

Subjective, Intersubjective, Objective: Philosophical Essays Volume 3 Donald Davidson

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Empirical Content

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Abstract and Keywords

Explores Schlick's and Neurath's dispute over the foundations of empirical knowledge, and thereby equips 'A Coherence Theory of Truth and Knowledge' with commentary and historical background. Davidson works out the difficulties involved in drawing epistemological conclusions from a verificationist theory of meaning and argues that there are promising hints of a better theory, akin to Davidson's coherence theory of truth and knowledge, in the logical positivists' writings.

Keywords: coherence, empirical content, epistemology, foundations of empirical knowledge, knowledge, logical positivism, Neurath, Schlick, truth, verificationist theory of meaning

The dispute between Schlick and Neurath over the foundations of empirical knowledge illustrates the difficulties in trying to draw epistemological conclusions from a verificationist theory of meaning. It also shows how assuming the general correctness of science does not automatically avoid, or provide an easy answer to, skepticism. But while neither Schlick nor Neurath arrived at a satisfactory account of empirical knowledge, there are promising hints of a better theory in their writings. Following up these hints, and drawing on further ideas in Hempel, Carnap, and particularly Quine, I suggest the direction I think a naturalistic epistemology should take.

The logical positivists agreed that the empirical content of an interpreted sentence derives from its relations to a subset of sentences that report,

or are based on, observation or experience. Two main sources of difficulty and dispute immediately became evident. One was the question how to characterize the relations between protocol sentences and other sentences. The history of the developments and changes in the views of the logical positivists and their followers on this problem has been masterfully recorded, as well as much contributed to, by Carl Hempel.¹ This is not my present subject.

The second question was how protocol sentences should be formulated, and what their relation to experience or observation is. This is the issue I wish to discuss, and on which Schlick and Neurath disagreed, Schlick endorsing a foundationalist epistemology and Neurath a coherence theory. The difference was expressed in fairly strong terms. Neurath described the foundationalist position as **(p. 160)** 'related to the belief in *immediate experiences* which is current in traditional academic philosophy', and remarked that 'methodological solipsism' (Carnap's term for a view like Schlick's) does 'not become more serviceable because of the addition of the word "methodological"'.² Schlick in turn called Neurath's version of the coherence theory an 'astounding error'.³

Astounding error or not, Carnap and Hempel at one time seemed to agree with Neurath. In 1935 Hempel wrote, 'I think that there is no essential difference left between protocol statements and other statements,' and he concurred with Carnap in holding that once the question which sentences were basic was put in the 'formal mode', the answer became a matter for convention to decide. 'This insight', he concluded, echoing Carnap's words, 'eliminates from the Logical Positivist's theory of verification and truth a remainder of absolutism which is due to metaphysical tendencies'.⁴ Schlick jeered at such conventionalism, saying it made truth as relative as 'all the measuring rods of physics'. In an ironic vein he added, 'and it is this view with its consequences that has been commended as banishing the last remnant of "absolutism" from philosophy'.⁵

It is not entirely clear, however, just where matters stood. Fifteen years later, in a quasi-historical article, Hempel asserted that 'The fundamental tenet of modern empiricism is the view that all non-analytic knowledge is based on experience.' He went on to explain what it means for knowledge to be based on experience: nonanalytic knowledge can be expressed by sentences that are confirmed (in a specified way) by observation sentences, which in turn are 'ascertained' to be true by direct observation.⁶ This sounds more

like Schlick, and indeed like the 'fatal confrontation of statements and facts' which Hempel had previously rejected.⁷

In a note added to a reprinting of 'Studies in the Logic of Confirmation'⁸ Hempel suggests a way of partially reconciling the apparently opposed points of view. Truth, he and Carnap had come to (p. 161) realize in the light of Tarski's work, is a legitimate semantic notion, and should not be treated as a matter of coherence. Confirmation, on the other hand, was of two sorts, *relative* and *absolute*. The logical study of confirmation was the study of the extent to which an arbitrary set of sentences confirmed a hypothesis. In this context, one could only say that relative to a set of sentences (whatever its provenance), a hypothesis was confirmed or disconfirmed. Absolute confirmation, on the other hand, depended on a 'pragmatic' decision to treat certain sentences as true. Neurath and Carnap, Hempel suggests, were thinking of relative confirmation, which invites a coherence theory. Schlick, and Hempel in 1950, were thinking of absolute confirmation.

