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The Nature and Status of Scientific Metatheory. The Debate between Otto Neurath and Åke Petzäll

Critics from the Nordic countries played a significant role in the development of the philosophies of the Vienna Circle. By the time the first English-language monograph-length critical study of Viennese neopositivism (Weinberg 1936) was published—as well as A. J. Ayer's *Language, Truth and Logic* (1936)—several critical studies by philosophers from Sweden and Finland were already gathering dust: Eino Kaila's *Der logische Neupositivismus* (1930), Åke Petzäll's *Logistischer Positivismus* (1931) and his *Zum Methodenproblem der Erkenntnislehre* (1935).

With their authors having participated in the meetings of the Circle, these early monographs mirror the development of Viennese neopositivism as few other critical monographs do. Kaila focussed on the philosophical methodology and the doctrines adopted in Carnap's *Der logische Aufbau der Welt* (1928). His arguments were discussed in the Circle's meetings and Carnap published a friendly review (1930) in which he nevertheless rejected Kaila's criticisms, in particular of the method of quasi-analysis. Petzäll also seems to have regarded an *Aufbau*-style *Konstitutionssystem* as the ideal type of Viennese neopositivism, but he rather focussed on the difficulties to find a version of the empiricist meaning criterion that not only all members of the Vienna Circle could agree to, but also was able to withstand critical attention. His first monograph reached the sceptical conclusion that verificationism was caught in a paradox.

> It is evident that, if we wish to adhere to that concept of a sentence that follows with necessity from the concept of meaning adopted, then we cannot call the instance of verification a sentence. … The analysis extends beyond language, but that is impossible in principle. (1931, 34)[1]

Focussing on Schlick's rendition of verificationism in "Die Wende der Philosophie" (1930), Petzäll already located a central aspect of the complex of issues that a few years later would be highlighted critically by Neurath's response to Schlick's "Über as Fundament der Erkenntnis" (1934). This was the tension between the finality that the immediacy of phenomenal experience seems to bestow on our interpretation of it and the fallibility that arises from the demand that the content of this experience be communicable and knowledge be of a propositional nature.

1 Translations from sources for which no translation is cited in the bibliography are by the present author.

Petzäll also noted that the notion of logical form as that which was, according to the *Tractatus*, shared by states of affairs and statements about them, and which, according to Schlick, alone made knowledge possible, likewise stood in need of further clarification (ibid., 35-6). Petzäll put his finger on allergic points of the new philosophy very early on.

When he returned to consider the state of Viennese neopositivism four years later in his *Zum Methodenproblem der Erkenntnislehre*, Petzäll was justified to note that

> The subsequent literature has shown that the difficulties within the 'scientific world-conception', which were demonstrated in my work, were working themselves out, already at the time when that little piece was written, in such a way that the splitting apart into different directions was imminent. (1935, 12)

This time Petzäll considered not only Schlick's then latest writings but also those of the physicalist wing of the Vienna Circle. And once again, as we shall see, he reached a negative conclusion. Both wings of the Circle, Petzäll argued, were caught in—as it happened, complementary—contradictions.

Petzäll second monograph is a good example of the difficulties of understanding fully the the efforts underway on the so-called left wing of the Vienna Circle to fashion an entirely new approach to the theory of scientific knowledge. The short debate between Otto Neurath and Petzäll, in the journal *Theoria* in 1936 can serve as our focus here: Neurath reviewed Petzäll's 1935 monograph, calling forth a response by Petzäll which was followed by a rejoinder by Neurath. What renders this debate valuable still today is that in his response to Petzäll, Neurath was forced to clarify the relation between his own naturalist approach to scientific knowledge and Carnap's more formalist logic of science. While Neurath's remarks left several loose ends, what emerged from them nevertheless was the outline programme for a bipartite metatheory of science comprising both formal investigations focussed on the logic of science as well as empirically informed analyses of scientific theorising (data acceptance, theory change etc.).[2]

1. Petzäll's *Zum Methodenproblem der Erkenntnisforschung*

Petzäll's *Zum Methodenproblem der Erkenntnisforschung* represents one of the very first accounts of what we now call the Vienna Circle's "protocol sentence debate" by a non-participant. (In this respect it may be compared with the introductory sections of Hempel's contemporaneous "On the Logical Positivists' Theory of Truth" (1935) which, however, also went on to contribute to that debate.)

2 Here I concentrate on the published Neurath-Petzall exchange and neglect their correspondence.

Petzäll's discussion in section 2 of his monograph covers, in this order, Carnap's "Überwindung der Metaphysik durch logische Analyse der Sprache" (1932a), Neurath's "Physikalismus" (1931) and "Soziologie Physikalismus"(1932a), Carnap's "Die physikalische Sprache als Universalsprache der Wissenschaft" (1932b), Neurath's "Protokollsätze" (1932b), Carnap's "Über Protokollsätze" (1932c) and *Logische Syntax der Sprache* (1934), Schlick's "Über das Fundament der Erkenntnis" and Neurath's "Radikaler Physikalismus und 'wirkliche Welt'" (1935). Petzäll clearly discerned that in the early 1930s Carnap moved from a position sympathetic to the psychologistic reading of the *Tractatus* that was then *en vogue* (with Wittgenstein's blessing) towards Neurath's more radical physicalism and that by 1934 the Circle's "splitting apart into different directions" was irreversible.

