting logical empiricists, left the historical aspects trumpeted by Neurath sidelined.

As noted above (see p. 135), Uebel locates what appears to be crucial to understanding where the protocol sentence debate left off: namely that a central question facing Carnap and Neurath was the extent to which formalizations can say anything about empirical science. Any potential compatibility between Neurath and Carnap would remain unstable until a resolution is proposed regarding the "competition of priorities between logic and the pragmatics of science." It is suggested in the present thesis that the question of the application of formalizations remains momentous for contemporary philosophical methodology. What answers afford themselves?

One might draw the conclusion from Uebel's point that until one can establish systematically when formal and when historico-sociological considerations are to be employed, there remains no resolution. (Uebel does not make this point.) One may offer another suggestion. The question for which one

[13] It is not necessary here to argue that Carnap and Neurath's pragmatic approach was necessarily the right or wrong approach. However it is important to be clear that theirs was indeed pragmatically motivated.

may request an in-principle answer – how much does one emphasize the formal aspects of science? – begets rather a pragmatic one. That answer would, moreover, be closely aligned with the pragmatic considerations embraced by both Neurath and Carnap. The predicament facing any sort of resolution between the parties of the protocol sentence debate might be understood as the predicament of empirical knowledge more generally: sometimes formal aspects of science enlighten inquirers to underlying structures of empirical science; sometimes to assess the scope and limitations of those structures within empirical science one must flesh out the broader conditions within which one applies those frameworks to indicate how and why the frameworks function in a given empirical domain. Both aspects need to be assumed to varying degrees depending on the occasion in question. Both history and formalism should be allocated a functional role in any adequate account of the structure, scope and consequent limitations of the application of any set of scientific theories to empirical domains.

This feature of logical empiricism is obscured among logical empiricists not just because of failings of the participants themselves to come to a shared agreement in the protocol sentence debate. The positive developments of the late 1930s took place, notably, under tumultuous social conditions. Lola Fleck (Cartwright et al., 1996, pp. 82-87) reminds readers that by the 1940s Carnap had migrated to the US in 1937, Neurath had been in exile from Vienna in The Hague since 1934 and the meetings of the Congress of the Unity of Science were organized by Neurath (with little funds while in exile) as a means of continuing to forge logical empiricist principles. Moreover, Neurath lost his second wife in 1937, again fled the Nazi invasion of The Netherlands on a fishing boat in the dark of night and spent seven months interned in the Pentonville Penitentiary in London for being an Austrian national.

Given these sorts of circumstances one might be less inclined to find fault with members of the Vienna Circle in their failure to find a principled agreement among themselves and recognize the level of agreement they achieved at all.[14] Much less so should Neurath, in so far as the present argument is correct, be regarded somehow as 'missing the point' of the technical direction taken by logical empiricism because he insisted on asserting certain aspects of their philosophy that were not central to the technical projects embraced by members who had migrated to the US. Peter Galison has shown the way in which America was far more hospitable to the technical developments than the social considerations tied to logical empiricism. Neurath's early death in the 1940s and his location in Oxford kept him outside the philosophical developments that thrived in America throughout the subsequent decades.[15]

In spite of their failure to find agreement on a resolution of the protocol sentence debate, their shared philosophical commitments indicate that the differences between Carnap and Neurath's views lie in their respective methodological orientations for analysis of science. In particular, they failed to agree on how much formalism could contribute to an analysis of science. However, the question regarding the extent to which formalism might inform such a task begets not an in-principle, but a pragmatic answer to be determined in the domain of science itself – or, rather, for the present purposes in the philosophical assessment of scientific practice. The tension that arises at this stage of the debate can be understood to be a part of the problem of empirical knowledge more generally. And it may answered by testing the bounds and applications of employed formal structures.

From a contemporary analysis, the lesson learned from the outcome of the protocol sentence debate could be just this point: the philosophical methodology characterizing logical empiricism by the mid 1940s could be the programme determining the extent to which one may pursue technical and historico-sociological aspects in given contexts. Indeed, if the problem presented by Carnap and Hempel is the extent to which formal frameworks can be enlisted

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[14] Russell's comment in 1940 that Neurath's philosophy renders him an 'exile' may have been better chosen, given Neurath's circumstances over the previous seven years.

[15] Galison (1998). See also Howard (2003) and the preface to Friedman (1999).

to clarify empirical statements, then the central question for a methodology originating in the Vienna Circle debates is how and when does one attend to formal or empirical aspects of an assessment of science? But, to be sure, this question marks the modus operandi: it does not require an in-principle answer but marks the catalyst for a programme of research. And it affords a tolerant view of the respective roles played by technical or empirical aspects of a philosophical inquiry into science. At times formal pursuits can be enlisted to clarify broader empirical considerations consisting of historical, psychological, sociological inquiries, while the empirical considerations can provide a general context for considering the extent of the application of a given formal inquiry to an empirical domain. Whither philosophical methodology as logic of science or historico-sociological naturalism? It depends upon pragmatic advantages to a given inquiry. But both appear to have a role.

This chapter has shown Hempel's emerging views out of the protocol sentence debate. In virtue of his attention to questions of vagueness in logic, his 1939 paper indicates the way in which he emerges as a philosopher who would become, in Friedman's words, 'a master of Carnapian explication'. However, it also indicates that he remained cognizant throughout the 1930s of the historico-sociological aspects trumpeted by Neurath. It has been argued that Carnap and Neurath can be understood to be interested in different methodological aspects of a common philosophical viewpoint. In addition, one can place Hempel at the centre of the emerging picture of the trajectory of logical empiricism. In virtue of the development of his D-N account of explanation, which will be examined in the next chapter, one can indicate how Carnap and Neurath's respective methodological approaches might be mutually incorporated into a fruitful philosophical methodology that seeks to explore, clarify and expand the structure and boundaries of scientific knowledge.

Chapter Five: Reformulating an understanding of Hempel's D-N account of explanation

> *I hope nothing which is said here will be interpreted as a claim that the semantic conception of truth is the 'right' or indeed the 'only possible' one. I do not have the slightest intention to contribute in any way to those endless, often violent discussions on the subject 'What is the right conception of truth'? I must confess I do not understand what is at stake in such disputes; for the problem itself is so vague that no definite solution is possible. In fact, it seems to me that the sense in which the phrase 'the right conception' is used has never been made clear. In most cases one gets the impression that the phrase is used in an almost mystical sense based upon the belief that every word has only one 'real' meaning (a kind of Platonic or Aristotelian idea), and that all the competing conceptions really attempt to catch hold of this one meaning; since, however, they contradict each other, only one attempt can be successful, and hence only one conception is the 'right' one.*
>
> *Disputes of this type are by no means restricted to the notion of truth. They occur in all domains where—instead of an exact, scientific terminology—common language with its vagueness and ambiguity is used; and they are always meaningless, and therefore in vain.* (Tarski, 1944)

The previous three chapters have examined the protocol sentence debate to suggest how one should understand Hempel's D-N account. The dynamics of the debate suggest a conception of the orientation of the Vienna Circle logical positivists that is at odds with conventional interpretations of the point of logical positivism. And it locates a programme with which to understand the dynamics of a philosophical methodology: there are logical aspects and empirical aspects of the assessment of science. The question is how the two can serve within a comprehensive methodological approach to understanding the structure of science. As discussed, theirs was not a reductivist programme that sought to interpret all empirical phenomena in terms of logic and mathematics. Rather, in the face of skepticism about the possibility of providing ontological foundations for a scientific system of knowledge, they sought a positive construction of what one can claim to know in science using formal tools.

The principles located in the protocol sentence debate suggest commitments the Vienna Circle shared that, when applied to an understanding of the point of Hempel's D-N account, shape what one should expect from that account. One can characterize the principles as 1.) conventional adoption of a methodology for assessing science, 2.) formal/empirical distinction employed to clarify the nature, scope and structure of science, and 3.) apriorism as indicating that, absent indubitable foundations for knowledge, one must assume some starting point for any assessment of science. Moreover, the grounds for recommending a given methodological assessment of scientific knowledge are pragmatic: is it empirically adequate and does it afford systematic theoretical development? These principles ought to shape the interpretation of the D-N account as a pragmatically adopted and systematically orientated methodology for the assessment of explanations in science. The Vienna Circle logical positivists were not committed to a given ontological outlook to underwrite science. Rather, given the limits of human knowledge, they argued that physicalism – which arguably can be understood in some lights to be a constitutional system in the widest sense – is characterized as adopted via convention. There is no prior, meaningful philosophical argument to be made in support of it. It amounts to a constructed and systematic methodological framework with which science confronts the world of empirical phenomena.

Thus locating the general features of their commitments can inform a grasp of the point of the D-N methodology. Carnap's ontological ambivalence runs through Hempel's adoption of the D-N account. The logical positivists were not after the conception of a right epistemological viewpoint that the adoption of the right constitutional system would vindicate. As discussed, for Carnap one could not meaningfully raise epistemological questions without first adopting some constitutional framework that gives rise to epistemological worries relative to the framework itself. Likewise, one does not try to find out what explanation really is and then construct the right account of explanation to vindicate it. Rather it is not until one assumes some account of explanation that one can formulate what explanation can be understood to be, relative to that account. As will be demonstrated in this chapter, the D-N account provides a general and basic outline of the structure of a great deal of accepted explanations in science. It affords a systematic backdrop against which to assess explanatory candidates. Moreover, given the adoption of the D-N methodology, science can be characterized as a highly systematic and fruitful heuristic, wherein inquirers attempt to specify the domain of application of employed general regularities. Scientists seek to establish where and when regularities apply in practice in order to explain the phenomena that can be said to fall under them. The subsumption of statements of empirical circumstances under established regularities amounts, to a greater or lesser extent, to an explanation (to be accepted or not) with the D-N framework.

As he develops his account, Hempel often explicitly states that one need not feel compelled to assume the D-N structure at the outset. There appears to be nothing about the 'world' that motivates its adoption. Rather, one is faced with a world of experience and Hempel attempts to make explicit certain aspects that appear to be assumed in a systematic, scientific confrontation of that experience. To be sure, it *is* the world of scientific statements that generates the basic pattern of what is taken to be acceptable explanations in science. Acceptability appears to be assumed in explanations that have a certain structure. The basic pattern of that structure, Hempel argues, can be represented by the D-N framework.

According to the canonical reading of Hempel's D-N account, science discovers laws in the world and the D-N model is intended to give a criterion of adequacy of explanatory candidates, which on the account must employ those laws. That view centers attention on whether the D-N model is the right account of explanation and can stand up against counter-examples intended to show its inadequacy. It has motivated a vast body of work surrounding what Reisch (2001) calls an explanation industry in the 1950s and 60s. Though this chapter touches upon certain features of them, it will not attend to the wide literature on issues of explanation both because what is sought here is neither the 'right' account of explanation, nor as argued presently, is it what Hempel appeared to be after. It is not a first-principles argument that the D-N account somehow has scientific explanation right. Neither is it an argument that the D-N account may or may not have internal challenges on a more detailed logical level. Rather, it aims at a clear exposition of the point of the covering-law model. One may thereby approach explanation problems with an eye for the explicatory orientation of the D-N model rather than confusing that orientation with pragmatic considerations (as when one finds an explanation acceptable.)

To be sure, one may find difficulties with asserting conventional origins of methodology, or supposing the formal/empirical distinction is a justifiable tool for analysis, or taking any a priori starting points in an empiricist methodology. But one need not be compelled to agree that this is an appropriate methodology to understand that the historical circumstances behind the D-N account indicate these principles motivated Hempel's influential philosophical explications of the scope and limitations of scientific knowledge. This aspect of the present thesis suggests the need for further research: how does such a conception of Hempel's work fit with his overall corpus? How, indeed, might a reconstituted conception of the D-N methodology shape approaches to questions of knowledge in the natural and social sciences?

This thesis does not afford the scope of such a monumental undertaking. However, the chapter examines how such a reconstituted view appears to be in accord with some of Hempel's key papers after the 1930s. Further, though Friedman has called Hempel of this period a 'master of Carnapian explication' as he attends to the logical aspects of scientific explanation, it is rather interesting to note that Hempel's works necessitate reference to the question of the empirical adequacy of the D-N framework as an idealization, which points to the role for assessment of how the framework can be employed: articulating the beliefs and assumptions that underlie its application in given contexts. This feature is crucial both to Hempel's intellectual legacy and for the possibility of drawing on Hempel and logical empiricism generally as a resource for informing contemporary philosophical discourse. It is hoped that the conclusions herein will point to a fruitful path of inquiry for those interested in the nature, scope and limitations of scientific knowledge.

This chapter has five sections. First, it highlights the principles extracted from the protocol sentence debate that underlie the adoption of the D-N account. Conventional adoption of a methodology, the distinction between formal and empirical aspects of science and the employment of a relativized a priori all appear to characterize the D-N account.

Second, it examines both the D-N account as an explicatory methodology and the distinction between logical and pragmatic aspects of explanation. The present appraisal is developed by attending to Hempel and Paul Oppenheim's seminal 1948 paper, which is divided roughly into two parts – the first being a general outline of the basic pattern of explanations in the sciences and the second being a more precise logical definition of explanation. This thesis emphasizes the first part: it attempts to extract what appears to be crucial with respect to a basic pattern of explanation outlined by Hempel and Oppenheim and indicates how that basic pattern is intended to bring clarity to the basic orientation of explanations in science. That the D-N account is taken to outline roughly a wide range of explanatory candidates in the natural and social sciences is an assumption gleaned from the examples found in Hempel's work. It considers the extent to which that account, in virtue of its emphasis on the distinction between formal and empirical aspects of explanation, can provide a systematic framework with which to clarify explanatory candidates.