This important distinction of Hempel's is revealing, since it does seem at times that early discussions of protocol sentences vacillated between treating such sentences as any sentences with a specified syntax, and treating them as sentences that were accepted, perhaps on the basis of observation or experience. But the distinction cannot reconcile all the differences. The differences that remained were these: Schlick held, while Neurath denied, that protocol sentences may be established as true once and for all; Schlick claimed, while Neurath denied, that a sentence could intelligibly be said to be compared to reality. There were also differences over the question of the proper subject matter of protocols, and the question whether they reported something private or something public. Obviously, these various points are closely related to one another.

One way to approach our central problem is to ask what the nature of *evidence* is: does it consist of objects, events, facts, experiences, sensations, beliefs, propositions, or sentences? Almost every one of these possible answers can be found in the writings of the Vienna Circle.

I observe two pieces of green paper [writes Schlick] and determine that they have the same color. The proposition which asserts the sameness of color is verified, among other ways, by the fact that at the same time I have two experiences of the same color. The proposition: 'there are two spots of the same color before me now' cannot be reduced to others; it is verified by the fact that it describes the given.⁹

What exactly does the verifying? Schlick says it is the fact that he has certain experiences that are veridical. But if the same experiences were not veridical, would they still verify the same proposition? The (p. 162) fact that I see a piece of paper implies that there is a piece of paper, but here we do not move from evidence to hypothesis in an interesting way; we merely deduce an entailed proposition. Elsewhere, Schlick insists that we must start with statements that 'have their origin' in observation sentences; and he elaborates this as: 'they derive, as one may confidently say in the traditional way of speaking, "from experience" '.¹⁰ In the same essay he declares that 'It is clear, and is so far as I know disputed by no one, that knowledge in life and science in *some* sense *begins* with confirmation of facts, and that the "protocol statements" in which this occurs stand in the same sense at the *beginning* of science.'¹¹ In another passage he says that everything goes back to what is 'immediately observed'.¹² As Ayer put it, some propositions can be 'directly confronted with the facts'.

Let me try to bring out in one further way the apparently puzzling question of the ontological status of evidence. We say that laws are confirmed by their positive instances; so the positive instances are, presumably, evidence for the laws they confirm. Suppose, for the sake of clarity in one direction, that we identify laws with universally quantified conditional sentences—sentences which are, needless to say, interpreted. What is a positive instance? Let the law have the form ' $(x)(Fx \rightarrow Gx)$ '. Then, Hempel suggests, it is reasonable to suppose that an *object* that is F and G confirms the law.¹³ (Goodman often talks this way in *Fact, Fiction, and Forecast*.) Hempel then explains that instead of viewing confirmation as a 'relation between an object or an ordered set of objects, representing the evidence, and a sentence, representing the hypothesis', he will take it to be a relation between a sentence that describes the evidence, and the hypothesis. Thus, 'The evidence adduced in support or criticism of a scientific hypothesis is always expressed in sentences, which frequently have the character of observation reports . . . The evidence . . . consists, in the last analysis, in data accessible to what is loosely called direct observation and such data are expressible in the form of "observation reports" '.¹⁴ What are accessible to observation are objects and events. These are not the same things as facts nor, of course, as sentences. Sentences can, in some loose sense, express facts (i.e. true propositions), and describe objects. None of what I say (p. 163) is criticism of Hempel's exemplary work on confirmation as a relation between sentences; I am using the distinction among various ways of describing positive instances of laws (or lawlike sentences) in order to emphasize the very different ways in which it is natural to talk of evidence.

Perhaps it is not strange to call a black raven—some actual bird—an *instance* of a law, but it does seem odd to say the bird is *evidence* for the law. At best this seems to be shorthand for saying it is the *fact* that this bird is a black raven that constitutes the evidence; or we could speak of the truth of the proposition, or of some appropriate sentence. So far, however, we have not touched on the epistemological issue, the question what it means for *someone* to have a reason to accept the law, to *possess* evidence. Neither the existence of the black raven nor the truth of the proposition or sentence that says there is a black raven in itself gives anyone a reason to believe there is a black raven, much less a reason to believe all ravens are black. For someone to have a reason to believe all ravens are black, it is necessary for him to *believe*, for example, that here is a black raven.

