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# QUINTESSENCE

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Basic Readings from the Philosophy of  
W. V. Quine

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## EPISTEMOLOGY NATURALIZED

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Epistemology is concerned with the foundations of science. Conceived thus broadly, epistemology includes the study of the foundations of mathematics as one of its departments. Specialists at the turn of the century thought that their efforts in this particular department were achieving notable success: mathematics seemed to reduce altogether to logic. In a more recent perspective this reduction is seen to be better describable as a reduction to logic and set theory. This correction is a disappointment epistemologically, since the firmness and obviousness that we associate with logic cannot be claimed for set theory. But still the success achieved in the foundations of mathematics remains exemplary by comparative standards, and we can illuminate the rest of epistemology somewhat by drawing parallels to this department.

*Epistemology  
includes foundations  
of mathematics*

*Reduction to  
logic has failed*

Studies in the foundations of mathematics divide symmetrically into two sorts, conceptual and doctrinal. The conceptual studies are concerned with meaning, the doctrinal with truth. The conceptual studies are concerned with clarifying concepts by defining them, some in terms of others. The doctrinal studies are concerned with establishing laws by proving them, some on the basis of others. Ideally the obscurer concepts would be defined in terms of the clearer ones so as to maximize clarity, and the less obvious laws would be proved from the more obvious ones so as to maximize certainty. Ideally the definitions would generate all the concepts from clear and distinct ideas, and the proofs would generate all the theorems from self-evident truths.

The two ideals are linked. For, if you define all the concepts by use of some favored subset of them, you thereby show how to translate all theorems into these favored terms. The clearer these terms are, the likelier it is that the truths couched in them will be obviously true, or derivable

from obvious truths. If in particular the concepts of mathematics were all reducible to the clear terms of logic, then all the truths of mathematics would go over into truths of logic; and surely the truths of logic are all obvious or at least potentially obvious, i.e., derivable from obvious truths by individually obvious steps.

This particular outcome is in fact denied us, however, since mathematics reduces only to set theory and not to logic proper. Such reduction still enhances clarity, but only because of the interrelations that emerge and not because the end terms of the analysis are clearer than others. As for the end truths, the axioms of set theory, these have less obviousness and certainty to recommend them than do most of the mathematical theorems that we would derive from them. Moreover, we know from Gödel's work that no consistent axiom system can cover mathematics even when we renounce self-evidence. Reduction in the foundations of mathematics remains mathematically and philosophically fascinating, but it does not do what the epistemologist would like of it: it does not reveal the ground of mathematical knowledge, it does not show how mathematical certainty is possible.

Still there remains a helpful thought, regarding epistemology generally, in that duality of structure which was especially conspicuous in the foundations of mathematics. I refer to the bifurcation into a theory of concepts, or meaning, and a theory of doctrine, or truth; for this applies to the epistemology of natural knowledge no less than to the foundations of mathematics. The parallel is as follows. Just as mathematics is to be reduced to logic, or logic and set theory, so natural knowledge is to be based somehow on sense experience. This means explaining the notion of body in sensory terms; here is the conceptual side. And it means justifying our knowledge of truths of nature in sensory terms; here is the doctrinal side of the bifurcation.

Hume pondered the epistemology of natural knowledge on both sides of the bifurcation, the conceptual and the doctrinal. His handling of the conceptual side of the problem, the explanation of body in sensory terms, was bold and simple: he identified bodies outright with the sense impressions. If common sense distinguishes between the material apple and our sense impressions of it on the ground that the apple is one and enduring while the impressions are many and fleeting, then, Hume held, so much the worse for common sense; the notion of its being the same apple on one occasion and another is a vulgar confusion.

Nearly a century after Hume's *Treatise*, the same view of bodies was

espoused by the early American philosopher Alexander Bryan Johnson.<sup>1</sup> "The word iron names an associated sight and feel," Johnson wrote.

What then of the doctrinal side, the justification of our knowledge of truths about nature? Here, Hume despaired. By his identification of bodies with impressions he did succeed in construing some singular statements about bodies as indubitable truths, yes; as truths about impressions, directly known. But general statements, also singular statements about the future, gained no increment of certainty by being construed as about impressions.