In Petzäll's monograph, however, this discussion of the protocol sentence debate, titled "The Problem of Knowledge Presented in a Concrete Example", is sandwiched between a short introductory section, headed "The Anarchy in Epistemology", and a long third section, titled "A Possible Starting Point for the Investigation of Knowledge" in which the author advanced his own suggestions for how the current impasse could be overcome. Petzäll's purpose emerges only gradually in this monograph. When in the first section Petzäll echoed the Vienna Circle's complaint about the current anarchy of philosophical systems and resolves to investigate their epistemological efforts due to their plea for clarity and intelligibility, one senses (as in his earlier book) a certain sympathy with his subjects. Early in section three, however, it becomes evident that Petzäll's own position is by no means so easily defined. Instead, Petzäll effects the stance of a cautious sceptic vis-à-vis epistemology, vaguely reminiscent of the Neo-Friesian Leonard Nelson's "Die Unmöglichkeit der Erkenntnistheorie" (1912). Petzäll claimed that his investigation of the epistemologies of the Vienna Circle demonstrated what holds for epistemology generally, namely, that "the time has not yet come for a *theory* of knowldge" (1935, 51, orig. emphasis).

Neurath's response engaged directly only with Petzäll's critique of physicalism. It must suffice here to note two things about Petzäll's alternative approach. First, it is notable that the issue over which, in Petzäll's view, the Viennese neopositivists came to grief, was no different from that which already afflicted Kant's and all epistemologies since, namely, the tension between genetic or causal inquiry and normative reasoning:

> Kant was unable to conduct his *quaestio juris*-investigation without falling back on the *de facto* obtainment, in the epistemic subject, of the condition that something is valid for somebody. The presupposition of validity as an a priori form in the transcendental apperception becomes a fact of the cognitive process in its actual course. ... But the tension between the descriptive genetic method on the one hand and the investigation of the grounds of validity on the other is not only characteristic for those researches that are based more or less on Kant. The contrast between pragmatism and fictionalism on the one and phenomenology on the other shows the two perspectives in sharp antithesis, but at the same time we

can observe a peculiar fusion of both elements in both of these tendencies. And finally we witness in logical positivism how in Schlick the psychological and physiological genesis of a certain cognition becomes the ground of the validity for the propositional system of the language of science that is amenable to formal analysis alone. (Ibid., 58-9)

For Petzäll, the frequent recurrence of this problem raises the question "whether the two points of view can be separated at all" (ibid., 59).

Yet Petzäll did not rest content with the fact that previous epistemologies were unable to bridge "contrast between genetic explanations and investigations of validity" (ibid., 58). The second point to note is that while he was happy to declare that there did exist a distinct "epistemological question" ("Erkenntnisfrage"), Petzäll conceded that its precise form is not yet known. "Its approximate meaning could be expressed by the question of what relation obtains between logical form and empirical content." (Ibid., 65) However, Petzäll also held that the pursuit of the epistemological question must not beg the sceptical question. Dogmatism in epistemology can be avoided, Petzäll argued, only by adopting a "provisional formula" that as yet avoids any characterization of what is known (and so stays clear of the ancient problem of the criterion): "We would formulate the provisional formula for the investigation of knowledge as follows: *'what is the meaning of validity claims?'*" (Ibid., 69) Investigating the question of the meaning of claims to epistemic validity was to cast light on the previously intractable issue of the relation of descriptive and normative questions and of that of empirical content and logical form.

Petzäll was aware that his diagnosis of the central issue of epistemology recalled and reopened the foundational debates around the previous turn of the century about the mutual relation of logic and psychology.

Our formula states with regard to the role of psychology and logic in the investigation of knowledge, that both methods complement each other in a field of which it is true to say in principle that it belongs neither to psychology nor to logic, but whose specific issues are only done justice to, if the methods of psychology and logic are combined. Claims to validity do not fall under any one of these sciences separately. They can only be comprehended by both together. (Ibid., 83)

Petzäll's diagnosis is not without its own obscurities. Clearly, his intention was to "inaugurate the investigations of all factors that enter as components into that which is in one or another way can be associated with the term 'knowledge'" (ibid., 76). (The distance from logical positivism that Petzäll sought to gain by this procedure, for instance, found expression in his hope that this procedure would "avoid all dogmatic couplings of 'thought' and language" (ibid., 78).) Yet his attempt to cover all bases seems beset by considerable tensions. Being naturalistic in some respects—precisely in wanting to investigate all factors and reject the sharp exclusion of causal considerations from normative investigations—yet traditionalist in others—in wishing not to beg the question of scepticism—it is difficult to see

how all his desiderata could be met. Still, Petzäll's plausible suggestion was this: before a "theory of knowledge" is attempted, we better become clearer about what the "problem of knowledge" is in the first place: to contribute to this alone was the professed point of his "reflections about the question of method in the investigation of knowledge" (ibid., 82-3).

2. Petzäll's Critique of Logical Positivism

Beginning his critique of logical positivism, Petzäll stressed that throughout he aimed for an immanent critique: the failings of the views analysed were to be shown by following the principles professed by the authors under investigation. In particular, Petzäll took very seriously the desideratum that philosophy should not transgress the bounds of language and he ascribed to Carnap and Neurath the aim to avoid the contradiction that he previously (in his first monograph) had diagnosed in Schlick. In doing so he certainly identified problematic points in Carnap's early physicalism.