We are off on a well-worn track. Surely it is not enough simply to believe that here is a black raven; not enough, either, that the belief should also be true. For both of these conditions together do not add up to evidence unless the person has an adequate reason for holding the belief. If the reason must be another belief, we are faced by an infinite regress or a circle. A regress would make knowledge impossible, while a circle would lead to the difficulties of a pure coherence theory of knowledge. I'll come back to the latter in a moment.

At this point we come to the various attempts to find states of mind that bridge or eliminate the gap between sensation, where no question of truth can arise, and judgement, which is plausibly a source of evidence. Quasi-sentences like 'Black here now' have been proposed as expressing such states of mind. And perhaps we will be persuaded that there are such states of mind if we overlook the fact that the verb has been omitted (since putting it in would push things too far in the direction of judgement) and that words like 'here' and 'now' cannot be understood except as involving a reference to an agent. In any case, the attempt to base science on such states of mind is doomed, since no one has ever succeeded in showing how to base knowledge of an objective, common world on such 'evidence'. Even Schlick, who somehow hoped to back protocol sentences of the form 'A experiences black at time *t*' by whatever it is that is expressed by **(p. 164)** 'Black here now' as said or thought by A at *t*, did not believe in a construction of science or a public world, with the construction based on 'immediate experiences'.

It should be obvious that no appeal to perception can clear up the question what constitutes a person's ultimate source of evidence. For if we take perception to consist in a sensation caused by an event in the world (or in

the body of the perceiver), the fact of causality cannot be given apart from the sensation, and the sensation cannot serve as evidence unless it causes a belief. But how does one know that the belief was caused by a sensation? Only further beliefs can help. If perception is expressed by locutions like 'A perceives that there is a black raven', then this can certainly serve as evidence. This does not solve the problem, it only transfers it to the concept of perception, since to perceive that there is a black raven is to be caused by a raven, and *in the right way*, to believe that there is a black raven.

One is struck, in reading early writings of the members of the Vienna Circle, by the embarrassed way in which they refer to experience, what is immediately given, what is directly observed. Thus Schlick says that all meaning goes back to ostensive situations, 'and this means, in an obvious sense, reference to "experience" or "possibility of verification" '.¹⁵ In 'The Turning Point in Philosophy' he says 'The act of verification in which the path to the solution finally ends is always of the same sort: it is the occurrence of a definite fact that is confirmed by observation, by means of immediate experience';¹⁶ but in 'Positivism and Realism' he expresses grave doubts about terms like 'the given' (*das Gegebene*), and worries that if we use the word 'experience,' we will 'presuppose a distinction between what experiences and what is experienced'.¹⁷

There is, then, good reason to conclude that there is no clear meaning to the idea of comparing our beliefs with reality or confronting our hypotheses with observations. This is not, of course, to deny that there is an ordinary sense in which we perform experiments and note the results, or discover in our everyday pursuits that some of our beliefs are true and others false. What should be denied is that these mundane events are to be analyzed as involving (p. 165) evidence that is not propositional in character—evidence that is not some sort of belief. No wonder Neurath and Carnap were attracted to the idea of a coherence theory!

Of course, coherence theories of knowledge are not without difficulties, and these, Schlick was, as we have seen, quick to point out. Let me pause here for a moment to make the obvious distinction between a coherence theory of knowledge and a coherence theory of truth. In his 1935 paper 'On the Logical Positivist's Theory of Truth' Hempel had barely distinguished between the two; understandably, since he was not then aware of Tarski's method for defining truth semantically. He therefore was at the time inclined to think the only sense we can make of the phrase 'Sentence S is true' is 'S is highly confirmed by accepted observation reports'.¹⁸ But the concept of

being highly confirmed by accepted observation reports belongs rather in the domain of epistemology; and when coupled with the idea that protocol statements 'may only be characterized by the historical fact' that they are accepted (i.e. believed true), leads directly to a coherence theory of knowledge. This is the theory to which Schlick objected.

