ON THE EMPIRICISM OF LOGICAL EMPIRICISM

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ABSTRACT

This paper warns against misunderstanding the logical empiricists' take on the concepts of experience and empiricism. Far from expressing traditionalist way-of-ideas conceptions, these concepts were themselves rethought and refashioned to accord with their overall aim of making contemporary philosophy of science fit for purpose. To this end, this paper disarms the supposed counter-examples of Schlick's foundationalism and Carnap's Aufbau and exemplifies the aimed for understanding by examples of the physialist theorising of Carnap and Neurath.

KEYWORDS

EMPIRICISM, LOGICAL EMPIRICISM, MORITZ SCHLICK, RUDOLF CARNAP, OTTO NEURATH.

I begin with a warning. My aim here is not to convey what would be news for fellow diggers in dusty archives, but rather to attempt a compact message of the continuing relevance of the type of empiricism represented by logical empiricism.[1]

1.   THE PAST IS A FOREIGN COUNTRY

I must also begin with brief historiographical homily. We all are in some sense familiar with logical empiricism. But with this apparent familiarity comes an often-unsuspected danger of misunderstanding. Importantly, this misunderstanding extends not just of the theoretical positions taken by logical empiricists—we may take this for granted—but to some of the very basic terms they used. Consider just two examples: reduction and physicalism.

There is much *talk of reduction and reducibility* in the writings of the logical empiricists. What must be stressed, first, is that *the intended meaning was epistemological, not ontological*. The point of the reductions the logical empiricists were talking about was not to shore up a recursively constructive logical atomism of facts. Its point lay rather in insisting that assertions that claimed to be scientific possessed demonstrable evidential support. And what must be stressed, second, is that *the intended effect was not meant to be what we nowadays understand as "reductive", i.e. eliminative*. There is, of course, one spectacular early exception to this but, as we'll see, even there the point was not to reach rock-bottom certainty and the exercise was soon abandoned anyway. And as if to prove the point, later Rudolf Carnap gave the ill-chosen name "reduction sentences" to formulations that only aimed to establish conditional evidential bearings on low-level theoretical terms like dispositions and foreswore their eliminative definition. And soon after, of course, his partial interpretation view granted high-level theory further measures of conceptual autonomy.

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[1] A version of this paper was presented at the workshop "Reconsidering Empiricism in the Philosophy of Science", Bristol Centre for Science and Philosophy, March 31 – April 1, 2021, organised by David Copp.

Likewise, *the import of physicalism was metalinguistic, not ontological*. For Carnap in 1932, this meant that all languages were translatable into the physical language. Neurath's conception was slightly more expansive, but he certainly agreed with this. So Viennese physicalism too was not meant to express a materialist metaphysics, even less an eliminative one. Its point lay rather in assuring that the evidence base for scientific knowledge claims is intersubjectively available.

On both accounts then *the focus lay on ensuring epistemic accountability*. Much the same can be said for their notion of the unity of science. The moral I'd urge is that familiar-looking philosophical terminology in historical authors cannot be assumed to agree in meaning with our use of it—even with authors from less than 100 years ago and belonging to the same philosophical tradition as us.

2.   EXPERIENCE IN THE VIENNA CIRCLE

So let's apply this moral right away to the central term in empiricism: how then was "experience" understood in the Vienna Circle? (I'll take the latter to be representative on the points that I wish to explore here for the whole movement, certainly in its European phase.) Before going into specifics, I must say how generally it was *not* understood.

Experience was not understood as naïve empiricism has it, as what Thomas Reid condemned as "sensualism": experience was not just having sensations or sensory impressions, thought was an intrinsic part of it. That this is so, is perhaps most easily accepted for those logical empiricists whose early exposure to suitable doses of Neokantianism is documented: certainly, therefore for Carnap, but also Moritz Schlick.[2] But even the native Austrians, not only the German-born and -educated members, held to Kant's now commonplace view that "thoughts without intuitions are empty, intuitions without concepts are blind". (Philipp Frank's well-known comment [1949: 7] about the recognition of Mach's

2   For the transformation of the concept of experience in Marburg Neokantianism, see RICHARDSON, 2003.

shortcomings on the part of the members of the so-called first Vienna Circle, the pre-World War 1 post-doctoral discussion group which included Hans Hahn, Otto Neurath and himself, should still contrary worries.) The Circle's admittedly noisy rejection of the *synthetic a priori* did not indicate that they wanted to return to a pre-Kantian empiricist state of nature or that they never even left it (as Popper once claimed). Rather, they sought to squarely confront the fact that for science "free creations of the humans mind" were indispensable.

Whether we read this last famous phrase as a quote from Richard Dedekind or Albert Einstein, it is clear that the theorists of the Vienna Circle were not simple concept empiricists. At a minimum, they were not committed to it, but in light of the inspiration Frank (1910) derived from Poincaré's conventionalism and Duhem's conception of theoretical terms and the abundant use made already early on, as in Schlick's pre-Vienna Circle *General Theory of Knowledge* (1918) and still before it, of Hilbert's notion of implicit definitions, I'm inclined to say that they actually *rejected concept empiricism*. That their conception of scientific theories was correctly called a "partial interpretation account" rather indicates obliquely their recognition of the inescapable need of science for concepts grounded only indirectly in experience (see, e.g., CARNAP, 1939).[3] Moreover, their's was a "logical empiricism" not only because they declined to account for logic empirically and allowed it separate standing, but also because their conception of experience included its conceptual and logical transformations. It follows that we'd be hard pressed put to find among the members of the Vienna Circle adherents of the phenomenalist logical positivism that A.J. Ayer promoted to ultimately deleterious effect after his return to England from his visit to Vienna. And it should be, but unfortunately is not, needless to say that we'd be hard put to find among them epistemological foundationalists who wish to show science to be based on secure non-inferentially justified propositions.