Third, it considers Hempel's categories within D-N explanation. He outlines further categories to facilitate a clearer classification of explanatory candidates, which suggests a very general conception of the nature of science as a heuristic in the hunt for regularities and other nomological functors under which to locate statements about empirical phenomena.

The fourth section looks at two examples in order to demonstrate how the D-N methodology can serve as a device with which to clarify explanatory candidates. First, it considers how to recommend one explanation over another by looking at a discussion surrounding Jared Diamond's (1997), *Guns, Germs and Steel.* Diamond's ecological explanation of societal development worries some critics that it obscures the more local, cultural explanations involving human agency. Second, it examines John Pemberton's paper on the limited usefulness of idealized models in economics.

Principles underlying Hempel's D-N account

Three general principles appear to characterize the views of Carnap, Hempel and Neurath in the protocol sentence debate. They should inform an understanding of the point of the D-N account. One can characterize them as 1.) conventional adoption of a methodology for assessing science, 2.) formal/empirical distinction employed to clarify the nature, scope and structure of science, and 3.) apriorism as indicating that, absent indubitable foundations for knowledge, one must assume some necessary starting point for any assessment of science. Moreover, the grounds for recommending a given methodological assessment of scientific knowledge are pragmatic: is it empirically adequate and does it afford systematic theoretical development?

The empirical aspects apply roughly to the pragmatic domain of explanation: empirical statements subsumed under general regularities, the empirical application of employed laws or other nomological functors in an explanation, the conditions under which explanations have been deemed acceptable, or what beliefs and assumptions play a role in the acceptance of explanations.[1] The employed distinction between formal and empirical aspects of explanation might be understood along the lines of that between logic on the one hand, psychology on the other (the former pertains to the formal features of explanation, while the latter, generally, to empirical-psychological conditions within which explanations might be deemed acceptable or understood as accepted) and the factual truth of the explanation offered. Furthermore, the apriorism-principle relating to the conventional adoption of the D-N account follows roughly from the first two principles: the idealized structure abstracted from scientific practice is put forward as an a priori starting point within the methodology that assumes the explanatory structure for the purpose of explicating explanatory candidates. 'A priori' here is understood in a relativized sense – if one elects to use the D-N framework, it is adopted conventionally as a tool for explicating scientific explanatory candidates, but there is no claim to provide an a priori justification for its adoption.

The preceding chapters presented these three principles arising from the protocol sentence debate. Chapter two indicated that, for the left wing of the Vienna Circle, protocol sentences and a logical framework are adopted via convention in an empiricist constitutional system. There are limits to a philosophico-scientific methodology grounded in empiricism: the starting point of statements formulated to confront phenomena, in conjunction with the logical framework used to articulate relations among them, provide constraints on what one might claim are the boundaries of science. This is why logical empiricists argued that appeal to notions like 'reality' or the 'world' cannot be understood as informing their developing methodology: for the logical empiricists the edifice of scientific knowledge consists in the vast number of statements formulated to confront the world and 'reality' is not a concept formulable within that edifice.

[1] Empirical statements may be present in an idealization, but the greater extent to which an idealization contains empirical statements, the less general it can be as an idealization. In Neurath's terms, the empirical statements introduce *Ballungen* into the explanatory form. The greater number of empirical factors present, the further the idealization moves from a general idealized status.

Chapter Three indicated that idealization emerges as a central feature of the methodology. Carnap held that there is an indispensible distinction between the formal structures of statements and empirical statements made according to the structure of an adopted framework. A class of empirical statements exhibits a structure that can be represented formally to a greater or lesser extent. Formalizations may be understood to be idealizations in the sense that they represent an idealized form of a given class of empirical statements to be applied systematically across wider domains. Within a formal language, one can clarify connections among idealized statements, which consequently provides a framework enlisted to assess empirical statements. Of course, as discussed, there was disagreement about the extent to which formal idealizations could provide materially adequate models of empirical statements in science, but Carnap and Hempel came to see this question as their domain of inquiry. As discussed in the previous chapter, Hempel (1939) employs Carnap's distinction and shows the way in which formalizations can prove their applicability when shown to apply across a number of empirical domains. The logic/psychology distinction demonstrated the means to bring clarity to problems in philosophy of science. And by applying the formal idealized representations to the domain of empirical application, logical empiricists could attempt to explicate empirical statements systematically.

Chapter Four examined Hempel's emergence in the debate in the 1930s. Philosophers can find in his methodology a place for, and indeed the necessity of, both formal and empirical aspects of an inquiry into the structure, limits and expansion of science (an indication of his employment of Carnap's distinction). Moreover, one can locate the role of a relativized apriorism in his thought. Empirical phenomena are diverse and wide-ranging. One must start somewhere for a systematic investigation. Absent the grounding of scientific knowledge in 'certainty' or 'truth',[2] conventionally adopted basic observation statements and an employed system of logic provide just such a starting point. In the context of the D-N account, its logical form is abstracted from practicing science and

[2] Recall that contra Schlick, Carnap and others in the left wing argued that truth could not effectively function as a foundation for scientific knowledge.

employed as an entry point to discussions of other instances where explanations are provided. Accepted explanatory candidates exhibit a form outlined by the D-N structure and that structure can be applied to other explanations as a way of assessing their structure, scope and the assumptions meant to prop them up. However, 'apriorism' ought not indicate that Hempel appeals to some absolute notion of the a priori: logical empiricists reject notions like the Kantian synthetic a priori. Rather, basic observational predicates, formal frameworks and idealizations consisting of employment of the two, are taken as 'a priori' in the sense that they are conventional starting points for the adoption of the given methodological framework within which they are assumed, and some starting points or other are necessary.[3]

Discussions pertaining to Hempel's intentions in constructing the D-N account should be understood against this backdrop. In short, that account is not intended to give a strict criterion of adequacy for particular explanations in science. Rather, in virtue of its being an idealization, it outlines an explanatory framework that reflects what scientists generally take themselves to provide in scientific practice (the question of its origin is a descriptive/psychological one) and a benchmark against which to assess the scope and structure of particular candidates of explanation. As will be demonstrated in the following section, Hempel does not suggest that there are laws in the world that science merely needs to discover to vindicate its claims to knowledge. Rather, laws and other nomological functors within an adopted idealized explanatory framework are employed to connect – to a greater or lesser extent – antecedent with consequent statements of empirical circumstances to flesh out the nature of explanations.

[3] This is not to argue that the given methodological framework is the necessary framework with which to account for scientific knowledge. Following Carnap, one might hold a certain ambivalence about the selected methodology. It is important only to be clear about the structure and empirical limits of the methodology in a given context. It can be assessed based on how well it serves the purpose at hand. For logical empiricists, given their particular programme of constructing an account of the structure of science based on a (huge, descriptively-produced) class of observation sentences in conjunction with an adopted logical framework, empirical statements and a system of logic provide the necessary starting points to build up this particular methodological approach.

Hempel's D-N methodology is not intended to provide the answer to the problem of scientific knowledge. It affords a systematic way into the problem.

D-N as an explicatory model and the distinction between logical and pragmatic aspects of explanation

Hempel's analysis of scientific explanation marks a cornerstone in the history of analytic philosophy. And yet, if not explicitly, there seems to be an underlying assumption that his covering-law model is a somewhat naïve, if remarkable, attempt to characterize scientific explanation through its reliance upon discovered scientific laws and relevant empirical conditions. The problem appears to be that a host of counterexamples do not conform to the D-N explanatory model, while false explanations can be seen to fit the D-N form. However, a survey of key papers with an eye for the emergent principles in the protocol sentence debate suggests an orientation of the D-N account as a pragmatic and systematic methodology for the clarification of explanatory candidates.

This section will survey Hempel and Oppenheim's seminal (1948) paper on explanation. Their account of explanation is not new, but has been assumed in previous discussions among other authors (ibid, p. 265, *fn* 7). What they attempt to do, in part, is make explicit certain assumptions about explanation through an elementary sketch of its apparent basic pattern. Though the paper was written by Hempel and Oppenheim, Hempel appears to be the primary author since he takes responsibility for its deficiencies. For the sake of brevity, the paper will be referred to as Hempel (1948).

Hempel (1948, p. 245) writes that "one of the foremost objectives of empirical science" is to explain "the phenomena in the world of our experience." And to explain phenomena is to answer the question 'why?' and not just 'what?' regarding those phenomena. Though generally this is accepted, he (ibid) notes that, "there exists considerable difference of opinion as to the function and the essential characteristics of scientific explanation." He intends the essay to "shed light" on the problems of explanation by providing both an "elementary" sketch of the "basic pattern" of scientific explanation and "a subsequent more rigorous analysis of the concept of law and the logical structure of explanatory arguments."

Hempel (1948, p. 246) gives some illustrations to demonstrate the basic pattern of explanations to be found in science. For example, when one drops a mercury thermometer in hot water, the mercury drops rapidly and then climbs. An explanation involves statements that the glass tube heats up initially expanding the glass and causing the mercury to fall slightly. Heat conduction transfers the heat to the mercury and (ibid), "as its coefficient of expansion is considerably larger than that of the glass, a rise of the mercury level results." He remarks that there are two types of statements in this explanation: those regarding the antecedent conditions of the glass, the mercury, the application of heat, etc., and those expressing general laws under which the event can be understood to fall – laws of "thermic expansion" of glass and mercury and of conductivity of glass (ibid). The event is explained by indicating how the antecedent conditions when falling under stated laws bring about the consequent conditions (fall and subsequent rise of the mercury).

A second example exhibiting this basic pattern (ibid), is the explanation of why the oar of a rowboat looks bent upward when placed in the water. This can be explained with reference to statements about antecedent conditions regarding the oar being straight, partially in the water, and about general laws of refraction and water being an "optically denser medium than air." The general laws under which the circumstances fall bring about the consequence that the oar looks bent. He (ibid) writes, "the question '*Why* does the phenomenon occur?' is construed as meaning 'according to what general laws, and by virtue of what antecedent conditions does the phenomenon occur?" By specifying antecedent circumstances and employing particular general laws, one can construct an explanation connecting these circumstances to the statement about the event or object to be explained.

Third, science provides explanations of not just particular events, but of laws with reference to more general laws. According to Hempel (ibid, p. 247), "the question might be asked: Why does the propagation of light conform to the law of refraction?" He replies that classical physics answers "in terms of the undulatory theory of light," which asserts that light propogation is a certain general type of wave phenomenon and that all phenomenon of this type fall under the law of refraction (ibid). Thus, one explains a law by indicating how it falls under a more general, comprehensive law. Likewise, they note (ibid), Galileo's law for the "free fall of bodies" can be explained by deducing it from Newton's laws of motion and the law of gravitation, along with statements regarding the mass and radius of the earth, etc.

Hempel (ibid, p. 249) locates some general characteristics of explanation from examples to bring about the basic pattern of the D-N model of explanation consisting of the two types of statements referred to above:

Explanans

$C_1, C_2, \ldots, C_k$ Statements about antecedent circumstances

$L_1, L_2, \ldots, L_r$ General laws

Explanandum

E Description of the empirical phenomenon to be explained

This pattern of explanation contains statements of antecedent circumstances and the employed laws under which the circumstances are said to fall.

He contains analysis to a simple formal system (in which one can facilitate a deduction from the explanans to the explanandum) and (ibid) notes that for a proposed explanation to be "sound" its statements need to satisfy logical and empirical conditions of adequacy. There are three logical conditions of adequacy within a simple formal system: 1.) The explanandum must follow logically from the explanans, "for otherwise, the explanans would not constitute adequate grounds for the explanandum." 2.) The explanans need to contain general laws that serve to deduce the explanandum. When deducing a general law from the explanans it may be the case that the explanans contain only statements about laws. 3.) The explanans must be empirical, at least in principle: they must be observable or testable (in principle).

Furthermore (ibid), as an empirical condition of adequacy, "The sentences constituting the explanans must be true." The discussion in the chapters above regarding the distinction between truth and acceptance – that is, between the truth of statements within a formal framework and the acceptability of empirical statements formulated roughly according to the structure outlined in the formal framework - appears to present a disconnect between what Hempel seems to hold in the Vienna Circle debates and what he asserts in this article. Attention needs to be given to account for the apparent distinction to indicate the *way in which* Hempel is committed to the empirical condition of adequacy. In short, there is a distinction between simple formal scientific explanations and more general statements that incorporate a large range of empirical assertions that can be structured according to the simple formal form. The former affords a simple domain in which to clarify the logical structure of a scientific explanation given the D-N account, while the latter involves a greater emphasis on how empirical data are formulated according to that structure.