Noting the central role of protocol statements in the elimination of metaphysics such that only empirical and logical statements remained, Petzäll began by asking of Carnap's position (in "Überwindung") whether the protocol sentences "were regarded as *empirical* statements on account of a formal, logical feature of theirs" (1935, 15, orig. emphasis). Relatedly, he asked of Neurath's position (in "Soziologie im Physikalismus") what distinguished the protocol sentences, especially so-called reality statements, from other statements featuring spatio-temporal determinants. In both cases Petzäll found that no answer was forthcoming and he voiced the suspicion that the relevant distinction could only be drawn by relying on resources that were not officially available: Carnap had limited philosophy to formal inquiries and Neurath had rejected the conception of (correspondence) truth.

Probing further the claim of physicalism that all meaningful statements can be expressed in the language of physics, Petzäll focussed on Carnap's admission (in "Universalsprache") that "in establishing the scientific system there is ... an element of convention, i.e. the form of the system is never completely settled by experience and is always partially determined by conventions" (1932b [1934, 49]). Since Carnap did not specify what these conventions were, the relation of protocol sentences to the other sentences of the scientific system was unclear. Moreover, in answering the question of how it became possible that the qualitative protocols of individuals were translatable into the quantitative language of physics, Carnap was forced, Petzäll argued, to "attribute to the protocol language a quality that has *nothing* to do with its logical nature" (1935, 24, orig. emphasis). Carnap held "that determinations of this kind are theoretically always possible is due to the fortunate circumstance (an empirical fact, not at all necessary in the logical sense) that /the

protocol//the content of experience/ has certain ordinal properties" (1932b [1934, 61]) and that this also holds for "the structural correspondences between /the protocols//the series of experiences/ of the various experimenters" (ibid., 64).[3] Carnap went on to claim that "these facts, though of an empirical nature, are of far wider range than single empirical facts or even specific natural laws. We are concerned here with a perfectly general structural property of experience which is the basis of the possibility of intersensory physics ... and intersubjective physics, respectively" (ibid., 65) Petzäll was surely right to comment that "these lines would fit better into the *Critique of Pure Reason* than where they are. For Kant they would not amount to a radical inconsistency." (1935, 25) He concluded:

> The result is thus reached that in proving the fundamental thesis of physicalism Carnap is forced to apply a procedure which he himself is forced to consider scientifically illegitimate, i.e. that belongs neither to the sciences nor to logic." (Ibid., 26-7)

Matters did not improve, so Petzäll, at the next stage of the debate. Noting Neurath's opposition (in "Protokollsätze") to Carnap's retention up until then of "original" protocol sentences that do not stand in need of justification, Petzäll remarked that "strangely enough" he did not criticise Carnap's "extra-logical speculations about the 'general structural property of experience'" (ibid., 29). Carnap's subsequent embrace (in "Über Protokollsätze") of a still more pronouced conventionalism concerning the language of science, in particular of the characterisation of protocol sentences, was likewise rejected by Petzäll. "If the form of these sentences is arbitrary, then one obviously cannot any longer speak of their special structural constitution." (Ibid., 32) This rendered Carnap's old method A invalid. Petzäll noted correctly that Carnap's new fallibilist method B had no need any more to invoke such an assumption, but there he found, unsurprisingly, that no good reason was provided to stop testing at one point rather than another.

Petzäll summarised the dilemma which he saw facing the physicalists as follows:

> If we limit, as Neurath wants to, the logic of science to the logical synatx of language, then language does not say anything; if we do not want to accept this consequence, then the logic of science must be more than the mere logical syntax of language. (Ibid., 35)

What prompted Petzäll were two things: first, Neurath's claim made in the course of rejecting the correspondence theory of truth that "statements can only be compared with statements" (1931 [1983, 53], 1932a [1983, 66]); second, Carnap's denial in *Logical Syntax* that "all logical investigations comprise two parts: a formal inquiry which is concerned only with the order and syntactical kind of the

3 The expressions on either side of the double forward slash belong to Carnap's formal mode and material mode of speech, respectively (appearing in parallel columns in the original).

linguistic expressions, and an inquiry of a material character, which has to do not merely with the formal design but, over and above that, with questions of meaning and sense" and his claim that "the formal method comprises all logical problems, if conducted sufficiently thoroughly, even the so-called material or meaning problems (as long as they are truly logical and not psychological problems)." (1934 [1937, §73]) Petzäll clearly took these remarks to mean that meaning had no role to play in physicalism and logical syntax, perhaps even that these doctrines denied the phenomena of meaning altogether. The alternative conception that logical syntax was meant to capture all those aspects of meaning that were relevant for epistemology (in parallel to the *Aufbau's* dismissal of the unfortunately named "epistemic content" in favour of "logical content") was not considered—nor that Neurath aimed to replace the theory of truth with a theory of protocol acceptance (as in his 1932b).

Turning to Schlick, Petzäll again found himself confronted with the unsolved problem of properly delimiting the special sentences "which Schlick does not want to call 'protocol sentences' but 'fundamental sentences'" (1935, 39). The "novelty" of Schlick's approach lay for Petzäll "mainly in the heavy stress on the psychological or 'physiological' character or, if you like, on the non-linguistic nature of a certain cognitions" (ibid., 43). Even though he criticised Neurath's response to Schlick as unhelpful, Petzäll remained unimpressed by Schlick's efforts.