Schlick agreed with Neurath that protocols belong, in a general way, to the hypotheses of science. They are objective, and therefore intersubjectively understood and testable. They are *about* observations or experiences, but they don't attempt to *express* them. They take the form, roughly at least, of sentences like 'A saw a black raven at time *t*'. It is clear that one cannot be certain of the truth of such sentences—not even A at time *t* can be certain he is seeing a black raven; or, leaving the question of certainty aside, it is clear that anyone who judges such a sentence to be true may be wrong. Further evidence is always relevant, and may come to outweigh the evidence of the moment. Where Schlick disagreed with Neurath is on the question whether there are indisputable grounds on the basis of which we judge protocols to be true.¹⁹

The objection to Neurath's coherence theory was the standard objection to all such theories: consistency is not enough, since it leaves no basis on which to choose between various and conflicting consistent theories. Perhaps a theory of this kind banishes the last (p. 166) remnant of absolutism from philosophy, as Schlick said of Neurath's claims, but it leaves us with no basis for judging truth. Schlick insisted that we must have, and do have, indubitable grounds for choosing some sentences as the true ones rather than others. There are statements that are *not* protocol statements, which 'express facts of one's own "perception" ('or whatever you like to call it', he adds). Schlick then admits that 'in spite of the fact that statements of this sort seem so simple and clear, philosophers have found themselves in a hopeless labyrinth the moment they actually attempted to use them as the foundation of all knowledge'. But he thinks we can steer clear of the familiar difficulties if we remember that 'one's *own* statements in the end play the only decisive role'.

One must admit that Schlick's attempt to explain his view ends in obscurity. The observation 'sentences' that constitute the 'ultimate criterion' of all knowledge are not really sentences, being always of the form 'Here now so and so'. Such sentences cannot be written down (since they lose their certainty in a moment); they express a 'feeling of *fulfillment*, a quite characteristic satisfaction: we are *satisfied*'. 'One cannot build any logically

tenable structure upon such confirmations, for they are gone the moment one begins to construct.' Finally, 'the occasion of understanding [observation statements] is at the same time that of verifying them: I grasp their meaning at the same time as I grasp their truth'. One can sympathize with Neurath for rejecting this last step. But then one is left with a coherence theory.

Hempel calls the Neurath–Carnap position a 'restrained' coherence theory. The reason is that Neurath and Carnap do provide us with a criterion for picking out one scientific theory from among the consistent ones. The criterion is that it is the consistent theory that maximizes agreement with the statements historically held true by 'mankind especially the scientists of our culture circle'.²⁰ In the end, protocol sentences have no pride of place; like any others they may be abandoned if they conflict with too much else we hold true.

Thus it turns out, rather surprisingly, that both Schlick and Neurath held views that could be called 'restrained' coherence theories. They agreed that everything in the corpus of science, including protocol sentences, must be viewed as only tentatively established at any stage in the progress of science, and all sentences remain open (p. 167) to revision in the light of new evidence. They likewise agreed that when revision was called for, there were no strict rules for deciding where the revision should be made; it was a matter for 'decision'. Their sole important difference concerned the question how the whole pattern of sentences accepted at a given time by science was to be related to experience, observation, or the real world. And on this score their answers were less than clear or satisfactory.

Schlick's answer was unsatisfactory because it ended with something so private that even its meaning could only be given at a moment for an individual. How such a basis could warrant belief in a public objective world was not explained. Neurath rejected the idea of a confrontation between a belief about the world and the world itself as well as the idea of an incorrigible subjective basis for scientific knowledge. But his suggestion as to how to 'restrain' a coherence theory is unappealing. He suggests that we start with protocols of the form 'A sees a black raven at *t*', and he dismisses the idea that such protocols are any more the basis of A's knowledge than of B's. This guarantees the intersubjective aspect of the language of science right down to the protocols (since, as Neurath said, 'every language *as such* is inter-subjective'²¹—clearly a crack at Schlick and his observation statements, whose meaning is revealed to only one person, and then only for a moment). Neurath imagines all protocols being thrown

into one great machine; a bell rings if a contradiction arises; something must then be thrown out, either one or more protocols, or perhaps a law or other theoretical statement; but '*who* rebuilds the machine, or *whose* protocol sentences are thrown into the machine is of no consequence whatsoever'.²²

There is an obvious difficulty here. If protocol sentences are known only by their form, throwing them *all* in the machine will mean each sentence *and* its negation will be thrown in, as well as 'A sees a black raven at spot *s* at *t*' and 'B sees a non-black raven at spot *s* at *t*.' No basis for science can emerge from this, just endless consistent systems. If, on the other hand, the protocols are limited to the sentences that express *beliefs*, it *will* matter who mends the machine and whose protocols are thrown in or out. For each person will weigh the sentences he accepts (whether protocols or not) in accord with the strength of his beliefs—that's what it means to say they are *his* (p. 168) beliefs. He will give weight to other people's protocols to the extent that he believes they are true, just as Schlick maintained.