The 'truth' claim is asserted relative to a simple formal framework for the purposes of spelling out the idealized structure. If the idealization from which one confronts empirical phenomena does not incorporate the truth condition, then it cannot function as a methodology because there is no systematic framework with which to link the explanans with the explanandum. Hempel either is being infuriatingly vague here by appearing to conflate empirical with formal aspects, or, perhaps more interestingly, advocating a subtle distinction in the specification of his methodology that it will be helpful to illuminate. His employment of the truth condition trades on the distinction emphasized in the protocol sentence debate between the formal and empirical aspects of science – and in this case of scientific explanation. The condition of truth is crucial for the formulation of the methodology in the formal structure of the model. Though it is not necessary for strictly logical assessment of the structure of an argument, it is necessary to define a law within the methodological framework – a definition without which the framework cannot function in the deductive-nomological pattern. One cannot deduce the explanandum from the explanans without a law, but one cannot define a law or other nomological connector in the formal framework without specifying it relative to true statements. The truth condition, which might appear to conflate the distinction that was so central to the protocol sentence debate, rather asserts that distinction for the purpose of clarifying the structure of the methodological framework. Asserting the distinction between logical and empirical conditions is crucial for defining a law in the methodology.

Hempel applies the concept of law within a formal language to true statements only (ibid, p. 265):

> The apparently plausible alternative procedure of requiring high confirmation rather than truth of a law seems to be inadequate: It would lead to a relativized concept of law, which would be expressed by the phrase 'sentence *S* is a law relative to the evidence *E*'.

He (ibid) notes, for example, that Bode's "general formula for the distance of planets from the sun" cannot have been considered only a law relative to the astronomical evidence available in the 1770s, such that the discovery of Neptune would cause it to cease to be a law. He remarks that the discovery of Neptune, rather, suggests that existing evidence provided a certain probability that the formula was a law. New evidence, such as the discovery of Neptune, reduces the probability to an extent that Bode's formula cannot be understood to have been true (and therefore not a law). He (ibid, *fn* 22) adds in a footnote that,

> The requirement of truth for laws has the consequence that a given empirical statement *S* can never be definitely known to be a law; for the sentence affirming the truth of *S* is tantamount to *S* and is therefore capable only of acquiring a more or less high probability, or degree of

> confirmation, relative to the experimental evidence available at any given time.

The truth requirement is essential for defining the concept 'law' in the idealized model. Otherwise one cannot employ the law in the idealized formal structure to deduce the explanandum. A generalization cannot serve this nomological purpose in the idealized form. Of course, there is then the question of the empirical application of the law and one's knowledge of it, but this points to the pragmatic aspects.[4]

Moreover, Hempel (ibid, pp. 251-8) gives an example to suggest that explanations in the social sciences fit the D-N form. Hempel (ibid, p. 251-2) points to a severe price drop in American cotton exchanges in 1946 that led exchanges in major trading centres to stop trading temporarily. The explanation given at the time involved a large holder who worried his stake was too big and consequently liquidated his stocks. Smaller speculators panicked and sold off their shares, which led to the sharp drop. Hempel (ibid, p. 252) notes that this explanation (regardless of its merits) can be understood to account for the phenomena by "integrating it into a general pattern of economic and socio-psychological regularities." It involves both statements of antecedent circumstances pertaining to the holdings of the large-scale speculator and sizable collective shares of the smaller holders, and of implied general regularities of supply and demand, and those of speculators who want to increase or sustain their financial positions –i.e. an increase in supply will send the price down and when prices drop, speculators will unload their stocks to limit losses, which sends the price down further.

Examples such as the cotton exchange suggest, for Hempel, that explanations in the social sciences have the same structure as those in the

[4] To contain the scope of this analysis, it focuses on deterministic laws, but laws can also be understood to have a certain probability. Hempel holds that explanations of this sort share the form of the D-N model, but a strict deduction to the explanandum cannot be produced. Rather it can be said to bring about the explanandum with a certain probability.

physical sciences, but many feel that causal explanations are inadequate in fields of human behaviour (ibid, p. 253). He assesses three arguments to this end: 1.) that causal explanations of the sort indicated by the covering-law models assume a repeatability that cannot occur in human behaviour; 2.) one cannot establish generalizations of human behaviour because responses to a given situation are based upon, not just the situation itself, but, the history of the individual; and 3.) explanation in the social sciences involves reference to motivations and teleological analysis, rather than causal analysis.

First of all, human behaviour in events such as the downturn of cotton prices has (ibid), "a particular uniqueness and irrepeatability" that cannot be accounted for in causal explanations, which assume a "repeatability of the phenomena under consideration." Furthermore, events in the social sciences do not lend themselves to testability. Hempel argues these claims misunderstand the "logical character of causal explanation," and are inconclusive. No event in the physical sciences in repeatable: each is distinct. "Nevertheless," argues Hempel (ibid), "individual events may conform to, and thus be explainable by means of, general [causal] laws" because causal laws assert only that any event of specified characteristics "is accompanied by another event which in turn has certain specified characteristics." Thus, friction brings about heat, so an event involving friction will beget heat. Moreover, for testability, events require only a set of instances where events of certain characteristics are followed by those of other specified characteristics. In the price drop of cotton, one can employ laws of supply and demand and generalities about actions of speculators to indicate that given certain statements about antecedent circumstances, they accompany consequences like the rapid drop in the price of a commodity. The extent to which a range of instances can be located in which particular antecedent conditions are followed by certain consequences like the price drop gives a range of application in which the general regularities can be said to apply.

This latter point is important because it indicates that explanations involving assumed regularities have a certain scope that is limited by the statements of empirical circumstances in which they apply and the general regularities employed to link statements about antecedent with consequent circumstances. When the range of circumstances is deemed wide enough that the regularity may be accepted as suitable to be used as a law within the methodology, then the law may be suggested as an employable explanatory device across a wider domain. The law will be taken a priori in the methodological assessment for the purpose of constructing an explanatory account, but it is adopted via convention. With respect to pragmatic aspects, one then needs to determine whether empirically the domain of the generalization may be expanded, or at least whether employing the law affords a systematic heuristic that can be tested to see whether it indeed holds. Barring testability it can be assessed based on whether it contributes to the expansion of the theoretical framework with which one confronts empirical phenomena.[5]

For Hempel, this point provides occasion to emphasize that reference to an event in an explanation is reference to particular characteristics of the event – to "the occurrence of some more or less complex characteristic in a specific spatio-temporal location, or in a certain object." Reference to an event does not refer to all the characteristics of the occurrence relating to the object. Neither does it refer to all the spatio-temporal characteristics surrounding the event. Thus an 'event' is not explained in its entirety, but only with respect to the specified characteristics noted within the explanatory account. If one wants to explain the drop in cotton prices with respect to supply and demand laws and statements about the circumstances under which the event occurred, that explanation needs to indicate that a drop in price corresponds to an increase in supply relative to demand and consequent drop in the price of cotton over a range of instances. The flip side is that the explanation appealing to general laws of supply and demand might prove too general to afford any enlightening explanation of the particular event.

Nevertheless, two points may be made regarding this situation. First of all, Hempel is attempting to indicate that the structure of the explanatory

[5] For a discussion about theoretical expansion as a criterion for acceptance of a regularity or explanatory candidate generally, see Hempel (1958).

candidate in this financial example shares the logical form of those employed in the natural sciences, which shows, for him, that the covering law models he has attempted to explicate can be utilized to assess explanations in the social sciences. This of course gives rise to the second point, regarding the empirical import of explanations of this form. The question is whether an explanation employing general laws of supply and demand and those regarding behaviour of speculators provides a 'good' explanation in this instance. This question must be distinguished from the former and to understand the point of Hempel's account one must be clear about the distinction: fitting the logical form of a covering law model does not, in itself, guarantee an acceptable empirical explanation. As will be discussed below, although the large body of Hempel's work attends to many logical aspects of explanation, these always give rise to the question of the empirical aspects. The covering law models give a framework for confronting empirical phenomena. They afford a systematic way into the problem of how to account for explanations in science. But to understand attentions to the logical aspects of this framework as an attempt to give necessary and sufficient conditions for any and all explanations in science is to place upon the covering laws models, and the deterministic D-N account in particular, demands that they are not constructed to address. This is to confuse the two points that, for clarity of assessing scientific explanation, must be held distinct.

This distinction of course can be seen to have roots in the protocol sentence debate, discussed in the chapters above, wherein Carnap held that the distinction between formal and empirical aspects of science is indispensible for the clarification of science. As argued, Hempel endorsed this distinction. One problem with Hempel (1948) is that in attempting to render explicit an assumed structure of explanations by practitioners of science, that distinction may have been obscured, which leads to the assumption that, with the covering law models, he is attempting to give criteria of acceptance for all explanation in science. Although the formal features of the D-N account may, indeed, preclude certain explanatory candidates that violate the logical form (i.e. by appealing to the consequent rather than the generalizations that bring about the consequent), the model functions principally to render explicit explanatory candidates in order to decide whether they ought to be accepted or not. The point of the covering law models is to attempt to explicate explanatory candidates in science by giving one a framework against which to specify statements of antecedent circumstances and their relation to consequent circumstances via employed general laws.

The second argument that explanations in the social sciences are distinct from those in the natural sciences is that human responses to situations depend upon not just the context of the situation, but also on the history of the person responding. This suggests that responses cannot be repeatable and thus, for example, any appeal to laws of supply and demand and dispositions of speculators wanting to improve their situation cannot afford any causal explanation. Hempel replies that there is no *a priori* reason why one cannot uncover generalizations that take into account the history of individuals. He (ibid, p. 254) notes, for example, that in the natural sciences regularities can be established for "physical phenomena, such as magnetic hysteresis and elastic fatigue, in which the magnitude of a specific physical effect depends upon the past history of the system involved." He argues no single events are repeatable, but features of events show enough repeatability that explanations may serve fruitfully across a range of events in both the natural sciences and the social sciences. One emerging question then is what is the range of application of a given explanation? Answers to this question surround attempting to specify both the empirical applicability of the employed laws within the explanatory account and the specific features of the specified circumstances of a given event that can be said to be repeatable.

The third argument against the employment of causal analysis present in covering-law models is that explanations of human behaviour involve reference to motivations, which employ teleological, as opposed to causal, analysis. Given this argument, explanation of the drop in cotton prices would need to specify the speculator's motivations as a factor, thereby indicating the need to refer to goals, which distinguishes it from the natural sciences. Hempel (ibid) agrees, but argues that this does not render them "essentially different" from causal explanations. Though reference to goals points to the future as a cause, he remarks that goals would still fall within antecedent circumstances, since the intention to bring about a future outcome is a desire (to effect a certain outcome) and therefore sits among the circumstances at the beginning of the action. He (ibid) concludes that there is "no formal difference on this account between motivational and causal explanation."

Furthermore, that motives cannot be observed does not indicate a difference between causal and teleological explanation because the same occurs in the natural sciences: electric charges cannot be observed, but are tested indirectly, which is (ibid), "sufficient to guarantee the empirical character of the explanatory statement." Likewise, motivations may be determined by indirect methods: "linguistic utterances ... slips of the pen or tongue." He (ibid) writes, "as long as these methods are 'operationally determined' with reasonable clarity and precision, there is no essential difference in this respect between motivational explanation and causal explanation in physics."

Hempel lays out the basic pattern assumed in scientific explanations. This thesis will forgo his subsequent closer look at a definition of explanation in order to emphasize how attention to the basic pattern suggests a revised interpretation of the point of his work.

One can consider the development of Hempel's account with an examination of another key paper. His "Aspects of scientific explanation" (1965q, p. 412) outlines three types of models characterizable as covering-law models: deductive-nomological, inductive-statistical and deductive-statistical models of explanation. He notes that these models are not intended to "describe how working scientists actually formulate their explanatory accounts." Rather he writes that they are intended (ibid), "to indicate in reasonably precise terms the logical structure and the rationale of various ways in which empirical science answers explanation-seeking why-questions." Hempel (ibid) argues that formulating covering-law models "is not to deny that there are other contexts in which we speak of explanation, nor is it to assert that the corresponding uses of the word 'explain' conform to one or another of our models." But he (ibid, p.

413) suggests that to "deplore" the D-N model on the grounds that it cannot fit with certain cases of explanation "is to miss the intent of the model." For Hempel, since the logical aspects of covering-law models pertain to clarifying the structure and rationale of explanation, it is not an argument against them to locate counter-examples wherein they appear not to apply. Explanation in science is a variously constituted enterprise and abstracting from particulars to an idealization affords a framework with which to assess those particulars.

One might think of any number of counter-examples for the D-N account. One illustration, emblematic of the often-trivializing nature of canonical philosophical discussions of explanation, is the event that someone attempts suicide by taking arsenic, but is hit by a bus before the arsenic does its job. A D-N explanation of the event would assert statements about antecedent circumstances that the man wants to die and he takes arsenic. It employs some statement of a general regularity that if someone takes arsenic he will die. If, with respect to taking arsenic, one construes an explanation regarding the individual's death according to the D-N model, one would have an explanation that fits the D-N form, but is empirically false.

1. A man who wants to die takes arsenic.
2. If a man takes arsenic, he will die.

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3. He dies.

(1) is a statement about antecedent circumstances. (2) is the law employed in this explanation to link antecedent circumstances with (3), the explanandum. The account provides an explanatory candidate for the death of a man – indeed all the premises are true, but in fact a bus hit the man before the arsenic actually killed him. The explanation is thus false. The obvious point of the counter-example is to indicate the inadequacy of the D-N account.