On the doctrinal side, I do not see that we are farther along today than where Hume left us. The Humean predicament is the human predicament. But on the conceptual side there has been progress. There the crucial step forward was made already before Alexander Bryan Johnson's day, although Johnson did not emulate it. It was made by Bentham in his theory of fictions. Bentham's step was the recognition of contextual definition, or what he called paraphrasis. He recognized that to explain a term we do not need to specify an object for it to refer to, nor even specify a synonymous word or phrase; we need only show, by whatever means, how to translate all the whole sentences in which the term is to be used. Hume's and Johnson's desperate measure of identifying bodies with impressions ceased to be the only conceivable way of making sense of talk of bodies, even granted that impressions were the only reality. One could undertake to explain talk of bodies in terms of talk of impressions by translating one's whole sentences about bodies into whole sentences about impressions, without equating the bodies themselves to anything at all.

This idea of contextual definition, or recognition of the sentence as the primary vehicle of meaning, was indispensable to the ensuing developments in the foundations of mathematics. It was explicit in Frege, and it attained its full flower in Russell's doctrine of singular descriptions as incomplete symbols.

Contextual definition was one of two resorts that could be expected to have a liberating effect upon the conceptual side of the epistemology of natural knowledge. The other is resort to the resources of set theory as auxiliary concepts. The epistemologist who is willing to eke out his austere ontology of sense impressions with these set-theoretic auxiliaries is suddenly rich: he has not just his impressions to play with, but sets of

*But statements about future behaviour of bodies could not be reduced (=problem of induction)*

*The problem of induction cannot be solved*

*Contextual definition is a step forward in conceptual reduction*

*Reduction in natural knowledge: bodies to sense-data*

*Hume identified bodies with sense impressions*

1. A. B. Johnson, *A Treatise on Language* (New York, 1836; Berkeley, 1947).

them, and sets of sets, and so on up. Constructions in the foundations of mathematics have shown that such set-theoretic aids are a powerful addition; after all, the entire glossary of concepts of classical mathematics is constructible from them. Thus equipped, our epistemologist may not need either to identify bodies with impressions or to settle for contextual definition; he may hope to find in some subtle construction of sets upon sets of sense impressions a category of objects enjoying just the formula properties that he wants for bodies.

The two resorts are very unequal in epistemological status. Contextual definition is unassailable. Sentences that have been given meaning as wholes are undeniably meaningful, and the use they make of their component terms is therefore meaningful, regardless of whether any translations are offered for those terms in isolation. Surely Hume and A. B. Johnson would have used contextual definition with pleasure if they had thought of it. Recourse to sets, on the other hand, is a drastic ontological move, a retreat from the austere ontology of impressions. There are philosophers who would rather settle for bodies outright than accept all these sets, which amount, after all, to the whole abstract ontology of mathematics.

This issue has not always been clear, however, owing to deceptive hints of continuity between elementary logic and set theory. This is why mathematics was once believed to reduce to logic, that is, to an innocent and unquestionable logic, and to inherit these qualities. And this is probably why Russell was content to resort to sets as well as to contextual definition when in *Our Knowledge of the External World* and elsewhere he addressed himself to the epistemology of natural knowledge, on its conceptual side.

To account for the external world as a logical construct of sense data—such, in Russell's terms, was the program. It was Carnap, in his *Der logische Aufbau der Welt* of 1928, who came nearest to executing it.

This was the conceptual side of epistemology; what of the doctrinal? There the Humean predicament remained unaltered. Carnap's constructions, if carried successfully to completion, would have enabled us to translate all sentences about the world into terms of sense data, or observation, plus logic and set theory. But the mere fact that a sentence is *couched* in terms of observation, logic, and set theory does not mean that it can be *proved* from observation sentences by logic and set theory. The most modest of generalizations about observable traits will cover more cases than its utterer can have had occasion actually to observe.

*The problem of induction is intact*

The hopelessness of grounding natural science upon immediate experience in a firmly logical way was acknowledged. The Cartesian quest for certainty had been the remote motivation of epistemology, both on its conceptual and its doctrinal side; but that quest was seen as a lost cause. To endow the truths of nature with the full authority of immediate experience was as forlorn a hope as hoping to endow the truths of mathematics with the potential obviousness of elementary logic.