"Surely you're joking, Herr Uebel" some of you may want to respond at this point. What about Carnap's *Aufbau* and what about Schlick's "On the Foundations of Knowledge"? Do they not try to ensure "epistemic

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[3]   For a discussion of the difficulties that do threaten the partial interpretation account, see DEMOPOULOUS, 2021, Ch.1.

accountability" precisely by strict reductionism of the type I discount, by nailing knowledge down to secure bases in sense experience? Well, let's see.

3.   ON SCHLICK'S "ON THE FOUNDATIONS OF KNOWLEDGE"

Not only the title of Schlick's paper but also the text, indeed the very first sentence, seems to indicate that its author found nothing wrong with "the wish for absolute certainty of knowledge", the search "for an unshakable foundation, immune from all doubt and forming the firm basis on which the tottering edifice of our knowledge is reared", "for the natural bedrock which is there before building commences, and does not itself sway" (1934/1979, p. 370). According to Schlick, this issue had also been raised in the Vienna Circle itself, but he believed that their debate about so-called protocol-sentences (the evidence statements for empirical theories) had taken the wrong turn. Schlick rejected the universal fallibilism which the "physicalists" Neurath and Carnap had by then come to agree upon, because he rejected their deconstruction of the epistemic autonomy of the individual author or bearer of scientific knowledge.

Schlick instead put forward his conception of "affirmations". He intended them to be statements that were not identical with the protocol statements recordable by scientists in the course of their work. Conceived of as describing phenomenal experience in terms (like "Red here now") that were incorrigible—as long as affirmed sincerely—they were held to constitute the only class of synthetic statements where understanding of their sense coincided with recognition of their truth. This remarkable characteristic of affirmations was explained by them employing (i) indexical expressions that were held to be irreplaceable by coordinate expressions or proper names and (ii) descriptive expressions the use of which was not constrained by prior usage but only the user's intentions at the time.

On analysis it turns out that Schlick's theory is unable to resolve the tension between the contradictory presuppositions that must be made in order, on the one hand, to provide for the subjective certainty that affirmations were meant to

afford our knowledge claims and, on the other hand, to provide for the objective legitimation that scientific knowledge claims require. Typically, interpreters either reject Schlick's affirmation theory wholesale or save only part of it for the price of discarding some other property that affirmations supposedly possessed. What they overlook are numerous hints that Schlick not only rejected his fellow logical empiricists' solution but also their conception of the problem of evidence. Schlick grappled with a still deeper problem—to be sure, it must be said, he did so still unsatisfactorily, but at least he need not stand accused of making schoolboy errors as he otherwise would be. His affirmations were not meant to serve as epistemological foundations for science.

One key to his alternative conception lies in the replacement of the static picture of science as a system of propositions with the dynamic picture of a process of inquiry.

> *Finality* is a very suitable word to describe the significance of [affirmation] statements. They are an absolute end, and in them the current task of knowledge is fulfilled. Science does not rest on them, but leads to them, and they show that it has led aright. They are the absolutely fixed points; we are glad to reach them, even if we cannot rest there. (1934/1979, p. 383, orig. emphasis)

This is an unusual response to the traditional question. Absolute certainty is granted to us, but it has no permanence. Schlick summarized his reorientation as follows:

> If we turn our attention to the connection of science with reality, and see in the system of its propositions what it really is, namely a means of orienting oneself among the facts, of attaining to the joy of confirmation, the feeling of finality, then the problem of the 'foundation' will automatically transform itself into that of the unshakable points of contact between knowledge and reality. (Ibid., p. 386)

This change in *Problemstellung* Schlick also put like this:

> In no sense do they lie at the basis of science, but knowledge, as it were, flickers out to them, reaching each one for a moment only, and at once consuming it. And newly fed and strengthened, it then flares on toward the next. These moments of fulfillment and combustion are of the essence. From

> them comes all the light of knowledge. And it is this light for whose source the philosopher is actually asking, when he seeks the foundation of all knowledge. (Ibid., p. 387)

Schlick was extremely serious about what to many readers looks like purple prose: as he understood them, the use of affirmations revealed a deep point of philosophical anthropology. Note that Schlick was explicit in declaring that affirmations were *not* meant to replace or aid the derivation of protocol statements since they do not belong to the language of science: "They do not occur within science itself, and can neither be derived from scientific propositions, nor the latter from them ..." (1935a/1979, p. 407; cf. 1935b/1979, p. 409). So what was their point?

As I understand him, Schlick's affirmations addressed his long-standing concern with philosophical skepticism which had occupied him already in his habilitation dissertation of 1910 and his book of 1918. What affirmations were to provide was an entirely new answer to scepticism that put to use what Schlick had recently learnt from the *middle* Wittgenstein for whom he was a rare and much appreciated philosophical interlocutor. What Schlick learnt directly from him and some of his manuscripts, like the so-called "Big Typescript", to which he had access, were the momentous changes in perspective that his moving on from the *Tractatus* entailed. Most prominently, of course, those included the shift to the "meaning is use" paradigm and, equally importantly in the present context, that concepts like "understanding" are "not the name of a single process accompanying reading or hearing, but of more or less interrelated processes against a background, or in a context, of facts of a particular kind, viz. the actual use of a learnt language or languages" (as in WITTGENSTEIN, 1969/1974, p. 74). The understanding of a word, for example, manifests itself in the "unthinking" ("unbedenklich" — better: untroubled) use I make of it (ibid., p. 73).