A Hempelian response to this counter-example, which will be examined in the next section, is that, empirically, the explanation is elliptically formulated and that to render the explanation acceptable one would have to articulate the premise that it was the arsenic that killed the man. That premise would be false. Thus the problem elicited by the counter-example does not testify against the viability of the D-N account, so much as locate the empirical aspects necessary for acceptance: for explanations to be accepted, one criterion is that they must be understood to be empirically adequate.[6] The formal structure of the D-N model is not intended to provide empirical adequacy for every explanation. Rather it is to give a framework with which to clarify the structure of explanatory candidates. In this instance, by representing the explanation in the D-N framework, one can locate the implicit and false premise that renders the explanation false.[7] To be sure, the D-N framework does not do a great deal of work here. And one may be led to attempt to locate false premises without using the D-N structure. However, it does provide a (very general) backdrop against which to proceed to assess the explanatory candidate for its empirical adequacy (and whether the extent of that adequacy is sufficient to lead one to accept it as an explanation). The question in the D-N framework pertains to the application of the general regularity: it is a sufficient law that if one takes enough arsenic one will die, but the empirically relevant question is whether that regularity applies in this particular instance. Does the arsenic cause the death of the individual in this situation? This empirical question cannot be answered by rendering an explanation in the D-N form. It is answered by an empirical inquiry into the cause of the death. The general regularity is not relevant to this particular instance because it does not locate the cause of the death.

[6] 'Empirical adequacy' is not strictly specifiable for Hempel. Though formulating a criterion of adequacy might be possible in order to apply 'empirical adequacy' more systematically across a wide domain, whether one determines an explanation empirically adequate or not, or indeed whether one selects a particular adequacy criterion, ultimately rests upon a decision, which suggests the need for a descriptive assessment of the context within which an explanation has been accepted.

[7] It is not being argued here that the D-N framework locates the false premise. It remains for Hempel an empirical question whether premises provide an acceptable explanation. Employing the D-N framework (perhaps trivially at times) aids in clarifying the structure of a given explanation so as to indicate where further empirical inquiry can indicate whether the explanation in question might be accepted.

To illustrate the distinction between the pragmatic aspects of explanation and the explicatory aspects intended by the D-N model, one might consider Hempel's response (1965, p. 420) to Michael Scriven's (1962) counter-example. It shares the form of the objection just considered, but is somewhat less ridiculous in nature. Scriven's intention is to argue that the D-N account is not sufficient and should be supplemented with the extra stipulation that "total evidence" must be included to render it adequate. The example will be paraphrased slightly, but will maintain the features of the argument. An old rail bridge needs to be destroyed and replaced by a newer one. A demolition crew places dynamite in all the key places on the bridge and sets a timer. Thirty seconds before the timer triggers detonation, an earthquake drops the bridge entirely. The explosives go off and the bridge is destroyed.[8] An explanation, relative to the dynamite premises and represented roughly by the D-N form, might look like the following:

1. Dynamite has been placed in strategic points on a bridge and a timed-detonator has been set.
2. The dynamite goes off.
3. If dynamite is set in appropriate points on a bridge and goes off, then the bridge will be destroyed.

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4. The bridge is destroyed.

According to Scriven's argument, this explanation fits the D-N form, but is empirically false. That is, one can deduce the explanandum from the explanans, but premises (1) to (3) are not an empirically adequate explanation of the destruction of the bridge. As a result, the D-N model needs to be supplemented with an additional requirement of "total evidence."

Hempel's reply is that such a counter-example does not rule against the D-N account itself. He argues that Scriven's counter-example misses the intent

[8] For Scriven's example, see his (1962, pp. 229-30).

of the covering-law model. The point there is to explicate the explanatory candidate in order to clarify the statements involved in the proposed explanation. Hempel argues that although Scriven's counter-example fits the D-N form, it amounts to an elliptically formulated explanation that is based on an assumed false premise. The explanation that the bridge was destroyed because of the dynamite is based on the false premise that there was a bridge to destroy in the first place. Since the bridge had been destroyed already immediately prior to the detonation of the dynamite, then the premise involving the claim that there was a bridge cannot have been asserted. Though the explanation fits the D-N form, it is incorrect because it employs a false premise, not because the D-N framework has misfired.

Nevertheless this is not to argue that the point of the D-N account is to provide sufficient conditions for a satisfactory explanation. What it does do is give the methodological framework with which to clarify the structure of the explanation and explore the empirical adequacy of its premises. In this case, although the false explanation is complete according to the D-N form, this does not mean it is acceptable. An empirical examination of the conditions under which the bridge was destroyed would indicate, first, that there was a bridge to destroy when the dynamite ignited, which there was not. This would suggest, second, that the regularity employed would not apply in this instance: (3) may be an acceptable law applicable under certain circumstances, but it does not apply in this situation. One can say then that the explanation is not acceptable. Examining the empirical adequacy of the premises renders more clearly the features of the explanation so that one can decide whether that explanation will in fact be accepted or not.

Moreover, for Hempel, Scriven's additional criterion of "total evidence" (ibid) is unnecessary and "too strong to be tenable." In no scientific research can a criterion of "total evidence" of all the facts be a reasonable demand in order to construct an acceptable explanation. Acceptable scientific explanations are not complete explanations of phenomena. To look for 'total evidence' misses the methodological import of the D-N account. Given Hempel's view, Scriven's counter-example against the D-N account conflates that account as an "explicatory model" with the pragmatic aspects explanation – conflating the clarification aspects of the model with whether one decides to accept the explanation.

Of course, if one assumes the D-N account, then one can tell the extent to which such an explanatory candidate adequately fits the D-N form. For instance, it can suggest when a proposed explanation can be rejected *as an explanation* – for example, if the explanandum simply does not follow from the explanans even in a loosely formulated way. Hempel (1959) argues that functional 'explanations' are not explanations because they do not fit the D-N form – he characterizes functionalism rather as an analysis. This is not to argue that functional analysis is not of central importance to scientific inquiry because it is a heuristic device, wherein one proposes functional hypotheses to determine whether they may be adequately characterizable as laws that operate across a sufficiently wide domain to be employed in explanations. Rejecting that an explanatory candidate actually fits the D-N form is not the same as rejecting an explanatory candidate that fits the form, but turns out to be either empirically false or insufficiently warranted to be accepted on the view of an inquirer. The former falls within the logical aspects of explanation that are given by the formal structure of the framework, while the latter pertain to pragmatic aspects, wherein one decides whether or not to accept an explanatory candidate.

According to Hempel, to explain is to make something (1965a, p. 425) "plain and intelligent," which is to understand it in pragmatic terms. The terms require "reference to persons involved in the process of explaining." Pragmatic explanation is relative in the sense that an explanation may suffice for one person, but not another, as in the case where an explanation involving a mathematical proof would be unintelligible to someone unfamiliar with higher-level mathematics. Hempel (ibid, p. 426) writes,

> whether a given argument *Y* proves (or explains) a certain item *X* to a person *P* will depend not only on *X* and *Y*, but quite importantly also on

P's beliefs at the time as well as on his intelligence, his critical standards, his personal idiosyncrasies, and so forth.

On the other hand, Hempel (ibid) argues that there is an important role for the logic of explanation: some objective notion is required in scientific explanation, such that "empirical implications and their evidential support" do not rely on individuals and their idiosyncratic beliefs. What this suggests is that some notion of explanation should be constructed that can be applied uniformly regardless of individuals involved. Thus a "nonpragmatic" structure of scientific explanation is abstracted from the pragmatic one (ibid): "it is this nonpragmatic conception of explanation which the covering-law models are meant to explicate." The D-N account is a systematic framework with which to indicate empirical adequacy of an explanation and the relation of evidence to a given explanation. But of course, the framework need be neither considered necessary for the assessment of explanations, nor designed to give universal sufficiency conditions for the acceptance of particular explanations.

Assessing non-pragmatic models of explanation does not imply that pragmatic aspects of explanation are not important (ibid, pp. 426-7); neither is it intended to say that someone will find an explanation satisfying only insofar as an explanation conforms to the covering-law models. Hempel notes that one might be satisfied with the identification of a particular causal fact in an instance without considering how that fact fits in with an explanatory model. He notes an example where in winter a man finds his house gets cold when he watches the television. Noting that the device is under the thermostat explains why the heat turns off when the television is on (the heat from the, by now antiquated, television raises the temperature under the thermostat). He (ibid, p. 427) remarks thus, "the pragmatic conditions for the acceptability of a proposed explanation do not coincide with the logic-systematic ones that the covering-law models are meant to explicate."

Categories within D-N explanation

Hempel notes that scientific explanations vary with respect to relative completeness. A complete D-N explanation is one in which the explanandum can be reached by a deduction from the explanans. For example, one can deduce the period in a model of the idealized pendulum with reference to the length of the pendulum and law of gravity. As will be discussed, scientific explanations are characterizable into categories reflecting relative degrees of completeness: elliptic explanations, partial explanations and explanatory sketches. It will be important to stress that for Hempel, these are methodological categories constructed for the purpose of clarifying the nature of explanations and are specified relative to the formal features of the D-N model. Moreover, there are no ultimate decision rules that tell one whether an explanation is partial, elliptical or a sketch, so it falls to the "judicious interpretation" of the individual characterizing explanatory candidates. This indicates that the categories are methodological constructs, which provide a framework with which to clarify the structure of explanatory candidates. And it gives rise to an assessment of the pragmatic aspects of explanation – the consideration of the empirical statements involved in the acceptance of an explanation.

The categories serve also, for the present purposes, to indicate how Hempel might characterize a good deal of scientific activity: the complete explanation functions as an idealized methodological imperative. Scientific explanation of phenomena would aim at expanding the class of accepted complete explanations. An expansion to cover all phenomena, of course, is not possible with existing frameworks and denotes the limits of scientific knowledge generally, but it marks a methodological directive in scientific activity. One aims to expand the theoretical and empirical application of accepted complete explanations and fill in partial explanations and explanation sketches with empirical statements such that they might be rendered complete.

Explanations vary widely in terms of (ibid, p. 424) "explicitness, completeness, and precision with which they specify the explanans and the explanandum." Thus they (ibid) "diverge more or less markedly from the idealized and schematized covering-law models." However, Hempel (ibid, p.

425) notes that, "adequate" scientific explanations "presuppose at least implicitly the deductive or inductive subsumability of whatever is to be explained under general laws or theoretical principles." He clarifies in a footnote that he is not claiming that any empirical phenomena can be explained by subsuming it under the cover of a law or some other nomological functor.[9] Rather, he (ibid, p. 425 *fn* 17) suggests that the "logic of all scientific explanations" is of the covering-law sort, which indicates that the D-N framework is a general starting point from which to assess explanations – the deductive connection between the explanans and explanandum provides an inferential framework employed in incomplete explanations. He is skeptical whether all phenomena can be scientifically explained and is not even sure the question whether all phenomena can be explained is intelligible: "I am inclined to think that it cannot be given any clear meaning at all." Moreover, any assertion about what laws actually hold in nature (ibid) "surely cannot be formed on analytic grounds alone but must be based on the results of empirical research." For Hempel, explanation requires both standards for analytic assessment and empirical research, but it is not the case that all phenomena are amenable to explanation via subsumption under covering laws.

Hempel is often understood to be giving necessary conditions for the adequacy of scientific explanations. However, the logic of the explanations does not give exclusive criteria of adequacy. Although it provides systematic rules of inference as a benchmark against which to assess explanatory candidates, adequacy is assumed in the explanatory candidates put forward. The rules of inference thus provide a methodical framework within which to locate and assess constitutive premises for their empirical adequacy (and whether and on what grounds an explanatory candidate is accepted).

Empirical explanations outlined by the D-N form consist in a range of incomplete explanations in which the explanandum cannot be deduced strictly from the explanans. All categories of incomplete explanations share the D-N

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[9] "Nomological functor" is understood as the regularity, law, statement, fact or sets of these that function to connect the explanandum with the statements about antecedent circumstances.

form to a greater or lesser extent, but move progressively farther from the deductive closure of a complete explanation. To be sure, some empirical explanations fit the complete category: with a relatively simple explanandum, one may construct a deductively closed link with the explanans—i.e., the simple physical model of the pendulum. However, as one moves into the domain of richer scientific statements that incorporate a wider range of empirical statements and proposed laws under which those statements can be said to fall, the explanatory candidates introduce a higher level of variables that may not be amenable to specification (Neurath's *Ballungen*). This factor indicates that part of the two-fold features of scientific inquiry (as given by the D-N methodology) is to hunt for empirical circumstances and regularities that can facilitate a more tightly connected inferential link between the explanans and explanandum, thereby pushing partial explanations toward the complete category. Science in this sense consists in a heuristic: a systematic, methodological confrontation of phenomena that aims at constructing ever-expanding classes of accepted, complete explanatory candidates.

Thus, Hempel should not be understood to be asserting that explanations of complex phenomena are deemed acceptable when formulated as complete explanations. Rather, as indicated from his position in the protocol sentence debate, it remains a question for him how far formal idealizations or schematizations (that include empirical statements of a specified scope of application) could be expanded to cover a widening empirical range. This characterizes a feature of the problem itself and is not understood as a solution to the problem of the limits of scientific knowledge.

As noted, elliptical and partial explanations and explanation sketches are three categories into which one might place an explanatory candidate. The first category of incompleteness is an elliptical explanation (ibid, p. 415), an example of which might be "a proposed mathematical proof" when one refrains from mention of "certain laws or particular facts that are tacitly taken for granted." Would they be included, they would facilitate a complete explanation. Hempel (ibid) gives an empirical example of a small rainbow in the spray of a lawn sprinkler, the explanation for which would be "because sunlight was reflected and refracted in the water droplets." Though no explicit rendering of the laws of reflection and refraction are given, they are implicit.