I would not dwell at such length on the familiar epistemological problems that beset Schlick and Neurath and their followers if I thought philosophy had now rejected them or solved them. On the contrary, I think the members of the Vienna Circle and their friends emphasized in a particularly useful way, even if partly in spite of themselves, a central unsettled problem in epistemology. And I think we can find a number of ideas and intuitions in the writings I have been discussing that point in the direction of a new view of the old problem.

The central problem may be stated as a dilemma in the theory of knowledge. Each person has a complex network of beliefs. Knowledge requires at least these two things: that some of these beliefs are true of the public world, and that each person has adequate reasons for holding these beliefs. I am willing to assume that the first condition is satisfied; the hard problem concerns the second issue, the way in which the system of beliefs is related to the world not merely semantically, but epistemologically. The dilemma arises because if we take as the connecting link something self-certifying (like Schlick's observation statements or events), it is so private as to lack connection with the sentences of the public language that alone are capable of expressing scientific, or even objective, claims. But if we start with sentences or beliefs already belonging to the public language (or what can be expressed in it), we find no intelligible way to base it on something self-certifying (Neurath's problem). In short, the foundations of knowledge must be subjective and objective at once, certain and yet open to question.

The problems I am rehearsing belong, we all know, to the foundations of epistemology, and in one form or another the problems are ancient. The logical positivists, one senses at once in their writings, were impatient with such problems, which they felt verged on the meaningless, or were to be solved by mere 'conventions' or 'decisions'. This attitude now seems to us, rereading these bold classics, cavalier. But the giddy conviction that a clear and correct line would somehow open up in the face of all that enthusiasm and intellectual power did produce some profoundly novel and valuable hints. What on the surface now may seem naive and failed attempts contained deeply suggestive intuitions of radical new ideas.

Given the positivists' tendency to remain within the vocabulary of (p. 169) intersubjective ideas and hypotheses, the flirtations of Neurath, Schlick, Hempel, and Carnap with some form of epistemological coherence theory is not surprising. But coherence theories have always been bedeviled by failure to distinguish between coherence theories of truth and coherence theories of knowledge. This is natural enough, since if knowledge, which is of the true, demands nothing but coherence of belief, how can truth require anything more than a set of coherent propositions? Thus one is invited to dismiss the difference between coherence of beliefs and coherence of sentences (or statements or propositions) as tests of knowledge and truth. But while we find this confusion in the writings of Schlick and Neurath, we also find moments when the distinction is clearly made. Neurath, we remember, hoped to destroy any aura of subjectivity in protocol statements by insisting that though it may be a 'historical accident' that one person is more inclined to accept his own protocols than those of someone else, in fact both are to be accepted on the same level. This leads him to the picture of the impersonal sorting machine into which protocol sentences are thrown. In proposing this idea, Neurath never hints that it is only sentences held to be true by someone that are to count. It is this fact that occasioned Schlick's outburst: 'The astounding error of the "coherence theory" can be explained only by the fact that its defenders and expositors were thinking only of such statements as actually occur in science.'²³ Here Schlick means, of course, such as occur as *assertions*. Yet in spite of much that we find in Neurath's article on protocol sentences, can he really be accused of having forgotten the difference between the coherence of an arbitrary set of sentences and the coherence of a set of sentences held true? The famous metaphor of the ship which must be rebuilt at sea piece by piece proves he was aware that it is beliefs that are at stake, not mere sentences, for if we were dealing with an arbitrary set of sentences, nothing would stop us from putting them all

in dry dock simultaneously. And Hempel, as I mentioned above, makes the distinction clear.

Since standard objections to coherence theories of knowledge parallel standard objections to coherence theories of truth, it is not immediately apparent why it is so important to distinguish between them. But of course beliefs are not historically or causally arbitrary; even if our *reasons* for our beliefs are always other beliefs, the *causes* sometimes lie elsewhere. Some appreciation of the (p. 170) importance of this point must, as we will see, be credited to the logical positivists.