Schlick applied these insights to epistemological matters and saw the unreflective ease of competence displayed in ordinary language use as key to answering the skeptical conundrum. Consider his dismissal of the fallibilist objection that there is no guarantee that a subject understood the proposition expressed in an affirmation correctly.

> I could indeed be the victim of a deception of memory. But just as in the case of an affirmation, such a doubt does *not* give rise to that uncertainty which is characteristic of a hypothesis, for it is not a doubt about the truth of a given proposition, but rather about whether the way I have chosen to present the proposition obeys the symbolic rules that are *otherwise customary*. … Perhaps it is quite untrue that I have always called the colour 'yellow'; if so, there is indeed a deception of memory, but even in this case, the affirmation remains *true* (so long as a lie is not in question). Its truth does not depend on how I have otherwise really employed the words, but only on how I *think* at this moment that I have employed them. But I cannot be mistaken about that […]. (1935b/1979, p. 412)

Here Schlick confronted another difficulty which the diagnosis of the failure of affirmations, as commonly understood, focuses upon. How can a momentary understanding of linguistic expressions which is not bound by their previous usage contribute to the legitimation of knowledge claims for whose formulation the common or agreed upon prior usage is essential? Given his wider horizon and concern with skepticism—ascertaining one's "connection with reality"— Schlick was able to declare the objection to constitute a problem only for an analyst who disregards the "unthinking" use and the presupposed embedding of our statements into the ordinary language we all speak. Schlick, in other words, took himself to have taken steps to dissolve the problem of skepticism by breaking the spell that a wrong conception of knowledge and justification held us under. The "old" problem—"the wish for absolute certainty of knowledge", the search "for an unshakable foundation, immune from all doubt and forming the firm basis on which the tottering edifice of our knowledge is reared"—was now dissolved "grammatically". There was no deeper ground to provide security for affirmations than meeting the condition to employ words according to the grammatical rules that hold for them, nor a longer-lasting foundation than the occasions of their use. If this condition was met, all doubt is idle.

Needless to say, Schlick's solution suffers from the fact that one extremely important component of Wittgenstein's mature views was not yet in place in the years of Schlick's intense interaction with him: the private language argument. But this is not my concern here. My point was simply to show that reading Schlick as providing epistemological foundations for science quite needlessly

convicts him of a philosophical naïvity utterly out of keeping with the critical acumen he displayed as the earliest philosophical defender of Einstein's theories of relativity, for instance. For better or worse, his horizons were far wider.[4]

4.   ON CARNAP'S EMPIRICISM:
FROM THE *AUFBAU* TO "TESTABILITY AND MEANING"

What about Carnap's *Aufbau*? We are all familiar with Quine's characterization of it as the most through attempt ever made to realize Russell's external world programme—to read it as a foundationalist tract par excellence. Thanks to the work of Michael Friedman and Alan Richardson, however, this reading has been shown to be deficient.[5] Whatever similarity to foundationalist edifices the *Aufbau* may display, Carnap's intentions were quite different. I won't retell their story of his Neokantian debt here but only highlight my understanding of why the *Aufbau's* radical reductionism supports the project they identified as Carnap's real concern before turning to Carnap's mature empiricism in "Testability and Meaning".

Apart from demonstrating the unity of empirical science by exhibiting that all of its domains were comprehensible in one language, the avowed aim of the *Aufbau* was demonstrating that "even though the subjective origin of all knowledge lies in the contents of experiences and their connections, it is still possible . . . to advance to an intersubjective, objective world, which can be conceptually comprehended and which is identical for all observers" (CARNAP, 1928, §2). A "rational reconstruction", that is, a simulation of human knowledge under carefully controlled thought-experimental conditions—i.e., the projected recasting of all scientific propositions into a phenomenalist language—was to show that what matters for *objectivity* is not the relation knowledge claims bear to what they are about (though that matters for their truth), but that their *content is expressible in purely structural terms*. Content was "constituted" without any

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[4] For further discussion of this interpretation of Schlick's affirmations, see UEBEL, 2020.
[5] See FRIEDMAN, 1987 and 1992 and RICHARDSON, 1998.

reference to intersubjectively inaccessible manifestations of subjectivity, be that intuition or meanings seemingly reaching out beyond experience itself.

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

But why should one want such radical structuralism-cum-reductionism? Answer: "this transformation is not only possible, it is imperative. For science wants to speak about what is objective, and whatever does not belong to the structure but to the material […] is, in the final analysis, subjective" (ibid.). So reductionism is in the program even without foundationalism. It is required, for Carnap planned to build his account of objectivity on it. (To mark this difference, we can call it "structuralist reductionism" to distinguish it from the "foundationalist reductionism" traditionally ascribed to the *Aufbau*.) Needless to say, this procedure also leaves the *Aufbau* about as far distant from the purely sensualist basis of traditional empiricism as possible. (Logical relations were

constitutive for the sense data that Carnap reconstructed from unanalyzed whole elementary experiences.) But logically constituted or not, it was still the tribunal of experience and not any apodictic synthetic aprioris that Carnap entrusted science to, so we can still claim his book for empiricism.