However, elliptical explanations must be considered in two ways – a point which Hempel does not make clear in his discussion. The first use, just mentioned, pertains to the logical structure of an explanation and defines elliptical explanations based on their structural features. However his example of the bridge employs the elliptical category in a different way: it pertains to pragmatic aspects of acceptance, rather than the logical aspects by which he defines the category, and denotes the incompleteness of any empirical explanation. It raises the question of the empirical relevance of omitted premises involved in an *acceptable* explanation.

Hempel remarks that the explanation that the dynamite destroyed the bridge was false because it was elliptically formulated and did not render explicit the empirically erroneous premise that there was a bridge for the dynamite to destroy. At first glance, his use appears problematic, since the proposed explanation is complete: one can deduce the explanandum (that the dynamite destroyed the bridge) from the explanans. It would therefore not be understood as elliptically formulated.

Hempel does not make clear that his example points to pragmatic questions of acceptance rather than logical ones. Elliptically formulated empirical explanations are those which do not include relevant empirical premises that would influence their acceptance. The question in this usage is not whether the premises can be rendered explicit to construct a complete explanation, but whether they affect the empirical adequacy and subsequent acceptance of the explanation in question. In this sense, the bridge example is not a false explanation because the notions 'truth' and 'falsity' pertain to closed formal structures and not the ambiguity of natural language explanations. If Hempel were to employ consistently the truth/acceptance distinction he claimed to have embraced, he would say, rather, that the explanation is not acceptable.

Although the explanation of the destruction of the bridge by dynamite is complete, the constitutive statements within it are sufficiently ambiguous to allow that the explanation is elliptical and that it involves an implicit (and erroneous) premise regarding conditions in which the bridge was still intact upon the detonation of the dynamite.

In this sense, with regard to the pragmatic aspects of empirical explanations, even complete explanations can be understood as potentially elliptically formulated on the grounds that they may be too general to say anything interesting about particulars. Filling in an idealization with relevant statements giving it an empirical interpretation may involve uncovering implicit premises that may not be empirically adequate. This underscores why complete explanations are not necessarily acceptable. And it underwrites Neurath's worries about the extent to which logical functions can serve to illuminate scientific procedure: a complete explanation is so in virtue of the deduction facilitated with highly idealized statements. As those statements come to be filled out with relevant empirical data, the explanation can be understood as elliptically formulated to an extent that Neurath would worry about the degree to which the idealized model really says anything about the event in question. However, for a reconstituted interpretation of Hempel's account, this points to the function of the D-N methodology as a systematic methodological framework with which to confront empirical explanations.

The simple explanation of the drop in cotton prices provides an uncomplicated example. It could be formulated completely with reference only to the law of supply and demand and some simple statements about antecedent conditions. If one states the price of cotton and that there is a rise in market supply, then appealing to the law of supply and demand one can expect the explanandum that there is a drop in price, since effectively the demand drops relative to supply, sending the price downward. However, this is an entirely simplified explanatory candidate that precludes reference to any host of causally relevant regularities at play. It is an acceptable a priori starting point for confrontation of the phenomena (that the price dropped), but it is not very interesting empirically because of the innumerable causal factors that are in play in a market. One can render this explanation elliptical because it does not account for all empirically relevant causes in which one might be interested for an explanation of the event.

To be sure, the idealization is a complete explanation, but if one wants an empirically richer set of explanatory candidates, it must necessarily be rendered elliptical, which then mandates an empirical examination of relevant conditions and causal functors. Upon inclusion of a richer set of causal laws regarding behaviour of the speculators, one may possibility furnish a complete explanation with a wider scope. However, given that, as Hempel notes, strict laws of human behavior are difficult to formulate, one may characterize a richer explanatory candidate as a partial explanation or perhaps an explanatory sketch.

A less complete explanatory candidate can be classified as a partial explanation, wherein the explanans account for the explanandum partially. For example, one can consider the Freudian slip. Hempel (ibid) notes that Freud would argue the slip of the pen, wherein one writes the wrong date, can be explained by the general hypothesis that "when a person has a strong, though perhaps subconscious, wish, then if he commits a slip of pen, tongue, or memory, the slip will take a form which expresses, and perhaps symbolically fulfills, that wish." Hempel argues that if one grants Freud's hypothesis (and it is not clear that Freud's hypothesis is acceptable), the explanans would only entail that there would be some slip or another, but it does not entail a particular slip that one would write a particular date, or make a particular statement (ibid):

> But inasmuch as the class, say *F*, of slips taking this latter form is a proper subclass of the class, say *W*, of those slips of the pen which in some way express and perhaps symbolically fulfill the specified wish, we might say that the explanandum as described by Freud—i.e., that he made

> a slip falling into the class F—is explained at least in part by this account, which places the slip into the wider class W.[10]

Hempel remarks that many explanations in psychoanalysis and historiography are "at most" partial explanations and "thus, the explanatory force of the argument is less than what it claims or appears to be." He goes on to contrast partial explanation from complete explanation, suggesting that the former "falls short" of the latter. Partial explanations, though, can function heuristically to give a framework within which to seek out relevant causes of behavior (in this example) that can fill in the explanation more completely. This identifies the second part of the two-fold features in the methodology: that part of the methodological mandate of the D-N account is to take partial explanations as a framework to confront the world and attempt to fill them in with a richer account of the nomological connections to produce a complete explanation of a sufficiently wide empirical scope. Thus, according to the D-N methodology, science aims in part to expand the class of accepted existing complete explanations and fill in accepted incomplete explanations to render them complete.

An explanation sketch can be viewed as (ibid, p. 424), "presenting the general outlines of what might well be developed, by gradual elaboration and supplementation, into a more closely reasoned explanatory argument." As with partial and elliptical explanations, the explanation sketch shares the D-N form, but the connections between the explanandum and the explanans are far looser. This is to say that the deductive closure of a complete explanation provides an inferential framework against which to structure an incomplete explanation. Deduction is the goal, but the limits of scientific knowledge render it a goal perhaps out of reach of the bulk of scientific explanations. It nevertheless provides an a priori starting point giving an inferential structure to incomplete explanations. The rapid price decline in the cotton exchange is an example of the explanation sketch. The explanation given for the price drop employs regularities

[10] To be more precise, Hempel is referring to statements about a class of slips and not the slips themselves.

of supply and demand and certain behavioral regularities of speculators. However, absent the possibility of specifying laws characterizing those behavioral regularities in a given context, this may provide only a rough sketch of a more complete explanation that might be provided given the development of such laws.

The categories are formulated as a way of classifying explanatory candidates, but Hempel (ibid, p. 424) writes,

> The decision whether a proposed explanatory account is to be qualified as an elliptically formulated deductive or probabilistic explanation, as a partial explanation, as an explanation sketch, or perhaps as none of these is a matter of judicious interpretation; it calls for an appraisal of the intent of the given argument and of the background assumptions that may be assumed to have been tacitly taken for granted, or at least to be available, in the given context. Unequivocal decision rules cannot be set down for this purpose.

The question of how one characterizes particular explanations surrounds not the nature of the explanations themselves, but how one can fruitfully classify them in order to clarify their features.

One may wonder how a "bad explanation" might relate to these categories. The term 'bad' here can be understood to be ambiguous and rendered more precise when thinking in terms of acceptability. Assuming the D-N account, an explanation might be deemed bad in one of two ways (though not these necessarily exclusively). First, it may not fit the D-N form. To attempt to explain the formation of clouds by saying that their function is to drop rain is not to explain them at all. (Of course, one might explain the way *that* clouds release moisture, but that is not the same thing.) That would be to appeal to an effect rather than some cause (or set of these) that functions as a nomological connector that would link cloud formation with statements about water vapor in the air, atmospheric pressure, temperature and laws relating to the behavior of these.

Second, a bad explanation may be characterized as one that is not accepted because of erroneous factual statements, the misapplication of certain laws or perhaps there is a more suitable explanatory candidate that better fits within the canon of existing scientific knowledge. To be sure, a 'bad' explanation may be one that is accepted.

Partial explanations effectively might be understood to house the bulk of empirical explanations in natural science. If one takes Neurath's worries about *Ballungen* seriously – and it appears Hempel does – the complete models, when applied to the empirical domain facilitate partial deductions to particular phenomena: deductions to a range of phenomena under which the particular can be said to fall. When Richard Feynman (1985) indicates the way quantum electrodynamics produces deductions about behavior of light over a specified range, one may understand that theory to facilitate a complete deduction. But one cannot deduce phenomena falling just outside the specified range. In this sense quantum electrodynamics can be said to provide a partial explanation of the behavior of light. It will be helpful to consider his comments briefly. Feynman's telling of the account is intended as a loose history to indicate developments of and the credibility behind quantum electrodynamics. This survey will not delve deeply into the details, but will examine the basic ideas to indicate how one might understand the scope of quantum electrodynamics and its applications according the D-N methodology.

Feynman (1985, p. 5) states, loosely, that the theory of quantum mechanics "supplied the theory behind chemistry," which makes it to have been a great success: the more general theory of quantum mechanics was said, roughly, to be able to explain the theory behind chemistry. However (ibid), there were difficulties calculating the interaction between light and matter, such that Maxwell's theory of electricity and magnetism had to be altered to fit with the recent developments of quantum mechanics. Thus, in 1929, quantum electrodynamics was developed. The problem for the new theory was that it could provide only partial explanations: it afforded rough calculations about the behavior of light, but discrepancies were massive when minor corrections were made for a more accurate calculation (ibid, p. 6): "So it turned out you couldn't really compute *anything* beyond a certain accuracy." In this sense, the model for calculation in quantum electrodynamics can be understood according to the D-N framework as complete, but once applied to light phenomena, it furnishes a partial explanation to be filled in and refined through subsequent testing.

Thus, Feynman (ibid) notes, Paul Dirac formulated a relativistic theory of the electron that ignored interaction of the electron with light. He postulated that the electron had a "magnetic moment" with a strength of 1 unit. In 1948, experiments indicated the strength was nearer to 1.00118 (±3 on the last digit). Once corrections were made to account for interaction with light, one could not deduce the magnetic strength at 1.00118, but instead (ibid), "the result was infinity—which was wrong experimentally." Feynman (ibid, pp.6-7) notes that in 1948 he and two others, using a "shell game," calculated a workable correction at 1.00116, "which was close enough the experimental number to show that we were on the right track," thereby furnishing quantum electrodynamics with partial explanations that accounted for electron interaction with light. He notes that experiment over the subsequent fifty years has given more and more accurate results to a specification of Dirac's number at 1.00115965221 (±4 on the last digit). The theoretical number is 1.00115965246 (±2 on the second to last digit). Thus, though there remains a gap between the theoretical specification and the empirical, thereby rendering explanations of magnetic strength of the electron partial, the methodology of quantum electrodynamics continues to push toward closing the gap. And Feynman stresses that the difference between theory and experiment is extremely small, rendering the partial explanations very accurate.

Though the quantum electrodynamics model itself can be characterized as a complete explanatory framework of electron behavior generally, its application to the electron in particular test cases affords a partial explanation, since it cannot explain outside a specified range of application (it goes vastly wrong with minor modifications to apply it outside the range). The model explains electron partially because one cannot deduce all electron phenomena according to the framework specified in the complete model.

One might say, moreover, that when Feynman states that quantum electrodynamics synthesizes all of chemistry, or indeed (ibid, p. 8), "describes *all* the phenomena of the physical world" he is employing it more as an explanatory sketch to be filled with further empirical study.

The D-N methodology thus affords categories into which to characterize explanations in quantum electrodynamics: as a model, one can characterize it as complete. When applying it to particular situations, like the behaviour of the electron, it affords partial explanations of those phenomena, since it cannot explain all the behaviour outside a limited range. And when applied to the physical world generally, it can be characterized as an explanation sketch that requires empirical study to fill in the explanatory framework in the empirical domain to which it is applied.

Likewise one might argue that explanatory sketches can be understood to characterize explanation in the social sciences, wherein nomological connections between the explanans employing behavioral models and the explanandum about particular human behavior are more loosely formulated. The Black-Scholes model, which can be classified in the category of complete explanation with simplifying assumptions may not facilitate a strict explanation of 'real world' phenomena when that world does not fit the idealized assumptions. It provides an approximation within which the aspects of the actual event described in the explanandum may or may not fall, but can be viewed as a heuristic with which one attempts to account for the variations of the real world event on the simplifying assumptions. It thereby may be characterized as an explanation sketch.

The historical backdrop to the development of the D-N account indicates that Hempel's intent with formulating it diverges from canonical expectations of the point of its application. A brief consideration of the canonical view, as well as an outlook for future discussion, follows in the final section. But this thesis has argued that the historical roots of Hempel's thought indicate a set of principles underlying his account.

It also suggests two general principals orienting logical empiricist philosophy of science. First of all, the protocol sentence debate suggests a distinction to be made between answering questions about our knowledge of the world and formulating a fruitful methodology with which to furnish answers to those questions. The methodology ought to be assessed for its value on the basis of whether it aids in clarification of science, which pertains as much to the logic of explanation as the pragmatic conditions within which an explanation, or indeed, an explanatory framework, is adopted. Complete models in Hempel's methodology can be applied to afford partial explanations or explanatory sketches of phenomena. It provides a systematic way into the problem of confronting and systematizing assessments of empirical phenomena. Hempel here can be seen as a unifying methodologist: bridging the gap between the methodological attentions characterizable in this thesis as Carnap's logic of science and Neurath's socio-historical naturalism. The D-N account necessitates assessment from both technical and empirical philosophical analysis by indicating the need to specify the idealized framework of the explanatory methodology and the limits and constraints upon it in its applications to the empirical world.