Petzäll's overall conclusion was this:

> The debate we have focused upon for detailed consideration thus shows us that it contains an unsolved problem that makes itself felt against all resistance and that due to its own dynamic produces a sharp and principled opposition between epistemologists who jointly reject the previously accepted philosophical questions as pseudo-problems and aim to distance themselves from traditional epistemology. Carnap and Neurath persist against all consequences with trying to get away from the question, without success. Schlick 'solves' it, without success. (Ibid., 47)

Thus we reach the unsolved problem that Petzäll called "the question of knowledge":

> We thus come upon the question of knowledge ... in two guises: in Schlick, in the guise of the question of what the nature of the element of experience is that makes verification absolute; in Carnap and Neurath in the guise of the question of what the relation is between the form and the content of what is said. Both of these guises of the question of knowledge can be summarised in a more general formulation: what is the relation between pure logical form and experience? (Ibid., 50)

With that formulation in hand, Petzäll then turned to develop his alternative approach to the "question of knowledge" (which we briefly surveyed above).

3. Neurath's Response

It would be difficult to imagine Neurath responding to this monograph had it been written by a philosopher in Germany. Instead, his engagement seems to presage Philipp Frank's response to Cassirer's book on determinism two years later in the same journal (1938). The absence of undue polemics in Neurath's response suggests that the point was to build bridges to Cassirer's Göteborg.

Recognising that he and Petzäll were "adherents of different views", Neurath conceded right away that "the rapid development of modern logical empiricism had the effect that there are still all sorts of points in dispute, even within the Vienna Circle" (1936a [1983, 159]). Instead of carefully going through the criticisms point by point, however, Neurath proposed to "elucidate the problem of knowledge as formulated by [Petzäll]" from his own standpoint. His own central point he characterised as follows.

> A radical physicalism—this is to denote the total conception, not only a special tenet—does not lead to a theory of knowledge of its own, as Petzäll demands. If, e.g., within physicalism we use the term 'validity', we deprive it of any 'absolute' meaning and avoid what we call 'pseudo-problems' of the theory of knowledge. We best start from the operation of science and look at its procedure. (Ibid.)

This, as it were, anti-philosophical point of Neurath's stood in clear contrast to Petzäll's distinctly philosophical stance. Petzäll had not announced it as such, but it shines through clearly when he noted about Carnap's intention, largely due to Neurath's influence, to put increasing distance between his and Wittgenstein's conception:

> That he cannot stay within the limits of his own method is not due to his misunderstanding Neurath's opposition, but due to the impossibility in principle for physicalism to justify physicalism with its own methods. (1935, 27)

The force of Petzäll's criticism is clear: physicalism relies for its plausibility on assumptions which it is in no position to substantiate. For Neurath, however, this constituted "no reproach" (1936a [1983, 165]).

What was Neurath's meaning here? It was not that physicalism could make arbitrary assumptions, but that physicalism situates the investigation of knowledge entirely differently from traditional epistemology. The investigation of knowledge was not to be undertaken by a philosophy that aimed for autonomy from empirical science in proving valid its own foundations (as Petzäll seemed to presuppose). Instead, as an investigation of scientific knowledge, it was reflectively undertaken by science itself. For Neurath, the philosophy of science was part of science as its own metatheory.

Importantly, this metatheory came in two parts: what he called "the behaviouristics of scholars" and logical investigations (ibid., 160). Neurath gave the following examples. To the former belong statements like "'The scholars of a certain epoch made experiments, undertook voyages of exploration, formulated statements of a certain kind' or 'Scholars who are under the influence of great amounts of alcohol formulate different statements than scholars who have consumed no alcohol'"; to the latter belong statements like "'This group of statements is of equal content with a second group of of statements of the same language' or 'This statement is in contradiction with other statements within a certain system' or 'From the statements "Homer is a Negro", "all Negroes are poets" follows "Homer is a poet"'"(ibid.). While Neurath here spoke simply of "logic" he clearly meant what Carnap called "logic of science". As can be seen from his examples, Neurath expected the logic of science to deal with issues of logical relations like consistency and entailement and with issues of meaning in so far these did not concern psychology.[4] What Neurath called the "behaviouristics of scholars" I shall call—with Frank (1957, 360)—the "pragmatics of science": it concerned, to begin with, descriptive statements from the history, psychology and sociology of science. But note that for Neurath also "the term 'accept' belongs to behaviouristics. We can think of the mass of statements that we accept as being unified in an encyclopedia." (1936a [1983, 160]). This raises the question of whether and where or how normative questions concerning data and theory acceptance find a place in Neurath's scheme. (I will return to this.)

Now, since both the pragmatics and the logic of science are second-order inquiries it can hardly be demanded of them that they are autonomous in that they can in some sense prove their own presuppositions for their very object, first-order science, which is given to them in the form in which they find it. That Petzäll claimed "the impossibility in principle for physicalism to justify physicalism with its own methods" constituted "no reproach" therefore. But neither can we expect science as a whole, the combination of first- and higher-order inquiries, to be so autonomous, for first-order inquiries of an empirical nature typically depend on what is given to them in experience and experiment. The idea of philosophical self-sufficiency that radical scepticism trades on, simply had to be abandoned.