In fact, for practical purposes of constructing logically perspicuous models of the language of empirical science Carnap soon gave up the very trait that linked the *Aufbau* to classical empiricism (and what it, in turn, shared with Cartesianism): "the way of ideas". Carnap abandoned methodological solipsism for all practical purposes when in "Testability and Meaning" he finally adopted the so-called "thing language" which spoke of observable objects, properties and events as the basic language of science and stated explicitly that what he called the "phenomenological" language "is a purely subjective one, suitable for soliloquy only, while the intersubjective thing-language is suitable for use among different subjects" (1936-37, p. 10). For the reconstruction of the language of unified science, Carnap now stated, phenomenal languages were unsuitable. While it was possible to design so-called reduction sentences that relate expressions of the thing-language to expressions in the phenomenal language, Carnap showed that was impossible to construct the former on the basis of the latter or effect a "retranslation" of the former in terms of the latter (1936-37, p. 464). (Notably, as a *logician* Carnap continued take an interest—as the Preface to the 2nd edition of *Aufbau* shows (1961, p. vii)—in the formal properties and expressive powers of languages taking whole experiences or sense-data as elementary elements and only a few relations between them as basic relations. But this was a far cry from adopting the strategy of methodological solipsism for the epistemological analysis of the languages of unified science and must not be mistaken for it [ibid., p. viii].) Carnap's rejection of methodological solipsism for practical purposes continued to stand firm in later years (cf. 1963, p. 869; p. 944-945).

Clearly, joining Neurath's physicalism—who had been agitating against methodological solipsism ever since the *Aufbau*—did not render Carnap less empiricist. But just how then did Carnap understand "empiricism" and "empiricist"? To begin with, he understood it "in their widest sense, and not in

the narrower sense of traditional positivism or sensationalism or any other doctrine restricting empirical knowledge to a certain kind of experience" (1936-37, p. 2n). This, of course, was not new then and we have already taken account of it. What was new here was that he regarded empiricism not as a truth-valuable doctrine but a proposal to employ a certain kind of language in scientific discourse:

> It seems to me that it is preferable to formulate the principle of empiricism not in the form of an assertion — 'all knowledge is empirical' or 'all synthetic sentences that we can know are based on (or connected with) experiences' or the like — but rather in the form of a proposal or requirement. As empiricists, we require the language of science to be restricted in a certain way; we require that descriptive predicates and hence synthetic sentences are not to be admitted unless they have some connection with possible observations, a connection that has to be characterized in a suitable way." (Ibid., p. 33)

After considering requirements of different strength, Carnap opted for the "*requirement of confirmability*: Every synthetic sentence must be confirmable" (Ibid., p. 34). This was the "most liberal" of the requirements Carnap considered.

This criterion not only abjured the old pretense of verification (definitive and final establishment of truth), but also that of ready testability. "We call a sentence *testable* if we know a method of testing for it; and we call it *confirmable* if we know under what conditions the sentence would be confirmed." Notably then, "a sentence may be confirmable without being testable" (ibid., p. 420)., for we may not know "how to set up this or that observation" (ibid., p. 421). Confirmability, for Carnap, meant the conceivability of testing, given the laws of nature as we understand them. Nevertheless, Carnap held that the requirement of confirmability "suffices to exclude all sentences of a non-empirical nature, e.g. those of transcendental metaphysics inasmuch as they are not confirmable, not even incompletely" (ibid., p. 35).

We may doubt, of course, whether the notorious problem of determining a foolproof criterion of empirical significance was solved here, but this is not our issue. (Nor is that, by contrast, Schlick's logical conceivability of testing

unwittingly gave up on empiricism.)[6]   What is, rather, is that for Carnap *empiricism is not a theoretical proposition but a practical methodological stance* very much in the spirit of his "principle of logical tolerance" so named in 1934 but practiced since late 1932: "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" (1934a/1937, §17).   With tolerance the search for the true nature of science's grounding in reality for the true protocol language, falls away, as does the choice for a true logic. The practical methodological stance is the choice of language for scientific discourse for which the philosopher makes "proposals". Such a choice answers, in Carnapian terms, to an external question, not an internal one. It is decided purely on grounds of utility.

Van Fraasen's observation is apt here: "If empiricism is a stance, its critique of metaphysics will be based at least in part on something other than justified theses: attitudes, commitments, values, goals." (2002, p. 48) So the question is: utility for what? Carnap's answer is clear: cognitive transparency and epistemic accountability. And if asked what's so good about these, he'd answer that they are an integral part of his credo of scientific humanism. Empiricism then, for a logical empiricist of a Carnapian persuasion, is not a truth-valuable doctrine but an attitude adopted ultimately down to a political choice. (There can be no accountability for philosophical *Führer* who decree that "nothing noths".) But this motivation does not reduce empiricism to a mere predilection: it imposes a framework—admittedly one amongst others—which makes evidence-based discourse possible. And for the adoption of such a framework there are lots of good reasons. In this respect Carnap's empiricism represents as global a concern with the conditions of knowledge as traditional epistemologies afford, albeit from a radically different angle: it challenges traditional spectator theories of knowledge.

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[6] For a discussion of the varieties of verificationism in the Vienna Circle, see UEBEL, 2019a.

The question arises, of course, whether it is up to the job. Can empiricism comprehend scientific rationality to help in the realization of the aims of scientific humanism? It is at this point that we must return to the issue of reductionism. It might be thought that by switching from methodological solipsism to physicalism Carnap only switched one reductive program for another, namely to logical behaviorism. Now it turns out that this is not so for Carnap or Neurath. For them, dispositions to behavior were never more than fallible indicators of mental states, established on inductive grounds: to consider them logical behaviorists is to misunderstand their reasoning about human agents very badly.[7] And dispositions themselves, in any case, Carnap soon enough learnt, were not reducible to observational predicates—thus his very misleadingly so-called "reduction sentences". They established evidential relations to be satisfied when attributing dispositional states but remaining experimentally dependent they did not eliminate them (1936-37). Likewise, soon after Carnap recognized the irreducibility of theoretical terms altogether—only some select nodes in the network of the implicitly defined terms of high-level theories can be linked to observation by complex chains of reduction sentences (1939). (Thereby belief ascription became a part of proper psychological theorizing.) And abandoning reliance on the misnamed reduction chains altogether, some twenty years later Carnap employed Ramseyfications to capture theoretical terms in an empirical matrix, as it were, again without reduction (1958, 1963, p. 961-966). I won't claim here that Carnap succeeded in everything he sought to achieve, but if he failed it was not for reasons of undue reductionism. His overall metaphilosophical programme of conceptual engineering—he called it "explication"—was, if anything, constructivist.