Second, Hempel's D-N account affords claims about scientific methodology generally, which could be understood then to have limits that need to be spelled out. The account of scientific knowledge that one can construct on the basis of Hempel's methodology indicates something like what Nancy Cartwright (1999) has called a 'dappled' picture of the world. Science gives a patchwork knowledge of the world just where it can be demonstrated that it provides acceptable explanations. But those explanations must be understood relative to limits specified by the explanans and explanandum statements. Explanatory candidates work just where they prove themselves to work. They may be employed in other domains as hypotheses for partial explanations or explanation sketches to be tested, but this indicates the methodological mandate:

the possibility of a complete explanation is not assumed at the outset, but is the goal when employing incomplete explanations. One fills out the a priori partial explanation in the hopes of expanding the class of accepted complete explanations. Science claims thus are understood relative to the scope of the framework within which they are put forward. And this, one might be inclined to assert, appears to characterize the physicalism of the logical empiricists of the protocol sentence debate. Knowledge of the world just is the ever-expanding class of scientific statements that are given structure and meaning according to a more or less specified set of rules. This knowledge is arguably Vienna Circle physicalism in the widest sense.

One might worry that embracing the sort of ontological ambivalence or agnosticism expressed in this view is to give up on philosophy altogether. However, it need not be considered this way. On the contrary, it indicates a philosophical skepticism about the possibility of underpinning scientific knowledge with a pre-linguistic ontology. But it is skepticism of an ambivalent sort.

A personal example might enlighten one to this ambivalence as well as (hopefully) provide an agreeable aside. A graduate student took his two-year-old son to the London Science Museum where the two were confronted with images of the moon in the space-travel exhibit. For the year previous, the two-year-old had recognized the moon as a white image in the sky that one could locate at night and sometimes on a clear day. "Moo!" and, gradually, "moon!" were the common exclamations upon its appearance. But the youngster wanted to know more about the moon when confronted with it in the exhibit. With some hesitancy about whether the two-year-old could grasp the concepts, the graduate student explained that the moon was a place to which one could travel and that rockets similar to those on display could take people there the way airplanes carry people to see their families far away. The boy remained quiet through the antique train and car concourse until reaching an Apollo space capsule that had been on the moon. A video displayed footage of the capsule orbiting the moon and landing in the ocean back on earth. Upon viewing the video, the boy realized that the moon was a place people travelled to. "Dad! The moon! You can go there!" Beside himself with joy, the boy watched the video again and again, pointing to the capsule in near disbelief that it could be so.

The graduate student wondered to himself what, exactly, he had taught his son. Had he taught him something real about the world, or had he introduced him to a sophisticated set of accepted concepts in science that facilitate technological advances, but are no more "real" than ancient stories about the moon being a chariot in the sky? Informally, the question might seem absurd, but, philosophically speaking, given that the bounds of Vienna Circle physicalism allow one to assert only that scientific knowledge is an ever-expanding set of statements about the world, one might be more reserved about the scope of the lesson taught. To be sure, knowledge of the moon landings involves the accounts of all the engineers in the control centre of the particular launches, the astronauts, historical accounts of the scientific theoretical and technological developments over centuries; also, newspaper reporting, technicians involved in construction and even conspiracy theories challenging the verity of the landings at all. This rather trite list could be expanded to account for the role of scientific pedagogy, a history of wars, cultural and political history etc. The point is that given the breadth of physicalism as a conception of the world, lifetimes of generations of inquirers could be, and indeed are, taken up examining what science is and does. With this consideration, the question of whether science "really" explains the world might become rather uninteresting. Philosophically speaking one may be resigned to assert *that* there are limits to what one can know about the world and that science is one – more or less systematic – way of confronting the world. What might be left to the philosopher in part is critically examining the details of the structure, problems and limits of that systematic methodology. The question of what is real may come to have less significance, and much less be understood as an important question to which an answer might underwrite whatever science can say about the world.

To be sure, one need not assume this view from the present assessment of the D-N account. However, it may be a relevant position one could take when asserting that the D-N account is a systematic framework for assessing explanatory candidates. One may remain ambivalent about the underpinnings of such an account and employ it via convention to examine how far it can clarify what science is explaining about empirical phenomena. One may do that without being drawn into debates surrounding questions of ontology. The historical question of whether Hempel held this view, or indeed Carnap or Neurath, is not the focus of this examination. But it can at least vindicate that their physicalism was not a naïve scientism that simply disregarded foundational questions. They shared a skepticism about foundations and endeavored to formulate a positive account of knowledge in the face of that skepticism.

Two Examples: Ecological versus cultural explanation of societal development and the usefulness of idealized models in economics

The following examples programmatically suggest how the D-N account informs an understanding of explanations. There are a host of other examples that might be used, but it should suffice to indicate the research direction motivated by the present thesis. It is not intended as the last, or exhaustive, word on the fruitfulness of the D-N account.

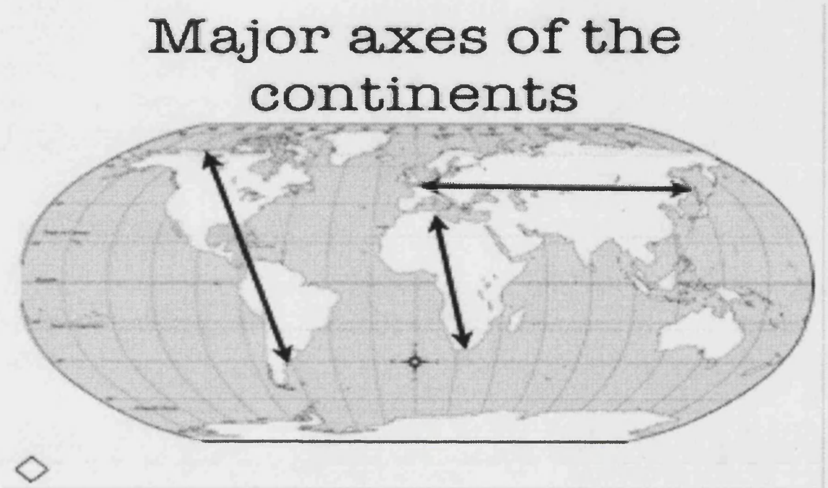
What basis does one have to recommend one explanation over another? This section considers a debate over explanations of societal development in Eurasian and African regions. Jared Diamond's (1997) *Guns, Germs and Steel*, gives an ecological explanation for why these respective societies developed on a different trajectory from one another. William H. McNeil (1997), in reviewing the book, takes issue with Diamond's ecological explanation on the grounds that it fails to acknowledge the role of individual human decisions in that development. One can clarify this debate and an ecological explanatory candidate by specifying the structure and scope of ecological explanations and indicating the way in which they need not be understood as opposing historico-cultural explanations. The short answer to the question of how to recommend one explanation over another surrounds the clarification of the explanatory candidates and an understanding of the purposes the particular explanations are intended to serve: whether ecological explanations are acceptable or not involves reference to the purposes of the inquirers themselves.

Diamond attempts to provide a general explanation of the development of various societies over 13,000 years. His book takes up the question of why some societies developed organized social structures, technology, writing and other features enabling them to conquer peoples of other continents, while others remained hunter-gatherers. He asks, "Why didn't Africans instead conquer Eurasia, bringing Native Americans as slaves?"

He (1997b) suggests that current historical analysis predominantly focuses on a scope too limited to answer "history's biggest unsolved question." Attending primarily to Eurasia since 3000 BC, these historical analyses do not adequately explain why Eurasia should be the focus of development rather than, for example, southwestern United States or Africa.

Thus, Diamond points out the role of ecological factors in development: certain ecological conditions facilitated food production. Moreover (ibid), cultural development can be mapped out roughly beginning with the advent of food production, which beget "dense populations, food storage, social stratification, and political centralization," and which led to "chiefdoms (5500 BC), metal tools (4000 BC), states (3700 BC), and writing (3200 BC)." In short, he argues that 11,000 years ago, the world was populated with hunter-gatherer societies, while certain conditions such as a decline in availability of wild foods led to the consequent increase in farming products, technology and resultant increase in population density. Population numbers themselves, in conjunction with technologies and diseases that arose from the development of those technologies (for example, new strains of disease in domesticated animals) facilitated the predominance of farmers neighboring hunter-gatherer societies.

However, Diamond needs to account for why certain peoples moved to food production, while others remained hunter-gatherers in spite of their relative environmental similarities. "Why Eurasia?" To respond to this consideration he outlines issues of the domesticability of wild animals, the flora and fauna of (in particular) the Fertile Crescent and the axes of travel of ideas of food production. For example, certain animals of different regions are more readily domesticable than others – i.e., the horse and zebra respectively. Moreover, Diamond argues that certain areas developed sooner because of the axes of distributed localities.



Eurasian local practices of animal domestication and food production could more readily spread along the east-west axis of similar latitudes than those axes running north and south in the Americas and Africa.[11]

So, Diamond's account outlines the development of societies from hunter-gatherers to food producers and consequently to a highly structured cultural fabric that coincided with the predominance of that culture. Statements about environmental circumstances fall under the scope of nomological statements covering broad conditions: food production, domesticability of animals found in a region and farming technologies etc. led the Eurasian peoples to develop structured societies more so than those in Africa, which remained largely migratory and hunter-gatherers. Importantly, Diamond takes himself to explain "history's broadest pattern." Such an explanation meets head-on an otherwise predominant, if only implicit, account centered on race. He writes (1997b),

[11] Figure courtesy of Dr. Jason Alexander, London School of Economics.

> In the absence of convincing explanations, many (most?) people resort, consciously or unconsciously, to racist assumptions: the conquerors supposedly had superior IQ or culture. That prevalence of racist theories, as loathsome as they are unsupported, is the strongest reason for studying the long-term factors behind human history.

In reviewing Diamond's book, McNeill argues that by Diamond's explanation, human agency recedes into the background of ecologically explanatory features. McNeill argues that the events of history must be explained in terms of decisions of participants in history – decisions that affected how certain societies developed. How can any adequate account of human history ignore human agency? He writes (1997),

> Diamond does not explicitly dismiss conscious human action as a factor in history.... It is rather that he seizes upon the early era in the unfolding of human capacities when food production was getting started some 13,000 years ago, and then, with a single leap of the imagination, attributes all the contemporary differences among human societies to the relative advantages particular populations have enjoyed as a result of the differences in the plants and animals available for domestication in different parts of the earth.... The vast differences in the wealth and power that different human societies have at their command today reflect what long chains of ancestors did, and did not, do by way of accepting and rejecting new ways of thought and action, most of which were in no way dictated by, or directly dependent on, environmental factors.

McNeill suggests the problem with Diamond's account is that it explains human history as determinately following the course set out 13,000 years ago with the advent of food production. It fails to account for the relevant individual decisions that have shaped the course of history since then.

The nature of their dispute is civilized, but McNeill's otherwise glowing review of Diamond's book raises the question about how to recommend one of their respective explanatory candidates. Thus there are two accounts: one that emphasizes broad patterns and one more locally situated – the first considering human development in the context of ecology; the second in one of culture and human agency. Which explanation more adequately accounts for the development of certain societies and not others? What explanation ought one to adopt? Which is the better account of societal development?

The D-N framework can be employed to clarify the explanatory scope of ecological explanations and recast the question of what is at stake in the discussion. One might be led initially to frame the discussion as one over competing explanations, but by specifying the domain of application of an ecological explanation, the D-N framework can underscore that an explanation brought about by appeal to ecological regularities gives a general backdrop to more locally situated historical explanations of events. Though one cannot deduce, for example, the emergence of the Achaemenid Empire in the mid-sixth century BC, one can nevertheless indicate ecological conditions that can be said to have brought about a level of social organization that led to specific conflicts in the region.

First of all however, it has been important in this thesis, and will be important to emphasize in this example, that one need not feel compelled to adopt the D-N framework as the only one that might illuminate this discussion about explanation. It is adopted conventionally – that is, without any attempt to justify it as the *right* account of explanation – and employed on the grounds that it can provide a fruitful means to clarify the discussion. With this in mind, it is assumed as an a priori framework with which to frame the discussion. Furthermore, it employs a distinction between the formal aspects of explanatory candidates that can specify the range of their application and the empirical aspects that illuminate features on which to base acceptance.

Moreover, the D-N account emphasizes that what is being explained is the explanandum – the statement regarding the development of Eurasian and African societies. This suggests that the question of whether to accept ecological or cultural explanations pertains not to the event itself, but to statements regarding the event. This changes what is at stake in an assessment of the debate, since one, thus, is not arguing for *one correct* explanation of the respective development of Eurasian and African regions. Rather, acceptance of an explanatory candidate will be informed by the different worries one holds in their explanations. McNeill questions whether Diamond's explanation is acceptable on the grounds that the latter does not account for human agency. However, Diamond's goal is not to provide such an account, but to study the "long-term factors behind human history." Thus the question of whether to accept an ecological or historical explanation depends upon the sort of explanation one is after: does one want to explain development in certain instances by an appeal to ecological regularities or more local human agency?