The logical positivists preferred to talk of sentences or 'statements' rather than beliefs, and we can easily enough make this switch as long as we remember that the sentences that correspond to beliefs are (1) sentences held true by someone, and (2) sentences that have an interpretation. Someone else can know what I believe if he knows what sentences I hold true, and what those sentences mean. Let me review some of the logical positivists' views in the light of these simple considerations.

There is Schlick's idea that observation sentences are understood in the act of grasping their truth. This may well seem extreme or obscure, but it is related to the correct doctrine that an interpreter is constrained to take first person present tense attributions of attitude as presumptively true. Such sentences (of English) as 'I believe I now see a black raven', if held true by a speaker, require of an interpreter that he assign a high a priori probability to their truth. This means: so interpret such sentences as to make them true when possible.

Hempel comments on the fact that the protocol statements 'produced' by different men might not admit the construction of a unique system of scientific statements. He goes on: 'but fortunately this possibility is not realized: in fact, by far the greater part of scientists will sooner or later come to an agreement, and so, as an empirical fact, a perpetually increasing and expanding system of coherent statements and theories results from their protocol statements'.²⁴ Again, we must assume that protocol statements are not any sentences written down, or uttered; they must be sentences their speakers believe to be true, or at least that a hearer believes the speaker to have held true. But it is surely odd to consider it merely 'fortunate' that there is a large degree of consensus: and why should we expect agreement to increase over time?

Schlick has an even more surprising discussion of the possibility that someone might discover that all his own observations in no way substantiate the assertions made about the world by other men. He says that under these circumstances one would not, as Neurath's protocol machine would, simply sacrifice one's own protocol statements. Instead, one would cling to a (p. 171)

system of knowledge into which one's own observations fitted un mutilated. And I can always construct such a system. I need only view the others as dreaming fools, in whose madness lies a remarkable method, or—to express it more objectively—I would say that the others live in a different world from mine . . . In any case no matter what world picture I construct, I would test its truth always in terms of my own experience.²⁵

This is a remarkable admission from someone who has objected that a coherence theory leaves us with an unacceptable relativism.

Elsewhere in Schlick, however, we find a rather different way of viewing the possibility of massive disagreement. He notes a basic contrast between a disagreement over whether two pieces of paper are the same color, and a disagreement over what color both pieces are. With respect to the first he says that 'by virtue of linguistic usage the proposition expresses just that experience'—i.e. the experience of sameness. But in the case of color, there is no objective way—that is, no way—to tell if you and I experience the same color. Schlick says that even if all your expressed judgements about color agree entirely with mine, I cannot infer from this that you experience the same quality. As long as the inner order of your experiences agrees with mine, we will understand each other perfectly.²⁶

Schlick seems in the end to reject the view that the experiences may be undetectably different. ' . . . the statement that different individuals have the same experience has its sole verifiable meaning in the fact that all their assertions . . . exhibit certain agreements . . . the statement *means* nothing but this.'²⁷ It is not easy to tell from this passage whether Schlick thinks the experiences might be qualitatively different while we could not in principle discern this, or meaningfully claim it; or whether he thinks no such situation could arise. The radical suggestion, which it is not impossible to read into Schlick's attack on the coherence theory, is that interpersonal agreement, and hence objectivity, are built into the way in which we determine the meanings of other people's utterances, and hence the contents of their beliefs.

I mentioned above Hempel's remark that 'fortunately' the protocol statements of different people allow the construction of a unique system of science. He adds that Carnap has 'perhaps provided us a (p. 172) possibility of explaining this fortunate fact'.²⁸ The possible explanation lies in the fact that 'young scientists are conditioned' to produce true protocol statements, and he adds, 'Perhaps the fact of the general and rather congruous conditioning of scientists may explain to a certain degree the fact of a unique system of science.'²⁹

It would be mysterious if people were first taught what various sentences mean, and *then* were conditioned to 'produce' the true ones; this would amount to teaching them, on the one hand, how to be better observers, and, on the other hand, how to be honest. But the situation may be seen rather as a matter of conditioning people, as we surely do, to hold certain sentences true under publicly observable conditions, and fixing on the interpretation of utterances of the sentences in accord with the success of the conditioning. This would explain interpersonal agreement on the main features of the environment in a natural way.

Carnap at one point seems clearly to take this line. In 'Psychology in Physical Language' (written in 1932) Carnap flatly rejects Neurath's idea that I must or can treat your protocol sentences on a par with my own.