Now it might be asked whether Carnap's empiricism is still "true" empiricism, but such terminological essentialism is just silly. It is true, of course, that his abstraction from any psychological concerns and concentration on the language of science alone greatly distances him from traditional empiricism in that it is no longer doxastic but at best propositional justification that he could

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[7]   Building on the analysis of Carnap in the 1930s in CRAWFORD, 2013, this is argued for concerning Neurath in UEBEL, 2019b and all stages of Carnap in UEBEL, 2021.

offer to investigate. Since it is the propositions of not only formal but also empirical science that Carnap is concerned with, it makes no sense, however, to deny him the label "empiricist"—irrespective of the undeniable difference of his philosophy from that tradition. What his exclusive focus on propositional justification points to rather, if it points to anything, is that his "logic of science" requires complementation from a "pragmatics of science" to satisfy some of the more traditional empiricist aims of offering ameliorative advice to the practitioners of science. (I'll return to this below.)

More pressing would be the question whether Carnap's acceptance—alongside Reichenbach's and Cassirer's—of the non-apodictic relative apriori does not compromise his empiricist credentials. This relative a priori concerns those elements of mathematical physics that ensure the applicability of the theory to empirical reality, elements that have to be antecedently in place for the theory to have empirical content at all. (As testing presupposes them to be in place, they cannot be tested independently.) Significantly, these a priori elements concern not only the mathematics involved but also a distinguished class of physical principles, which, like the mathematics presupposed, differ between different theories, e.g. Newtonian physics, the special theory of relativity and the general theory of relativity. There is debate about whether the get-out already suggested by Schlick in his debate with Reichenbach about this matter—namely to consider these relative apriori determinations to be conventions that we adopt and suspend as required—can be made to work. However, since it has been shown to be possible that, as Friedman put it (2001, p. 92), what "is characterized as a non-empirical constitutive principle, entirely beyond the reach of standard empirical testing at one stage of scientific progress, can be subject to precisely such testing at a later stage", the prospect opens up that, over time, all relative aprioris can be "empiricised", as it were, admittedly at the cost of incurring new relative aprioris. A liberal understanding of empiricism should, I think, be able to live with this.

In sum: Carnap's consistently practiced anti-foundationalism (we disregard one brief recidivist episode).[8] Empiricism for him was a stance, the choice of scientific language that was constrained by considerations of cognitive utility and epistemic accountability. It also was an exercise in philosophy—but not understood as tearing apart the veil of mere appearance by closest possible attention to what is really given (instead of speculation or intuition), but understood, as noted, as conceptual engineering.

5.   ON NEURATH'S EMPRICISM: SCIENTIFIC TESTIMONY PROTOCOL SENTENCES

I hinted at the Neokantian background to Carnap's *Aufbau* and his subsequent debate with Neurath about the content, form and status of scientific evidence statements, a debate in the course of which Carnap gradually abandoned methodological solipsism as a going concern for the language of science. It might be wondered whether the concern with objectivity was likewise abandoned, given that the specific problematic of the *Aufbau* had vanished. The answer is, as far as I can see, that this concern was by no means abandoned, but merely changed focus. As Carnap stressed in the 1930s, it was the supreme advantage of the physicalist language that it was intersubjective—that it afforded real intersubjectivity and not merely simulated it—and so rendered science possible. What this very strongly suggests is that *Carnap's interest in what allows for objectivity had not changed at all*, but that he no longer located the possibility of objectivity in the logical structure of the contents of individual consciousnesses as represented in the genealogical tree of concepts of the *Aufbau,* i.e. in a "constitutional" structure that was simply presumed to be shared across the species. Instead, *Carnap now located the possibility of objectivity in the logical structures of public language*, at first only in the structure of the language of mathematical physics but soon also in that of the everyday "thing languages"

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[8] For documentation and discussion see UEBEL, 2007, Ch. 6.

speaking of observable things, properties, relations, and events, languages that people as a matter of fact did share already.

Note that this also suggests a negative answer to the question whether concern with the possibility of objectivity is necessarily of a Kantian nature. To be sure, it seems to have been Kant who first asked that question, but unless we seek to answer this question in a transcendental fashion, merely worrying about objectivity and what makes it possible does not make us Kantian. I'm pressing the negative answer here not so much because of concern with Carnap becoming a proper empiricist post-*Aufbau*, but with Neurath. Of all the logical empiricists he is perhaps the most unlikely candidate for a Kantian fellowship. Yet it was Neurath who wrote at one of the heights of his debate with Carnap: "The possibility of science becomes apparent in science itself" (1932a/1983, p. 61). Long and correctly, I think, read as an avowal of epistemological naturalism, this statement of Neurath's is equally well understood as an answer to the question about what makes the objectivity of science possible—for naturalism is such an answer.