The explanandum in question – societal development of different regions – is a statement regarding the event, an explanation of which must be understood relative to the explanatory framework in which it is couched. An ecological explanation of the development of Eurasian cultures employs general statements of the ecological circumstances in which society was found: any explanation thus is understood relative to the general ecological regularities intended to indicate the resultant explanandum. The flip side of this is that ecological explanations are understood to cover a certain scope at a certain level of generality. Ecological conditions falling under the umbrella of ecological regularities beget an explanandum of a certain scope and generality.

Since food production proliferated in Eurasia, one can use that feature to explain development of food storage technologies, social stratification and subsequent political organization. However, as noted, Diamond then needs to provide an explanation of the emergence of food production. This involves statements of the general circumstances in Eurasia and regularities under which they can be said to fall: types and availability of food resources relative to population, flora and fauna conditions and types of animals facilitated a growth of farming communities over nomadic hunter-gatherers. The explanation of societal development is thus put forward by the employment of statements of ecological conditions and general ecological regularities indicating that given antecedent conditions beget consequent conditions.

In virtue of it being an explanation according to the D-N inferential framework, an ecological explanation could be structured as statements of the following:

1. Ecological circumstances pertaining to domesticable animals, flora and fauna suitable for cultivation and competition for food:

   $C_1, C_2 \ldots, C_n$
2. General regularities covering these circumstances that claim domesticable animals, cultivatable vegetation etc. leads to the proliferation of food production:

   $L_1, L_2 \ldots, L_n$
3. Further regularities specifying that food production begets a certain level of societal development:

   $L_x, L_y \ldots$
4. Explanandum: societal development of a particular region

Rendering the explanatory candidate according to this structure can illuminate a number of features of the ecological explanation. First, it indicates the scope of the explanation. The explanandum indicated by the explanation is understood relative to specified ecological conditions and regularities. One need not expect that it will say anything about specific events involving human agency.

Second, the explanation clearly is not complete insofar as one can deduce the explanandum from the explanans. If one could construct an idealized version of the explanation by isolating certain premises such that a deduction could be produced, one would characterize it rather as a partial explanation in which the explanans could deduce a range of statements of development. Though one might construct an idealized explanation that a level of societal development occurred in Eurasia in the mid-sixth century BC, one could not deduce which empire arose out of more specific statements of historical circumstances.

Third, one might recognize the saliency of Neurath's concept of *Ballungen* in this rendering. Recall that for Neurath any statement is always reducible to more finely grained analysis, which for him amounts in part to an argument for the limits of logic in characterizing language employed in science: one cannot deduce complex scientific statements from primitive logical reconstructions. Much less can one deduce complex statements from complex statements: in the case of Diamond's explanatory framework one cannot deduce the explanandum from the explanans. In this sense, the explanation might be characterizable as an explanation sketch that gives an inferential framework according to which one can organize various empirical statements and laws or regularities. The connection between empirical statements of flora and fauna conditions to regularities indicating they "beget" food production in itself is worthy of an industry of research.

On the other hand, with respect to the formal aspects of the D-N framework as a methodology, one can recognize its usefulness in systematically examining *Ballungen*. It is employed to provide an inferential framework with which to confront the question of explanation. By rendering Diamond's ecological explanation according to the D-N structure, it presents areas for further empirical assessment. One might attend to an examination of domesticable animals and the way in which, for example, the horse, as opposed to the zebra, had been used to bring about conditions under which food production thrives. Or one may undertake research questioning when the regularity 'food production begets a certain type of social organization' can be said, historically to be an apt generalization. That may inform the extent to which the food production thesis can be an appropriate nomological connector in ecological explanations. By framing the explanation according to the D-N framework one can clarify the constituent premises of an explanation, illuminating where areas for further research can fill in explanatory sketches or partial explanations to render them more complete.

Having specified the structure and features of an ecological explanation, one can consider the conditions under which to accept such an explanation.

The question of acceptability of a given explanatory candidate is raised with reference to both the logical connection between the explanans and explanandum and an assessment of whether empirical premises respond to relevant empirical worries. One can examine whether descriptive statements regarding the ecological conditions in Eurasia are empirically adequate and the extent to which the employed regularities can be said to bring about the explanandum: that is, whether "food production begets" the societal development outlined in Diamond's explanandum. Moreover, examining the logical connection indicates that ecological considerations can be specified as distinct from more local cultural ones: statements about human agency need not be understood to follow from the employment of ecological regularities.

McNeil is worried, not only that Diamond's explanation does not account for human agency, but also – that Diamond's explanation puts forward a deterministic view of the behaviour of human agents – that "with a leap imagination" Diamond attempts to explain all current differences in human development in virtue of the availability of domesticable plants and animals. However, Diamond's ecological framework does not need to explain particular human actions because the explanandum of societal development must be understood relative to the framework producing the explanation. That framework employs general regularities that would not entail individual human actions. This is not to say that human agency is not relevant to the explanation of societal development. Rather, it indicates that ecological explanations operate on a level of generality that does not bring about inferences to particular human actions or events that shape the course of history. When one considers whether to accept Diamond's ecological explanation, one does so with an eye for competing explanatory accounts on a similar level of generality (like the racist accounts Diamond claims to be addressing).

With regard to recommending ecological or cultural explanations of societal development, the interesting question may become how does one *reconcile* historico-cultural explanations with ecological ones? How do they fit together? The two families of explanatory candidates thus would not be conceived as competing, but rather as referring to different species of regularities that can be assessed for the way in which they may supplement one another.

Given these brief points, one can restructure the discussion between McNeil and Diamond. The former criticizes ecological explanations on the grounds that they ignore human agency – the way particular human decisions shaped the development of respective societies in Eurasia and Africa. This presents their differences as being a competition between explanatory candidates. However, if one characterizes an ecological explanation as understood with reference to general ecological regularities that function as a nomological connector within a D-N framework, then one can be clearer about the scope and generality of application of that candidate. If one then is concerned about human agency, more locally situated cultural explanations can be characterized against the backdrop of the broadly construed environmental ones. The latter need not preclude the former, but provides a general backdrop against which to begin to describe the role of human agency.

One further point: for the purpose of this discussion, it has been assumed that both families of explanatory candidates refer to the same explanandum. This view suffices to emphasize the point above. However more may be said with respect to the statement about the event – namely that the phrase 'societal development of Eurasia and Africa' is sufficiently ambiguous to allow explanations with varying degrees of generality (ecological or historical), which might lead to the conclusion that historical and ecological explanations are in competition. However, the explanandum in the ecological sense might be posed more precisely as "the societal development trajectory of Eurasia with reference to ecological conditions." This would serve to be more clear about the intended generality of employed regularities in the explanans and could reduce ambiguity that might give rise to the worry that ecological explanations ignore or obscure human agency. Likewise, the historico-cultural explanations may have an explanandum "the societal development trajectory of Eurasia with reference to historico-cultural conditions of human agency." Merely restating the respective explananda as such would reduce the worry that there is a problem of competing explanations.

Before considering the second example, it should be remarked that one might suggest this assessment misses McNeill's obvious point: he is just worried that Diamond has ignored human agents in his explanation. Merely structuring the ecological explanation to more clearly specify *that statements of ecological regularities link statements of environmental circumstances with an ambiguous statement about societal development* does not address the worry. However, the D-N framework puts one in a position more clearly to underscore the point of Diamond's premise: ecological factors can be employed to uncover broader trends behind human development. And Diamond is addressing explanations that generalize from localized historical accounts of human history to general racist claims about the dominance of certain cultures over others. His ecological account gives compelling – indeed acceptable – arguments as an alternative to unsubstantiated racist explanations for different societal development tracks in Eurasia and Africa. Of course, the D-N account itself does not give the arguments. Neither does it tell whether either account is acceptable. But it does afford one the means systematically to structure an assessment of the respective families of explanatory candidates in order to more clearly determine whether and in which circumstances one might accept one or other of these.

The second example considers how to characterize the usefulness of models in economics. John Pemberton (1993) argues that idealized models are of limited use in economics on the grounds that they employ restrictive antecedent clauses (RACs) and one can never be certain that the correct clauses are present to get the model right. The D-N account, as a methodological framework, enables one to clarify some key features of his argument and suggests how one can fruitfully relocate the source of the challenge for idealized models from the RACs defining a given model to the domain where complexity arises – among the (ibid, p. 10), "heterogeneity of the microcomponents of economics, such as consumers and commodities."

Thus, this section proceeds in three parts. First, it outlines Pemberton's argument, showing how he motivates his claim regarding the limited usefulness of idealized models. Second, it uses the D-N framework and its formal/empirical distinction to clarify how idealized models can be understood to fit within an explanatory candidate.[12] Third, it gives a critical assessment of Pemberton's conclusion and locates some general implications for an understanding of the point of the D-N methodology. To address the problem of models' usefulness one needs to look, not exclusively to the causes characterizing the idealized model (pertaining to the formal aspects), but to the users and the context in which the models are applied (pertaining to the question of acceptance and empirical adequacy).

According to the D-N framework, idealized models can be characterized within a broader explanatory methodology as an apriori framework used to group empirical phenomena according to certain regularities. An idealization can then be applied to the empirical domain as an explanatory sketch that gives an inferential framework to be filled in with further empirical inquiry. This suggests that Pemberton's worries about the usefulness of idealizations is perhaps misplaced. Further, it points to a feature of the D-N account as a unifying methodology: one employs it to clarify the relation of the explanans within a model to the statement about economic phenomena to be explained and then looks to further empirical conditions and regularities under which they fall to indicate how and when the model works.

[12] It will be noted below that economic models are utilized to make predictions, which is a distinct usage from the explanatory discussion here. However, in fleshing out the relation of idealized models to economic phenomena, the D-N framework can nevertheless prove useful without wading into questions about the structural relations between prediction and explanation.

Pemberton argues that when idealized models fail to capture relevant material causes they can be very wrong. In addition, one cannot predict when those models will go wrong. Thus, he argues (ibid, p. 1), "doubt is cast on the ability of simple ideal models to capture the causal complexity of economics sufficiently well to provide reliable accurate predictions." What are idealized models according to Pemberton?

Idealized models (IMRACs) are defined by restrictive antecedent clauses. That is, these models get their definition by a prescribed set of clauses that isolate certain causal antecedents and ignore others. These other ignored clauses may actually be relevant in a given phenomenon – they may turn out to be relevant material causes in the economic event in question. It is when the idealized model does not capture the relevant material causes of an event that it fails to provide an adequate account of economic phenomena.

One example he provides of a restrictive antecedent clause is the assumption of perfect knowledge among consumers whose aim is to maximize their wellbeing. He remarks that this assumption (ibid, p. 2), "is of course false of all actual consumers or producers – it underpins a model in which behaviour is more simple and more predictable than in reality." Many various complex features of human behaviour are ignored to isolate certain defining causes: the model is defined by the clauses stating perfect knowledge of consumers and their desire to maximize wellbeing. Thus, as a simple example with reference to choices, one can explain an individual's choice of an apple over a cigarette with reference to the law that consumers aim to maximize their wellbeing. One can represent it in the D-N form and assert that given statements about antecedent circumstances that an individual desires an apple; he desires also a cigarette, but, for the purposes of exposition, cannot have both. Under the behavioral law that individuals aim at maximizing their wellbeing and additional empirical statements that choice of the apple rather than the cigarette maximizes an individual's wellbeing, one can deduce that the individual chooses the apple over the cigarette. However, this provides an explanation of choice with reference to one law, while other laws, or nomological functors, will be at play in the actual behavior of individuals, who may opt for the cigarette.

The question then arises how the model says anything about real events: in virtue of defining the regularities in the model by two clauses (perfect knowledge and maximizing wellbeing), for example, the model necessarily ignores material causes that are going to be relevant to any actual event – like those of addiction, or behavioral dispositions in certain social contexts, etc. Consumers do not have perfect knowledge of all relevant factors; nor do they aim to maximize their wellbeing in all instances.

Pemberton is, of course, correct: if an idealized model captures the relevant material causes of an event, then it is going to be able to tell something about that event. Moreover, since often one cannot know when a model will capture these material causes in economics, one cannot reliably use idealized models alone to give knowledge about economic phenomena. The problem Pemberton suggests is in locating the *right* causes in a given instance in idealized models in order for them to be useful. However, if idealizations are defined precisely in virtue of isolating particular clauses and ignoring other potentially empirically relevant ones, then it may not seem consistent to worry that the idealization should be assessed for its empirical relevance.

Recall that Hempel argued the D-N model is not about how scientists actually formulate their explanations. Rather the explicatory nature of the model is intended to render explanatory candidates more precise by providing an inferential framework against which to assess them. Likewise, if one assumes the D-N methodology, IMRACs need not be required to be about any particular event. Their RACs isolate particular clauses that render precise the relation between variables representing circumstances and employed regularities of the explanans and the explanandum, which is the statement about the event to be explained.

Hempel's categories of explanation can aid here in specifying the relation of idealizations to the broader methodology within which they are being employed: IMRACS (idealized models) can be characterized as complete when one can deduce specific conclusions from them. The model of choice between an apple and a cigarette can be characterized as complete when one can deduce an explanation of the choice of an apple in an idealized situation. However, one can characterize the application of that model to a particular situation as an explanatory sketch. It gives an inferential framework to fill in with relevant empirical statements.