Generally speaking [he writes], a psychologist's spoken, written, or printed protocol sentences, when they are based on so-called introspection, are to be interpreted by the reader, and so figure in inter-subjective science, *not chiefly as scientific sentences, but as scientific facts*. The epistemological confusion of contemporary psychology stems, to a large extent, from this confusion of facts in the form of sentences with the sentences themselves considered as parts of science.³⁰

The inferences we are permitted to draw from the fact that someone else utters a sentence are not the deductive consequences that flow from that sentence as interpreted, but rather the sort of inference we can draw from observing the movements of a voltmeter, or the movements of a raindrop. The point is not that others do not mean anything by the sentences they utter, but that we cannot take for granted that we know in advance *what* they mean; and interpretation is explicitly called for or implicitly assumed.

I think that by following out this line, along with several other suggestions drawn from passages I have been quoting, we can (p. 173) discover the

outline of a correct view of the foundations of empirical knowledge, a view that reconciles Neurath's coherentist theory with Schlick's insistence on a basic tie to experience and observation.

From here on, although I shall be borrowing in many and obvious ways on ideas of Schlick, Neurath, Hempel, and Carnap, I am stating my own position. This is a position deeply influenced by Quine, though it is not Quine's position.

Neurath was right in rejecting the intelligibility of comparing sentences or beliefs with reality. We experiment and observe, but this is not 'comparing' in any but a metaphorical sense, for our experimentation bears no epistemological fruit except as it *causes* us to add to, cling to, or abandon our beliefs. This causal relation cannot be a relation of *confirmation* or *disconfirmation*, since the cause is not a proposition or belief, but just an event in the world or in our sensory apparatus. Nor can such events be considered in themselves to be evidence, unless, of course, they cause us to believe something. And then it is the belief that is properly called the evidence, not the event.

Neurath was also right in saying that, given this situation, we may as well admit that protocols, like any other propositions of science or common sense, can be wrong; we stand ready to tinker where tinkering does the most good. As Hempel observed, no epistemological priority is left to protocols—they are like the rest. All this is, of course, the line Quine was later to exploit in arguing against the analytic–synthetic distinction.

We are left, then, as Neurath insisted, in a situation where our only evidence for a belief is other beliefs; this is not merely the *logical* situation, but also the pragmatic situation. And since no belief is self-certifying, no set of beliefs can supply a certain basis for the rest. How then can we escape Schlick's objection that this makes 'arbitrary fairy stories to be as true as a historical report'? He concludes: 'Thus the coherence theory is shown to be logically impossible . . . for by means of it I can arrive at any number of consistent systems of statements which are incompatible with one another.'³¹ It's not clear what it means to say I could 'arrive' at various systems, since I do not invent my beliefs; most of them are not voluntary. Still, the point of the criticism would seem to remain in the form of a challenge to say what reason I have to consider the bulk of my beliefs true.

(p. 174) The key to the answer lies, I think, in generalizing Carnap's two suggestions that we are conditioned to produce (hold true) specific

sentences under particular conditions, and that we cannot use other people's statements as evidence until we have interpreted them. Carnap said this only about protocol sentences, but the same should be said about all language.

Language is in its nature, as Neurath insisted, intersubjective; what someone else's words mean on a given occasion is always something that we can in principle learn from public clues. Consider how we discover what some simple sentence means, say 'There's a table' or 'Here's a piece of green paper'. Our basic evidence is that the speaker is caused to assent (not just on this occasion, but generally) to these sentences by the presence of tables or pieces of green paper, while the absence of these objects causes him (generally) to dissent from the same sentences. I do not think of assent and dissent as overt speech acts, but as attitudes towards sentences sometimes revealed in speech and sometimes in other ways. My main point is that our basic methodology for interpreting the words of others necessarily makes it the case that most of the time the simplest sentences that speakers hold true *are* true. It is not the *speaker* who must perform the impossible feat of comparing his belief with reality; it is the *interpreter* who must take into account the causal interaction between world and speaker in order to find out what the speaker means, and hence what that speaker believes. Each speaker can do no better than make his system of beliefs coherent, adjusting the system as rationally as he can as new beliefs are thrust on him. But there is no need to fear that these beliefs might be just a fairy tale. For the sentences that express the beliefs, and the beliefs themselves, are correctly understood to be about the public things and events that cause them, and so must be mainly veridical. Each individual knows this, since he knows the nature of speech and belief. This does not, of course, tell him *which* of his beliefs and sentences are true, but it does assure him that his overall picture of the world around him is like the picture other people have, and is in its large features correct.