Whether asking about the conditions of the possibility of scientific knowledge makes us Kantian in a relevant sense depends on how we go about answering it. It does so if we were to go about answering it in a resolutely a priori fashion. Now it might be thought that since we are asking after the conditions of a possibility of *x* we are asking after what is necessary for *x* and therefore must engage in an a priori inquiry. But that is so only if it is logical conditions of possibility that we are inquiring into. Yet science is more than its abstract propositional product: there is also its physically embodied procedural dimension. Of course, Carnap was always careful to distinguish the domain of his "logic of science" from the domains of the psychology, sociology and history (and presumably economics) of science, but this does not mean that he dismissed these empirical studies of science as irrelevant. Occasionally he even briefly ventured into them himself—and noted this—as when in "Testability and Meaning" he outlined his intended meaning of "observable" (1936/37, p. 454). This indicates nicely that even the logic of science was unable to remain wholly aloof from what Neurath called the "behavioristics of scholars" which was where

his expertise lay. In effect, Carnap's logic of science depended on its complementation by what Frank more felicitously called the "pragmatics of knowledge" (1957, p. 360)[9].

Recall then Neurath's "The possibility of science becomes apparent in science itself": as far as he was concerned, the conditions of the possibility of objectivity—or better: evidence-guided intersubjectivity—lay in the procedures of scientific reasoning. By this he did not mean its abstract logic but the concrete do's and don't's—and those not even of such highfalutin matters as wholesale theory acceptance but of such seemingly modest dimensions as acceptance of observational testimony. As for Carnap, for Neurath empiricism demanded that for any scientifically acceptable sentence we must be able to establish its logical relation, however indirect as it may be, to in principle observable evidence, i.e. to protocol sentences. When Neurath explored what secures the objectivity of science, it was not just the mere intersubjective intelligibility of these protocol sentences that he considered—a necessary condition to be sure—but even more so the layered and interlocking conditions on their acceptance by scientists into scientific discourse as established observational facts.

Carnap and Neurath can be seen to have observed a division of labour: the former pursued the logic and the latter the pragmatics of science. Carnap's logic of science investigated the internal structure of theories and their relation to their evidential base in purely logical terms. Neurath's pragmatics of science investigated scientific *practice* by means of the empirical sciences of science, the psychology and sociology as well as the history of science. So while the logic of science investigated abstract relations of evidential support, the pragmatics of science investigated concrete theory choice and change. Most concretely, in its concern with protocol sentences, the pragmatics of science investigated the empirical basis of science neither as phenomenal mental states, nor as a distinguished class of sentences of specified semi-formal language systems (theories), but as acts of giving and receiving observational testimony. This, in essence, is what Neurath's theory of protocol sentences, encapsulated in his proposal for how to understand them, is all about.

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[9] For discussion of the resultant bipartite metatheory conception of philosophy, see UEBEL, 2015.

To be sure, his proposal was made exceedingly cryptically: "A complete protocol sentence might for example be worded like this: 'Otto's protocol at 3:17 o'clock: [Otto's speech-thinking at 3:16 o'clock was: (at 3:15 o'clock there was a table in the room perceived by Otto)]'." (1932b/1983, p. 93). Let's call this "Schema PS". Many of his readers were puzzled by it and the most absurd allegations were levelled at its author. Typically, it was overlooked that Neurath went on:

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

Neurath's protocol sentences in the form given by schema PS are concerned with a certain type of public statement: recordings of empirical evidence. They represent prototypical forms of scientific testimony, or better: they seek to spell out the condition under which observational testimony is elevated to the status of scientific datum.

Numerous things are to be noted here to prevent misunderstandings. To begin with, schema PS spells out the *acceptance conditions* for *3rd-person knowledge claims*—*not* the truth conditions of 1st-person knowledge claims. These conditions are represented by the various clauses of schema PS some of which are represented as embedded parentheses ("brackets"). Disembedding them will make clear their contents. So, the intention behind the strange structure of schema PS was to render as explicit as possible the acceptance conditions of observational testimony. To bring this out most clearly it is advisable to use a variant of schema PS, PS*, that Neurath used in correspondence and once in a lecture.[10] It renders protocol statements as four-part affairs which feature an unadorned observation statement as its innermost embedded clause. The intention then was to make transparent the interplay of different conditions the

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[10] For source details and further discussion, see UEBEL, 2007, Ch. 11 and UEBEL, 2009 and BENTLEY, 2021, Ch. 3.

acceptance of observational testimony is subject to by iterated embedding of a singular observation statement.

Accordingly, schema PS* reads:

protocol (thought [stimulation state {observable fact}])

The decomposition or analysis ("deletion of brackets") reveals the following four clauses:

(i)   protocol (thought [stimulation state {observable fact}])

(ii)   thought [stimulation state {observable fact}]

(iii)   stimulation state {observable fact}

(iv)   observable fact

Each of (i)-(iv) expresses a different condition testimony must meet in order to be accepted. Schema PS* thus gives us the following complex acceptance condition observational testimony: (i) & (ii) & (iii) & (iv).

Note that while each of the clauses (i), (ii) and (iii) are non-truth-functional on their own due to the opacity of their embedded clauses, the total complex acceptance condition allows treatment by truth-tables for conjunction. Observational testimony is acceptable when all four of the conditions specified are satisfied. "Acceptable" here means that the observation reported is legitimately treated as a *bona fide* empirical datum.

Ever so briefly, these are the conditions that are specified by Neurath's proposal in the form of schema PS*. Condition (i) — *protocol (thought [stimulation state {observable fact}])* — expresses what can be called the "institutional condition". It represents a framework-fixing performative utterance that reflects the public nature of scientific enterprise. It is essential to a protocol statement that it be made with communicative intent. Condition (ii) — *thought [stimulation state {observable fact}]* — expresses what can be called the "intentional condition". Its point is to ensure that the report possesses authenticity insofar as its wording is consonant with the testifier's categories. Condition (iii) — *stimulation state {observable fact}* — expresses what can be called the "causal condition". It states

that causal interaction with the sense organs of the testifier has taken place and indicates the sense modality of putative observational input. Condition (iv)—*observable fact*—expresses what can be called the "negative defeasibility condition". Its function is *not* to establish the content of the innermost clause as indeed factive (that would make the account circular), but instead, in good Lockean fashion, to indicate that we have no evidence to the contrary. Further conditions on the acceptance of observational testimony (e.g., sincerity) can expressed according to schema PS* by the combination of conditions (e.g., [ii]&[iii]).