Moreover, one can specify with Hempel's methodology what one should expect IMRACs to do. Employing the distinction between formal and empirical aspects of the application of models, one defines the model according to the formal aspects: the structure of the model gives inference rules for the idealized clauses employed within it. This serves to specify what the model is. But the empirical aspects are distinct. They pertain to how the idealized model is applied to a particular empirical situation and point to an empirical examination of the conditions under which, and the extent to which, the model actually provides a fruitful approximation of the empirical domain. It is the empirical examination of the application of the model that determines when the model has been successful. Acceptance of the model as useful therefore, pertains to the empirical assessment of its application. It does not relate to the idealized RACs that define the model. The point of the IMRAC is to provide the formal inference structure that indicates the way in which the explanans (RACs) connect with the explanandum (the statement about the economic event). The IMRAC is not meant to give an adequate picture of the empirical domain. It serves as the benchmark against which to assess actual applications in economics.

Pemberton uses another example of the Black-Scholes model. Black-Scholes derives a formula to value a stock option with a number of restrictive antecedent clauses. Pemberton notes that if the RACs are correct then the formula makes a correct prediction. However, he claims that the RACs are often false and generates a counter example to a prediction made by the Black-Scholes model. Thus he has shown that Black-Scholes can go wrong.

One may not be surprised by this. Given that an idealized model eliminates potentially relevant material causes, there necessarily will be counter-examples to its predictions. Pemberton's counter-example does not show anything more than an instance of when Black-Scholes does not apply. However the problem is not that Black-Scholes fails to capture relevant material causes in the counter-example, but that one might expect it to do this in the first place.

Furthermore, Pemberton argues that the RACs in a model are "false," but this conflates the formal/empirical distinction that is indispensible in clarifying the difference between an IMRAC's usefulness as an idealization within a methodological framework and the question of whether the framework should be accepted as empirically adequate. He suggests that RACs are false because they do not define a particular economic event with which one is concerned. However, RACs are not meant to characterize any real event, but rather are intended to characterize an idealized model, with which one can furnish empirical explanations of events. If one wants to assert that the assumptions of Black-Scholes are false, what is being asserted is that the assumptions do not accurately characterize the model – that the descriptions of the model are false. This is because, given the distinction between truth and acceptance, truth or falsity apply only within a closed formal framework – in this case the framework of the Black-Scholes model. Whether an explanation provided with that model is acceptable is a distinct question. Of course, what Pemberton means is that as false assumptions, they do not characterize any actual economic event. But this should not tell anything new if one maintains an unambiguous notion of the character of idealized models. The notion of falsity is misplaced here.

Thus, Pemberton obscures his conception of an IMRAC by asserting both that a model is defined by restrictive antecedent clauses, and that models must capture relevant material causes in order to tell anything about economic phenomena. Of course, *if* an idealized model does capture relevant material causes, it will tell something about that event. However Pemberton is mistaken to locate the source of this challenge in getting the right causes into the idealized model. Clarifying the nature of an idealized model suggests that the problem lies not in failed attempts to get the right causes into the model, but in the complexity of economic phenomena itself: in the (ibid, p. 10), "heterogeneity of the microcomponents of economics, such as consumers and commodities."

A clarification on the emphasis must be made here. One difference between the Black-Scholes example and the question of explanation is that Black-Scholes attempts to make projections about stock prices so that speculators can hedge against volatility of the pricing. The present discussion confines itself to explanation for the purpose of clarifying the point of the D-N model of explanation, excluding discussions about prediction. This is to argue that the D-N account – and by generalization from that account, models or theories in general – provides an explanatory framework for confronting phenomena (in Carnap's terms). Black-Scholes idealizes causes to provide a systematic framework. As idealizations, models ought not be understood to be about empirical phenomena, but ought to be understood as conventionally adopted a priori frameworks for characterizing phenomena with more or less specification.

So although the Black-Scholes model focuses on projections, one can characterize its framework according to the explanatory structure outlined by the D-N account – namely that a certain range of stock prices can be said to have been expected given statements of empirical conditions in the form of variables in the equation and regularities indicated by the functions of the model. And, moreover, if Black-Scholes is idealized precisely in virtue of excluding relevant material causes, then it cannot be expected to capture relevant material causes bringing about an actual stock price. It can only be expected to provide the approximations that are deduced more or less completely within the model and relative to its constitutive (idealized) assumptions. This not only locates the limits of Black-Scholes, but suggests, positively, how one might understand its function within a methodology construed according to the D-N account: Black-

Scholes provides a relativized a priori framework with which to formulate approximations of economic phenomena. An empirical assessment of its application can indicate how and when it can serve as a useful device in a systematic approach to organizing economic phenomena.

Now, perhaps it will be argued that this assessment misses the point. Pemberton has suggested that IMRACs are of limited use precisely because their empirical application is in question. However the point here is the D-N methodology recasts the problem from one of getting just the right causes into the model, to examining the empirical domain in which it is applied. The usefulness of an idealization is to give a systematic framework with which to confront the empirical domain. In this sense an IMRAC should not be considered as constituting the methodology itself, but as being a constitutive member within a broader framework that considers, not just the idealization, but the necessary empirical caveats and statements required to apply it to a given situation. Acceptance of an IMRAC pertains to the pragmatic aspects of the methodology within which it is found. Whether one uses Black-Scholes or some other formula for deriving projections is taken here as a sociological fact. The pragmatic question of its usefulness surrounds first, a clarification of the structure of the idealization and second, an assessment of the range of successes of its application.

A general claim about the D-N methodology may be made here. It was argued above that one of the lessons from the protocol sentence debate was that the philosophical methodology characterizing logical positivism could be the programme determining the extent to which one may pursue technical and historic-sociological aspects in given contexts. Further, it was suggested that Hempel's emerging philosophy of science can be seen to unify these two methodological aspects: the D-N methodology has a place for both technical considerations of the structure of employed models and internal formal problems that may arise, and the broader empirical considerations relevant to whether particular applications of those models might be accepted or not. In the case of Pemberton's IMRACs, the usefulness of idealizations lies in their providing a systematic framework for the application of a methodology. Though more successful models may be produced, it is attention to the domain of their empirical application that is relevant to whether they are accepted or not. Questioning the usefulness of an idealization on the grounds that it is not empirically adequate is to miss the intent of the idealization.

The canonical view considered and outlook

The argument for the orientation of the D-N account in this thesis affords a certain understanding of the point of Hempel's methodology that distinguishes itself from what can be called the canonical view. This final section will look briefly at how one may address standard views on the point of the D-N account by looking at one example of its portrayal.

In *Fact and method*, Richard Miller (1987, p. 3) writes that although few people would call themselves positivists, it is nevertheless the dominant philosophy of science. He (ibid) claims the most urgent task for philosophy of science is to develop a replacement that provides philosophical and practical guidance "that positivism promised." He (ibid) defines positivism as:

> the assumption that most important methodological notions—for example, explanation, confirmation and the identification of one kind of entity with another—can each be applied according to rules that are the same for all sciences and historical periods, that are valid a priori, and that only require knowledge of the internal content of the propositions involved for their effective application. Positivism, in this sense, is an expression of the worship of generality that has dominated philosophy at least since Kant: the idea that absolutely general, a priori rules determine what is reasonable.

For Miller, the problems for positivism lead to a certain class of post-positivist criticisms that he rejects. He argues, rather, that the failures of positivism lead to some version of realism. This may be fine, but the failure of positivism to deliver on its goals of providing an accepted systematic standard of philosophical assessment need not lead inevitably to discussions on either post-positivism or realism. Clarifying a 'positivist' programme suggests that Hempel, in particular, need not share the sorts of commitments attributed to positivists by Miller.[13]

First of all, Miller (ibid, p. 3) attributes to positivism the commitment that explanation "can ... be applied according to rules that are the same for all sciences and historical periods." As can be seen from the discussion above, Hempel's commitment is not to the assertion that explanation *can* be applied according to specified rules that are the same for all sciences. Rather, he argues that in general, explanations put forward in the natural and social sciences and history share the form outlined by the D-N model to a greater or lesser extent. And he argues that in contrast to the pragmatic aspects of explanation pertaining to when explanations are acceptable or not, an adequate philosophical methodology for analyzing science needs a systematic standard that does not fall prey to idiosyncrasies of individual inquirers. So he attempts to explicate one standard with the covering-law models (which may or may not be adopted in practice) in order to show how the methodology might work across a sufficiently wide domain to prove its worth.

Second, Miller (ibid) argues that positivists hold that explanation principles are "valid a priori" and that the appeal to the a priori is a commitment to a Kantian notion that "absolutely general, a priori rules determine what is reasonable" (ibid). It has been shown that debates among the left wing of the Vienna Circle did not embrace the Kantian notion of the a priori, but attempted, rather, to deal with the problem of the breakdown of the assertion that there can be such a notion of general a priori principles that can be given scientific formulation. As early as the Vienna Circle manifesto in 1929, the Vienna Circle

[13] Of course, 'positivism' can have many understandings. This thesis has considered the 'positivism' of Hempel as it can be understood to have emerged in debates with Carnap and Neurath.

members explicitly rejected the Kantian synthetic a priori (Neurath et. al. 1929, p. 308):

> The scientific world-conception knows no unconditionally valid knowledge derived from pure reason, no 'synthetic judgments a priori' of the kind that lie at the basis of Kantian epistemology and even more, of all pre- and post-Kantian ontology and metaphysics.... It is precisely in the rejection of the possibility of synthetic knowledge a priori that the basic thesis of modern empiricism lies.

To be sure, a priori notions are an important feature of the development of what has been called here a Carnapian constitutional system, but any such notions are assumed to be relativized and adopted via convention. It has been shown that, for Carnap – and Hempel who embraced Carnap's approach – certain features are taken as an a priori starting point for the construction of a system of scientific knowledge. One starts with an adopted class of basic observational predicates and a logical framework with which to articulate relations among statements within science. Though factual and formal features of any constitutional system are revisable in the face of further empirical developments, they are nevertheless necessary and taken a priori for the construction of the system in question. Likewise, for a philosophical methodology aimed at explicating explanatory candidates in science, the D-N form is taken a priori in the sense that some form is necessary to characterize explanation and this one appears to be widespread throughout scientific practice. As a methodology, it is employed to clarify what scientists appear to take themselves to offer when giving systematic explanations of the world.

Third, Miller (ibid, p.3) argues that positivists are committed to the notion that explanations require only "knowledge of the internal content of the propositions involved for their effective application." This point appears to be wrong, given the present historical assessment. Certainly, with regard to the logical aspects of explanation, one is concerned with the internal structure of propositions. In order to clarify an explanation, one attends to how the explanans are connected to the explanandum. However, for Hempel, the logical aspects of explanation pertain to clarifying an explanatory candidate. They do not preclude the pragmatic aspects of when one accepts an explanation. Indeed they point to further empirical research required to expand and develop hypotheses within an explanation. Idealized explanations are not about the particular phenomena. They give a framework with which to confront phenomena. As one moves farther into the empirical domain, an explanation diverges more and more from the idealized model. Hempel's positivism does not claim that the world fits his model: that is to get the situation the wrong way around. The D-N model is part of a methodological framework with which to confront the world. For Hempel it reflects what scientists generally take themselves to be offering and can be applied fruitfully to further clarify what is on offer.

This of course is not intended to be the last word. Rather, it suggests where further research might lead to flesh out this reconstituted interpretation. One glaring historical question that must be addressed is how such a view of Hempel's thought relates to the broader canon of his work. This chapter has located particular instances wherein principles of the historical roots of his thought appear to align themselves with a reconstituted understanding of a key idea for which he has become known (D-N as an explicatory model). However there needs to be further discussion on how it fits into his overall work, including later work wherein, as noted, Friedman has said Hempel converted from Carnapian logic of explanation to a reconstituted version of Neurath's historico-naturalism. It has been suggested in this thesis that such a conversion amounted to a shift of methodological focus within a relatively (though surely not strictly) unified philosophical outlook. Moreover, an understanding of Hempel's view surely might benefit from an assessment that incorporates richer categories than merely those characterizable as Carnapian in early and Neurathian in later life.

Further, the concept 'a priori' presents an interesting set of problems. One should be worried about what it means to employ a relativized a priori. Friedman (2001) provides what can serve as a fruitful launch point regarding what he calls a dynamic and relativized a priori and how that might shape future conceptions of the way philosophers view their modus operandi. In terms of understanding how any such feature fit within the intellectual landscape of positivists like Carnap and Hempel, Richardson (1998), treated in the second chapter of this thesis, affords a look at understanding Carnap in relation to neo-Kantianism. Both Friedman and Richardson indicate that early positivism is fruitfully understood to be dealing with the challenge of the breakdown of the Kantian synthetic a priori.

This thesis has mined a fragment of the history of a leading figure of twentieth-century philosophy of science in order to clarify his place therein. It does not argue that the way forward for philosophy of science is to return to logical empiricism (or positivism). Rather, insofar as its concern is at all about where philosophy might go, it suggests that an adequate historical understanding of the point of this great influence in the line of philosophical ideas may yet provide a resource to inform how one asks questions in philosophy of science. Whether philosophers take up the torch of D-N explanations in a reconstituted and revitalized form is matter for the course of history. But it is hoped in this thesis that Hempel's contribution might not sit unexamined as a naïve view on philosophical approaches to science that leads to disputes over what the 'right' account of explanation might be. Rather, a richer grasp of the emphasis and dynamics of the development of his thought might serve to provide a framework with which theorists of econometrics, economic methodologists or philosophers of science (among others) can contextualize their more technical pursuits to further clarify the scope and limitations of scientific knowledge.

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