How then did Neurath propose to treat questions that Petzäll found troubling, for instance, how did he ensure that his collections of accepted statements were of an empirical nature? Neurath's answer was that "the reduction of testing with observation statements—protocol statements—would determine the empiricist character of the encyclopedia" (ibid., 161). But what ensured that protocol statements were of an empirical nature? This was to be answered not by the logic of science alone, but only in conjunction with the pragmatics of science. Very roughly, the logic of science determined protocol statements to contain a certain type of terms

4 Neurath simply disregarded Petzäll's overreaching criticism that logical syntax reduced all statements to tautologies.

the application of which the pragmatics of science showed to be fairly directly responsive to experience. The precise specification of the terms in question depended on the favoured form of observation statements: on this issue there obtained a difference between the physicalists Neurath and Carnap which Neurath chose not to discuss. Here it may be added, therefore, that it is characteristic that Carnap left it for psychology to determine which predicates should be considered observational ones (1936/37), while Neurath insisted on the use of perception terms in the formulation of protocols (1932b). Of course, Neurath's way of reasoning—nowadays we'd call it "naturalistic"—would not have satisfied a philosopher who sought to establish the possibility of knowledge against the threat of radical scepticism. But this was one ambition that neither Neurath nor Carnap shared.

It fits with this outlook that Neurath refused to attach to the term 'valid' an "'absolute' meaning". There are two aspects to this. First, there is Neurath's problematic rejection of any truth talk which he felt "leads to all kinds of difficulties" (1936a [1983, 161]). Believing truth talk somehow inherently to involve a metaphysical conception of correspondence between linguistic entities and the world (for the postulation of such a relation lay beyond empirical control), Neurath resolved to make do without truth and instead only use the concept of acceptance. We may leave open here the question whether this still allowed him to develop a somewhat minimalist epistemology, but it is clear that such fastidiousness precludes the appreciation of standard semantics and all that this entails. (I will not try to defend this strand of Neurath's thinking.)

A second aspect of Neurath's anti-absolutism is that he also rejected talk of "verification" or "falsification", "even of a 'limit' to which confirmation or shaking would approach". ("Shaking" was Neurath's term for disconfirmation.) His reason was that "verification and falsification need as premise the use only of precise terms", whereas "the total encyclopedia with all its observation statements necessarily also contains terms that are just precise enough to be used within certain boundaries" (ibid., 161-2) what elsewhere he called "Ballungen". What shines through here, but again was not designated as such by Neurath, is an important difference in the conception of the "universal language of science". The basic language of a Neurathian encyclopedia was distinct from the language that Carnap had claimed was universal in his "Universalsprache" of 1932, the precise and wholly quantitative language of mathematical physics. Neurath reasoned that, typically, protocol statements contain imprecise terms, so hypothesis testing in science is inevitably afflicted with imprecision. Ultimately this meant that, for Neurath, even protocol sentences were "not absolutely distinguished either by terms or validity" (ibid., 164) and were themselves accepted or rejected in the light of more or less theoretical considerations.

Neurath summed up his alternative conception of investigations into knowledge:

Physicalism uses the concept of 'validity' in a historical sense and with reference to a certain mass of statements, it does not arrive at formulations of 'dignity' … that should somehow lead us to 'the real world', to 'the one true world in itself'. (Ibid., 165)

"Metaphysical dignity" was Petzäll's descriptive term for the distinction that historically had been held to be enjoyed by universal truths (1935, 58); whether Petzäll himself actually hankered after such solutions of the "question of knowledge" is not entirely clear, but Neurath evidently sought to shut this door very firmly:

… one would never get to the confrontation of statement and reality; language and reality; thinking and being; knowledge and reality; subject and object; logical form and experience; etc.—all these are formulations that Petzäll employs as the starting point for considerations of epistemological considerations (cf. his pp. 7, 56, 60, 66)." (1936a, [1983, 163], trans. altered)

There were still other points on which Neurath sought to correct Petzäll—physicalism did not hold that all the statements or laws of the special sciences reduced to statements or laws in physics (ibid., 164) and it did not accept the narrow behaviorism of Watson (ibid.)—but it is fair to say that Neurath rested his case as follows.

Though much may still need clarification, the present state of research gives no cause for the assumption that we need specific terms and specific statements of a separate 'theory of knowledge' besides the statements of science as a whole (including the logical disciplines) for the building up of our science. (Ibid., 166)

Or, as Philipp Frank was to put it some fifteen years later, "the fact that no special science can … 'defend its own principles' does not lead to the conclusion that the system of all sciences cannot do so" (1951, 30). In their different contexts Neurath pointed to the combination of empirical and logical inquiries and Frank to that of the natural and the social sciences; shifting to the distinction between the logic and the pragmatics of science, we could in Neurath's spirit say with Frank that their cooperation "would reach the objectives that that were formerly reserved for philosophy" (ibid.).

4. Petzäll's Reply and Neurath's Rejoinder

Yet does the rejection of correspondence truth and realism not lead to relativism? This is a counter one could expect from a theorist of knowledge with traditional philosophical ambitions, but that was not how Petzäll did react. It seems Petzäll was genuinely surprised by the strategy that Neurath had chosen which he characterised as follows:

> According to Neurath, the task of the so-called theory of knowledge is taken over by two sciences, by the logic of language and the so-called behaviouristics of scholars. (1936 [1983, 166])

Petzäll's concern lay in learning "more about the way in which these two spheres of research participate in the construction of the encyclopedia" (ibid., 167). Since the precise wording of the questions he put to Neurath is less significant than the point of the answers received—Petzäll detected confusions between the logical and the behavioural spheres in certain formulations which Neurath then disambiguated—I turn straight to Neurath's response.

> The question which contradictions can just be tolerated, which not, how one behaves altogether in the development of the whole of science, is a question of behaviouristics, of history of science, of behavioristsics of scholars. But the discussion of contradictions, the discussion of the question which groups of statements are logically of equal content, belongs to the sphere of logic. If I am occupied with the behaviour of people who produce encyclopedias, I am concerned with behavioristics; if I am occupied with the logical interconnecting of the statements themselves, I am not concerned with behavioristics. (Ibid., 169).