I hope that even this compressed form of reporting on my reconstruction of Neurath's proposal renders plain the promising method that it lays out. It specifies as the (antifoundationalist) "foundation" of science a particular set of complex interactions between practicing scientists. Some readers may wonder how the evidently normative intent of Neurath's proposal coheres with his professed naturalism: clearly, schema PS* is not just descriptive but proposes standards of scientific legitimacy. The answer is simply that for naturalists epistemic norms are not absolute but conditional, conceived of instrumentally. As with Carnap's stance empiricism, the "proof" of such norms lies not in some mythical command but the practical utility that following these standards promises.

Needless to say, perhaps, the general methodology of observation testimony acceptance is to be regarded as revisable proposal. As Neurath added to his last employment of the simile of sailors repairing their on the open sea (which Quine made famous): "The whole business will go on in a way we cannot even anticipate today." (1944, p. 47). This fallibilist attitude also, of course, rules over any particular judgement passed under the aegis of this proposal. Even in its elevated status as accepted datum, the observation sentence remains a provisional datum and is liable to be revised, i.e. to have its accepted status revoked, depending on what is learnt further about the satisfaction of its acceptance conditions. I should also add that having attained the status of accepted datum does not yet fix its role in further scientific theorising. As long as they remain accepted, scientist can legitimately appeal to these data as positive

evidence, of course, but whether these data can serve as disconfirming counter-evidence is potentially open to further debate. One of the points Neurath made in criticism of Popper's early naïve falsificationism was precisely that a seemingly falsifying instance is only treated as such under certain conditions that do not always obtain (1935/1983, p. 124). Sometimes we let the recalcitrant datum stand but treat is as what many years later Kuhn called a scientific "anomaly": we simply defer drawing consequences from its acceptance for our other accepted theories until further evidence becomes available.

Much more would need to be said to convince you of its wisdom, perhaps, but already it should be apparent that Neurath's theory sketch combines (i) normative determinations of the acceptance conditions for observational testimony with (ii) a descriptive apparatus taking account of the neurophysiological and psychological conditions of individual perception and the interpersonal and institutional social conditions of scientific cooperation. The multi-disciplinary investigations to which further pursuit of Neurath's proposal would lead were not undertaken in his day or for a long time since, and they may easily be taken to reflect only a peculiarly "positivistic" mind-set. It may be noted, however, that under different headings and with different terminologies, broadly germane investigations are underway nowadays in science studies and philosophy, the keyword "meta-data" denoting contextual information about, among other things, the origin and production of data.[11] Thus Sabina Leonelli has repeatedly dawn attention to the importance of meta-data for ongoing research in biology, and their significance for general epistemology of science by contributing to a concept of "enriched evidence" was recently stressed by Nora Boyd.[12]

Like Carnap's explicationist stance empiricism, we may conclude, Neurath's outline of a theory of protocol sentences is about as far from the "legend" of simplistic and outdated logical empiricism as it is possible to get.

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[11] I wish to thank Hugo Rimeur (Archives Henri-Poincaré) for drawing my attention to this work.
[12] See, e.g., LEONELLI, 2009; 2019 and BOYD, 2018.

6. REFERENCES

AYER, A. J. (ed.) *Logical Positivism*, New York: Free Press, 1959.

BENTLEY, J. Logical Empiricism and Naturalism: Neurath and Carnap's Meta-Theory of Science. PhD dissertation, University of Manchester, 2021.

BOYD, N. M. "Enriched Evidence." *Philosophy of Science* 85:2018, p. 403–421.

CARNAP, R. *Der logische Aufbau der Welt*, Berlin: Weltkreis-Verlag, 1928. Trans. *The Logical Structure of the World*, Berkeley: University of California Press, 1967, repr. Chicago: Open Court, 2003.

CARNAP, R. *Logische Syntax der Sprache*, Vienna: Springer, 1934. Rev. ed. trans. *The Logical Syntax of Language*, London: Kegan Paul, Trench, Trubner & Cie, 1937, repr. Chicago: Open Court, 2002.

CARNAP, R. "Testability and Meaning". *Philosophy of Science* 3: 1936-37, p. 419-471; 4: p. 1-40.

CARNAP, R. *Foundations of Logic and Mathematics*. Chicago: University of Chicago Press, 1939.

CARNAP, R. *Logical Foundations of Probability*. Chicago: University of Chicago Press, 1950.

CARNAP. R. "Beobachtungssprache und theoretische Sprache." *Dialectica* 12: 1958, p. 236–248. Trans. "Observational Language and Theoretical Language" in HINTIKKA, J. (ed.) *Rudolf Carnap, Logical Empiricist*, Dordrecht: Reidel, 1975, p. 75-85.

CARNAP, R. "Vorwort zur zweiten Auflage", *Der logische Aufbau der Welt*. Hamburg: Meiner, 1961. Transl. "Preface to 2nd edition" in trans. of CARNAP, 1928, p. v-xi.

CARNAP, R. "Intellectual Autobiography" and "Comments and Replies", 1963a, in SCHILPP, P. A. (ed.) *The Philosophy of Rudolf Carnap*, LaSalle, Ill.: Open Court, 1963, p. 3-84; p. 859-1016.