Neurath, Carnap, and Hempel were right, I believe, in abandoning the search for a basic sort of evidence on which our knowledge of the world could rest. None is available, and none is needed. What they perhaps failed to appreciate is *why* it is not needed. It is not needed because the causal relations between our beliefs and speech, and the **(p. 175)** world also supply the interpretation of our language and of our beliefs. In this rather special sense, 'experience' is the source of all knowledge. But this is a sense that does not encourage us to find a mental or inferential bridge between external events and ordinary beliefs. The bridge is there all right—a causal

bridge that involves the sense organs. The error lies, as Neurath saw, in trying to turn this causal bridge into an epistemological one, with sense data, uninterpreted givens, or unwritable sentences constituting its impossible spans.

There are of course some beliefs that carry a very high degree of certitude, and in some cases their content creates a presumption in favor of their truth. These are beliefs about our own present propositional attitudes. But the relative certitude of these beliefs does not suit them to be the foundation of empirical knowledge. It springs, rather, from the nature of interpretation. As interpreters we have to treat self-ascriptions of belief, doubt, desire, and the like as privileged; this is an essential step in interpreting the rest of what the person says and thinks. The foundations of interpretation are not the foundations of knowledge, though an appreciation of the nature of interpretation can lead to an appreciation of the essentially veridical nature of belief. (p. 176)

Notes:

(1) See Carl Hempel, 'Empiricist Criteria of Cognitive Significance: Problems and Changes', in *Aspects of Scientific Explanation*.

(2) Otto Neurath, 'Protocol Sentences'; quoted from Ayer (ed.), *Logical Positivism*, 204, 206. All subsequent page references from *Logical Positivism* are preceded by *LP*.

(3) Moritz Schlick, 'The Foundation of Knowledge', *LP* 215.

(4) Carl Hempel, 'On the Logical Positivist's Theory of Truth', 58, 59.

(5) Moritz Schlick, 'The Foundation of Knowledge', *LP* 213.

(6) Carl Hempel, 'The Empiricist Criterion of Meaning', *LP* 108-10.

(7) Carl Hempel, 'On the Logical Positivist's Theory of Truth', 51.

(8) Note 49 to 'Studies in the logic of Confirmation', in *Aspects of Scientific Explanation*.

(9) Moritz Schlick, 'Positivism and Realism', *LP* 92-3.

(10) Moritz Schlick, 'The Foundation of Knowledge', *LP* 215.

- (11) Ibid. 210.
- (12) Ibid. 220.
- (13) Carl Hempel, 'Studies in the Logic of Confirmation', in *Aspects of Scientific Explanation*, 14.
- (14) Ibid. 21-2.
- (15) Moritz Schlick, 'Meaning and Verification', 148.
- (16) Moritz Schlick, 'The Turning Point in Philosophy', *LP* 56.
- (17) Moritz Schlick, 'Positivism and Realism', *LP* 84.
- (18) Carl Hempel, 'Studies in the Logic of Confirmation', in *Aspects of Scientific Explanation*, 42.
- (19) Moritz Schlick, 'The Foundation of Knowledge', *LP* 213. The following seven quotations are from *LP* 218-25.
- (20) Carl Hempel, 'On the Logical Positivist's Theory of Truth', 57.
- (21) Otto Neurath, 'Protocol Sentences', *LP*. 205.
- (22) Ibid. 207.
- (23) Moritz Schlick, 'The Foundation of Knowledge', *LP* 215.
- (24) Carl Hempel, 'On the Logical Positivist's Theory of Truth', 57.
- (25) Moritz Schlick, 'The Foundation of Knowledge', *LP* 219.
- (26) Here I paraphrase Schlick, 'Positivism and Realism', *LP* 93.
- (27) Ibid. 93.
- (28) Carl Hempel, 'On the Logical Positivist's Theory of Truth', 57.
- (29) Ibid. 58.
- (30) Rudolph Carnap, 'Psychology in Physical Language', *LP* 195.
- (31) Moritz Schlick, 'The Foundation of Knowledge', *LP* 216.