As so often, Neurath's own clarifications are not fully self-explanatory. For instance, when Petzäll distinguished what I called the two "branches of metatheory" as "two sciences", he may have had it in mind that both make a different contribution to "the construction of the encyclopedia", one descriptive, the other normative. But precisely concerning the distinction between descriptive and normative inquiries, Neurath's remarks seem to remain studious silent.

On a second look, however, these remarks can also be read as studiously ambiguous. Neurath's quoted sentences can be read as themselves concerned with descriptive or with normative questions in mind: "can be tolerated", "how one behaves", "being occupied with"—of all of them we can ask "in what sense"? Moreover, this holds not only with regard to the questions in the pragmatics of science, but also in the logic of science itself given that "the" logic is no longer given and that therefore any descriptive judgement about the logical relation of two statements is dependent on the prior decision of which logic was or is to be adopted. Lastly, it must be recalled that for Neurath the distinction between descriptive and normative questions was not a fundamental and categorical one—given that kind of normativity was concerned that he recognised (as opposed to one he rejected as metaphysical). To be sure, Neurath had no time for deontological norms of a Kantian variety, but he agreed with Max Weber that it remained within the purview of scientific investigations to determine instrumental norms. These depended, after all, on observable means-ends relations: given a stated end, it was an empirical matter what type of course of action was more likely to lead to success than another. Such instrumental normativity Neurath had explicictly defended as legitimate in "Soziologie im Physikalismus" by comparing their status

to recipes for cooking, for instance, and it was this defense that he now happily fell back on.

So questions of instrumental normativity were not categorically distinguished from descriptive matters for Neurath and raised no special problems. Specify the parameters of which ends are pursued under what circumstances and descriptive inquiries will deliver answers to "which contradictions can just be tolerated, which not" in this normative sense. Just as the logic of science holds both a descriptive and a normative office—it describes what follows from what, given the rules of the logico-linguistic system under investigation, and so prescribes what someone who has adopted this system should deduce—so the pragmatics of science has both a descriptive and normative office: it can describe what are appropriate methodological means for given cognitive ends and thereby legitimate their adoption for these purposes. In a fuller discussion Neurath would perhaps have addressed this matter explicitly as well, but in this rejoinder he kept matters short.

In retrospect, of course, Neurath can be blamed for a certain blindness that also affected Carnap at the time: instrumental normativity is not as plain a concept as they apparently took it to be. Moreover, whether the aim of scientific theories should be successful prediction—"an occupation with predictions and their testing" is what "logical empiricism leads to" (ibid., 170)—or not is debatable. (Does the idea of empirical adequacy capture the nature of the scientific enterprise as a criterion of theory choice?) Yet one can admit this and with Neurath also ask whether such questions require a distinct, philosophical theory of scientific knowledge—or whether they are best understood as discussions within metatheory itself.

5. The Relevance of the Petzäll-Neurath Debate

Given that Neurath's conception of a bipartite metatheory as a replacement for philosophy has been widely disregarded in logical empiricism, it is doubtful whether the debate between Neurath and Petzäll was noted by fellow logical empiricists at all—apart from Hempel and Frank, who also published in *Theoria* in the following two years, and Carnap.

Hempel's paper "De la problème de la vérité", in fact, explicitly referred to this exchange and sought to clarify some issues raised about the notion of truth in the light of Tarski's theory. In doing so, he practically took Neurath's side in the debate with Petzäll, noting that "Tarski's theory [of truth] does not concern the criteria by which the system of the propositions of the empirical sciences is established" (1937 [2000, 54]). In his own work, however, Hempel soon abandoned any concern with the pragmatics of science for work on formal confirmation theory in the logic of science, only to return to the pragmatics of science late in his life (see Friedman 2000).

Frank's contribution to *Theoria* does not explicitly signal his adoption of the bipartite conception of metatheory, though he adopted, like Neurath, Karel Reach's convention of describing metaphysical statements as "isolated" ones (1938 [1949, 173]). But already in 1932 Frank had written:

> The events around Galileo make it clear that the passionate conflicts connected with a physical theory have nothing to do with its suitability to represent natural processes but much more with their relationships to the political and social events of the time. Therefore there is no need to amplify the positivist conception of science by a metaphysical concept of truth but only by a more comprehensive study of the connections that exist between the activity of the invention of theories and the other normal human activities. (1932 [1998, 14])

There Frank clearly suggested adding the sociological dimension to the theory of science that at the time was conducted mostly in terms of the analysis of the symbol system it used (and that in syntactic terms). This of course meant adding something like a behaviouristics of scholars to the logic of science. Frank can thus be seen to have anticipated the distinction which Neurath first drew explicitly in the debate with Petzäll. He can therefore be counted as a supporter of the conception of a bipartite metatheory. Indeed, in the 1950s Frank mainly worked on the pragmatics of science (1951, 1954-6, 1957), but this work was increasingly ignored in mainstream philosophy of science.