CRAWFORD, S. "The Myth of Logical Behaviorism and the Origins of the Identity Theory" in M. BEANEY, M. (ed.) *The Oxford Handbook of the History of Analytic Philosophy*, Oxford: Oxford University Press, 2013, p. 621–55.

DEMOPOULOS, W. *On Theories.* Cambridge, Mass.: Harvard University Press, 2021.

FRANK, P. 1910. "A. Rey, *Die Theorie der Physik bei den modernen Physikern*, 1909." *Monatshefte für Mathematik und Physik 21:* 1910*, Literaturberichte*, p. 43-45.

FRANK, P. "Historical Introduction." In FRANK, P. *Modern Science and its Philosophy*, Cambridge, Mass: Harvard University Press, 1949, p. 1-51.

FRANK, P. *Philosophy of Science. The Link Between Science and Philosophy*. Englewood Cliffs, NJ: Prentice-Hall, 1957. Repr. Minola: Dover, 2004.

FRIEDMAN, M. "Carnap's *Aufbau* Reconsidered." *Nous* 21: 1987, p. 521-45. Repr. in FRIEDMAN, 1999, p. 89-113.

FRIEDMAN, M. "Epistemology in the *Aufbau*." Synthese 93: 1992, p. 15-57. Repr. with postscript in FRIEDMAN, 1999, p. 114-164.

FRIEDMAN, M. *Reconsidering Logical Positivism*, Cambridge: Cambridge University Press, 1999.

FRIEDMAN, M. *Dynamics of Reason*. Stanford: CSLI Publications, 2001.

LEONELLI, S. "On the Locality of Data and Claims about Phenomena." *Philosophy of Science* 76: 2009, p. 737–749.

LEONELLI, S. "What Distinguishes Data from Models?", *European Journal for Philosophy of Science* 9: 2019, Article 22

NEURATH, O. 1932a. "Soziologie im Physikalismus." *Erkenntnis* 2: 1932a, p. 393-431. Trans. "Sociology and Physicalism" in AYER, 1959, p. 282-320, and "Sociology in the Framework of Physicalism" in NEURATH, 1983, p. 58-90.

NEURATH, O. "Protokollsätze." *Erkenntnis* 3: 1932b, p. 204–14. Trans . "Protocol Sentences" in AYER, 1959, p. 199-208, and "Protocol Statements," in NEURATH, 1983, p. 91–9.

NEURATH, O. "Pseudorationalismus der Falsifikation." *Erkenntnis* 5: 1935, p. 353-65. Trans. "Pseudorationalism of Falsification" in NEURATH, 1983, p. 121-31.

NEURATH, O. *Foundations of the Social Sciences*. Chicago: University of Chicago Press, 1994.

NEURATH, O. *Philosophical Papers 1913-1946* (ed. by R.S. Cohen and M. Neurath). Dordrecht: Reidel, 1983.

RICHARDSON, A. *Carnap's Construction of the World*. Cambridge: Cambridge University Press, 1998.

RICHARDSON, A. "Conceiving, Experiencing, and Conceiving Experiencing: Neo-Kantianism and the History of the Concept of Experience." *Topoi* 22, 2003, p. 55-67.

SCHLICK, M. *Allgemeine Erkenntnislehre*, Berlin: Springer, 1918, 2nd rev. ed. 1925. Transl. *General Theory of Knowledge*, Vienna: Springer, 1974, repr. Lasalle: Open Court, 1985.

SCHLICK, M. "Über das Fundament der Erkenntnis." *Erkenntnis* 4, 1934, p. 79-99. Transl. "The Foundation of Knowledge" in AYER, 1959, p. 209-27, and in SCHLICK, 1979, p. 370-87.

SCHLICK, M. "Facts and Propositions." *Analysis* 2, 1935a, p. 65-70. Repr. in SCHLICK, 1979, p. 400-4.

SCHLICK, M. "Introduction." In SCHLICK, 1935d, trans. "Introduction" in SCHLICK, 1979, 1935b, p. 405-7.

SCHLICK, M. "Sur les 'Constatations'." In Schlick 1935d, trans. "On 'Affirmations'" in Schlick 1979, p. 407-13, 1935c.

SCHLICK, M. *Sur le Fondament du Conaissance*, Paris: Heinman & Cie, 1935d.

UEBEL, T. *Empiricism at the Crossroads. The Vienna Circle's Protocol-Sentence Debate*. Chicago: Open Court, 2007.

UEBEL, T. 2009. "Neurath's Protocol Statements Revisited: Sketch of a Theory of Scientific Testimony", *Studies in History and Philosophy of Science* 40: 4–13.

UEBEL, T. "Three Challenges to the Complementarity of the Logic and the Pragmatics of Science", *Studies in History and Philosophy of Science* 53, 2015, p. 23–32.

UEBEL, T. "Verificationism and (Some of) its Discontents." *Journal for the History of Analytical Philosophy* 7.4, 201a9, p. 1-37.

UEBEL, T. "Neurath on Verstehen", *European Journal of Philosophy*, 2019bp. 2791-2938.

UEBEL, T. 2020. "Schlick and Wittgenstein: The Theory of Affirmations Revisited." *Journal of the History of Philosophy* 58, 2020, p. 141-166.

UEBEL, T. "Carnap, Knowledge of Other Minds, and Physicalism", *Philosopher's Imprint, 2021 (forthcoming).*

VAN FRAASEN, B. *The Empirical Stance*, New Haven: Yale University Press, 2002.

WITTGENSTEIN, L. *Philosophische Grammatik*, Frankfurt: Suhrkamp, 1969. Trans. *Philosophical Grammar* (ed. by R. Rhees), Oxford: Basil Blackwell, 1974.