But what about Carnap? Here we come to one reason why Neurath may have chosen to respond to Petzäll in the way he did. Not only did Petzäll offer him an opening of sorts by himself suggesting that both causal and normative inquiries pertain to "the epistemological question" against which Neurath could show how this was really to be done, but he also afforded him an opportunity to address an issue that had arisen within the physicalist wing of the Vienna Circle. Just what was the relation between Carnap's purely formal logic of science and Neurath's naturalistic approach to theorising about science? Given that the protocol sentence debate had ended inconclusively with all of Schlick, Carnap and Neurath espousing different conceptions of their favoured form of protocols, it stood to reason to ask even whether Carnap's and Neurath's approaches were still compatible.

Consider what Carnap said about the logic of science as the successor to philosophy in §72 of *Logical Syntax*, appropriately entitled "Philosophy Replaced by the Logic of Science". Carnap began with a basic distinction of types of discourse. (Significantly enough, he allowed himself use of the material mode of speech, not the formal mode of speech which at the time would have constricted him to matters of syntax alone.)

The questions dealt with in any theoretical field ... can roughly be divided into object-questions and logical questions. ... By object-questions are to be understood those that have to do with the objects of the domain under consideration, such as inquiries regarding their properties and relations. The logical questions, on the other hand, do not refer directly to the objects, but to sentences, terms, theories, and so on, which themselves refer to the objects. (1934 [1937, 277])

With the basic distinction between first- and second-order (or generally higher-order) discourses in hand, Carnap turned to consider the proper domain of philosophy.

According to traditional usage, the name 'philosophy' serves as a collective designation for inquiries of very different kinds. Object-questions as well as logical questions are to be found amongst these inquiries. (Ibid., 276-7)

Carnap went on give examples of such supposedly philosophical object-questions. Some of them concerned

suppositious objects which are not to be found in the object domains of the sciences (for instance, the thing-in-itself, the absolute, the transcendental, the objective idea, the ultimate cause of the world, non-being, and such things as values, absolute norms, the categorical imperative, and so on) (ibid., 278),

others concerned "things which likewise occur in the empirical sciences" (ibid.). About the logical questions he remarked that they

occur principally in the logic (including applied logic), and also in the so-called theory of knowledge (or epistemology), where they are, however, for the most part entangled with psychological questions". (Ibid.)

Finally,

the problems of the so-called philosophical foundation of the various sciences ... include both object-questions and logical questions (ibid.)

Carnap's own findings stoods in opposition to this traditional understanding:

The logical analysis of philosophical questions shows them to vary greatly in character. As regards those object-questions whose objects do not occur in the exact sciences, critical analysis has revealed that they are pseudo-problems. The suppositious sentences of metaphysics, of the philosophy of values, of ethics (in so far as it is treated as a normative discipline and not as a psycho-social investigation of facts) are pseudo-sentences; they have no logical content, but are only expressions of feeling which which in their turn stimulate feelings and volitional tendencies on the part of the hearer. In the other departments of philosophy the psychological questions must first of all be eliminated; these belong to psy-

chology, which is one of the empirical sciences, and are to be handled by it with empirical methods. (Ibid.)

So much for idealist metaphysics and its relatives. But Carnap did not stop there.

The remaining questions, that is, in ordinary terminology, questions of logic, of the theory of knowledge (or epistemology), of natural philosophy, of the philosophy of history, etc. are sometimes designated by those who regard metaphysics as unscientific as questionsof scientific philosophy. As usually formulated, these questions are in part logical questions, but I part also object-questions which refer to objects of the special sciences. Philosophical questions, however, according to the view of philosophers, are supposedd to examine such objects as are also investigated by the special sciences from a quite different standpoint, anemly, from a purely philosophical one. (Ibid., 279)

Not surprisingly, Carnap had no patience with this supposedly "philosophical" perspective on object-questions.

As opposed to this, we shall here maintain that all these remaining philosophical questions are logical questions. Even the suppositious object-questions are logical questions in a misleading guise. The supposedly peculiarly philosophical point of view from which the objects of science are to be investigated proves to be illusory, just as, previously, the supposed peculiarly philosophical realm of objects proper to metaphysics disappeared under analysis. (Ibid.)

Thus:

Apart from the questions of the individual sciences, only the questions of the logical analysis of science, of its sentences, terms, concepts, theories, etc., are left as genuine scientific questions. We shall call this complex of questions the *logic of science*. (Ibid, orig. emphasis)

Accordingly, "once philosophy is purified of all unscientific elements, only the logic of science remains" (ibid.). Note that Carnap's analysis here is in fact not disabled by the fact that at the time he possessed a particularly narrow understanding of the logic of science, namely, as "the syntax of the language of science" (as §73 was to argue). His rejection of the traditional understandings of philosophy remained the same once the semantics of the language of science was admitted into the logic of science (as it was soon after).

Now it is clear there is one reading of these passages on which Carnap's delimitation of the successor discipline to traditional philosophy renders problematic the idea of a bipartite metatheory I attributed to Neurath and Frank: *legitimate philosophy comprised only the logic of science, nothing else*. It also seems to me that there is little doubt that Carnap was widely understood in this way and continues to be so. Understood in this way, of course, a sharp contrast opens up between his conception and Neurath's "behaviouristics of scholars". Given, moreover, a

comparison of the exemplary clarity with which Carnap's inquiries proceeded with Neurath's decidely less clear explorations, it is perhaps no surprise that not only did the view that their perspectives were irreconcilable won the day, but that Neurath's perspective was not taken up in the burgeoning movement of logical empiricism.

Yet this is not the only reading possible. Consider what philosophy is qua logic of science: an a priori, not an empirical inquiry. To designate the logic of science as the heir to traditional philosophy is thus to stress the one point of continuity that obtained between them: the presumed fact that philosophy was separate from science and possessed its own distinct methodology. Just this, of course, allows the easy assimilation of Carnap's "logic of science" to Reichenbach's "analysis of science" in his influential *Experience and Prediction* (1938). But it is also one of the differences between Carnap's logic of science and Reichenbach's analysis of science that points to an alternative reading of the relation of Carnap's logic of science to Neurath's naturalistic inquiries. Whereas Reichenbach allowed into the analysis of science not only the problems of logic, probability theory and, importantly, "all the basic problems of traditional epistemology" (1938, 8), Carnap stressed that to designate his logic of science as "theory of epistemology (or epistemology)" is

> not quite unobjectionable, since it misleadingly suggests a resemblance between the problems of of our logic of science and the problems of traditional epistemology; the latter, however, are always permeated by pseudo-concepts and pseudo-questions, and frequently in such a way that their disentanglement is impossible. (1934 [1937, 280])

Thus when Carnap declared that "the logic of science takes the place of the inextricable tangle of problems which is known as philosophy" (ibid., 279), he also announced a much sharper break between traditional philosophy and his logic of science than did Reichenbach for his analysis of science. It is just this difference from Reichenbach that places Carnap back in the company of Neurath.

To see this, note that his logic of science not only possesses, as befits any logic, an a priori methodology—it provides justifications for its pronouncements on the bais of reasoning on a priori grounds—but also that it is clearly designated as a second-order inquiry. Now consider what Carnap added (in square brackets) following his introduction of the designation "logic of science" (quoted above):

> We shall not here employ the expression 'theory of science' ['Wissenschaftslehre']; if it is to be used at all, it is more appropriate to the wider domain of questions which, in addition to the logic of science, includes also the empirical investigation of scientific activity, such as historical, sociological, and, above all, psychological inquiries. (Ibid., 279)

Carnap here recognised as perfectly legitimate other second-order inquiries but noted that these, unlike the logic of science, were of an empirical nature. Moreover, Carnap here grouped both of them together under the heading "theory of sci-

ence", which is fully coextensive with what above I called "scientific metatheory" which likewise divides in logical and empirically based inquiries. I conclude that Carnap's conception is fully commensurable with Neurath's as he outlined it in his response to Petzäll.

Historically speaking, of course, it is true to that Carnap, for his part, stuck with his focus on the logic of science. But mine is not just a reconstruction that is logically compatible with the material but was not as such recognised by the historical actors (as may perhaps still be claimed). In a Festschrift for Frank in 1965 he wrote:

> Frank recognized more clearly, I think, than most other philosophers and scientists that it is of greatest importance that those who work in theoretical fields be aware of the role of their work in the wider context of life, of the life of society and culture. Therefore, Frank, both in his own thought and in his teaching activities, paid close attention to the historical development in order to show how currents of thinking are motivated not only by striving for knowledge but to a great extent also by practical and emotional needs and social situations. He showed that this holds for theoretical work just as much as for work in other fields like art and religion. (1965, xi-xii)

Here Carnap recognised the idea of a bipartite metatheory not only as legitimate but also as useful. Elsewhere he endorsed the bipartite nature of metatheory by acknowledging the importance of the empirical aspects of the theory of science and noted that

> unfortunately a division of labor is necessary, and therefore I am compelled to leave the detailed work in this direction to philosophically interested sociologists and sociologically trained philosophers. (1963, 868)

In other words, despite his own concentration on the formalist logic of science, Carnap recognised the legitimacy and usefulness of the pragmatics of science and the incompleteness of a philosophy of science that only concentrates on the former.

6. Conclusion

I argued that in his debate with Petzäll, Neurath formulated a distinction between two types of metatheory that sought to codify not only conditions of peaceful coexistence, but also of fruitful cooperation between the logical and the empirical perspectives on the study of science. This distinction was already "in the air" but in light of recent developments increasingly needed to be made explicit. Though aiming in a different direction by using the Vienna Circle's protocol sentence debate as a mere foil for his discussion, Petzäll's efforts to delineate an approach to the epistemological question that also sought to find room for both logical and

empirical reasoning provided a welcome occasion for Neurath's intervention that, however, has been widely overlooked.

Let me close with a thought on the question to which my paper leads but which clearly goes far beyond it. The exclusive contraction of what we nowadays once again happily call "philosophy of science" to the formal logic of science—however widely understood (i.e. as also comprising semantics)—has been perceived to be under attack ever since Kuhn. We may note that the failings of, in a word, disembodying scientific thought by de-contextualising it both historically and socially are meant to be remedied precisely by widening the inquiries so as to include what here we called the pragmatics of science. Suppose this correction to be successful or, given its ongoing nature, to have hope of succeeding. What we also need to ask, however, is whether this correction goes far enough. What are we to make of the current fashion to rediscover the metaphysics of science? Can this be understood as a kind of Strawsonian exercise in descriptive metaphysics and then be assimilated to the explicatory project that the later Carnap saw himself himself to be engaged in? Or take the discussions about the choice of parameters with regard to which epistemologies establish their instrumental norms, discussions which Neurath allowed for but did not engage in himself. Could such discussions find a place in the conception of philosophy of science as a bipartite metatheory that we find shared by Carnap, Frank and Neurath? But whatever the answer to these questions, that they arise at all so very closely on the heel of revisiting the debate between Neurath and Ake Petzäll may surely serve as one example of the acuity and fruitfulness of the reception of logical empiricism in Scandinavia and Finland.

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