What then could have motivated Carnap's heroic efforts on the conceptual side of epistemology, when hope of certainty on the doctrinal side was abandoned? There were two good reasons still. One was that such constructions could be expected to elicit and clarify the sensory evidence for science, even if the inferential steps between sensory evidence and scientific doctrine must fall short of certainty. The other reason was that such constructions would deepen our understanding of our discourse about the world, even apart from questions of evidence; it would make all cognitive discourse as clear as observation terms and logic and, I must regretfully add, set theory.

It was sad for epistemologists, Hume and others, to have to acquiesce in the impossibility of strictly deriving the science of the external world from sensory evidence. Two cardinal tenets of empiricism remained unassailable, however, and so remain to this day. One is that whatever evidence there *is* for science *is* sensory evidence. The other, to which I shall recur, is that all inculcation of meanings of words must rest ultimately on sensory evidence. Hence the continuing attractiveness of the idea of a *logischer Aufbau* in which the sensory content of discourse would stand forth explicitly.

If Carnap had successfully carried such a construction through, how could he have told whether it was the right one? The question would have had no point. He was seeking what he called a *rational reconstruction*. Any construction of physicalistic discourse in terms of sense experience, logic, and set theory would have been seen as satisfactory if it made the physicalistic discourse come out right. If there is one way there are many, but any would be a great achievement.

But why all this creative reconstruction, all this make-believe? The stimulation of his sensory receptors is all the evidence anybody has had to go on, ultimately, in arriving at his picture of the world. Why not just see how this construction really proceeds? Why not settle for psychology? Such a surrender of the epistemological burden to psychology is a move that was disallowed in earlier times as circular reasoning. If the

*Utility of Carnapian reductions*

*Two principles are intact: doctrine of sensory evidence and meaning verificationism*

*The plurality of rational reconstructions*

*Can epistemology rest on psychology?*

*Is there a circularity?*

epistemologist's goal is validation of the grounds of empirical science, he defeats his purpose by using psychology or other empirical science in the validation. However, such scruples against circularity have little point once we have stopped dreaming of deducing science from observations. If we are out simply to understand the link between observation and science, we are well advised to use any available information, including that provided by the very science whose link with observation we are seeking to understand.

But there remains a different reason, unconnected with fears of circularity, for still favoring creative reconstruction. We should like to be able to *translate* science into logic and observation terms and set theory. This would be a great epistemological achievement, for it would show all the rest of the concepts of science to be theoretically superfluous. It would legitimize them—to whatever degree the concepts of set theory, logic, and observation are themselves legitimate—by showing that everything done with the one apparatus could in principle be done with the other. If psychology itself could deliver a truly translational reduction of this kind, we should welcome it; but certainly it cannot, for certainly we did not grow up learning definitions of physicalistic language in terms of a prior language of set theory, logic, and observation. Here, then, would be good reason for persisting in a rational reconstruction: we want to establish the essential innocence of physical concepts, by showing them to be theoretically dispensable.

The fact is, though, that the construction which Carnap outlined in *Der logische Aufbau der Welt* does not give translational reduction either. It would not even if the outline were filled in. The crucial point comes where Carnap is explaining how to assign sense qualities to positions in physical space and time. These assignments are to be made in such a way as to fulfill, as well as possible, certain desiderata which he states, and with growth of experience the assignments are to be revised to suit. This plan, however illuminating, does not offer any key to *translating* the sentences of science into terms of observation, logic, and set theory.

We must despair of any such reduction. Carnap had despaired of it by 1936, when, in "Testability and Meaning,"<sup>2</sup> he introduced so-called *reduction forms* of a type weaker than definition. Definitions had shown always how to translate sentences into equivalent sentences. Contextual

definition of a term showed how to translate sentences containing the term into equivalent sentences lacking the term. Reduction forms of Carnap's liberalized kind, on the other hand, do not in general give equivalences; they give implications. They explain a new term, if only partially, by specifying some sentences which are implied by sentences containing the term, and other sentences which imply sentences containing the term.

It is tempting to suppose that the countenancing of reduction forms in this liberal sense is just one further step of liberalization comparable to the earlier one, taken by Bentham, of countenancing contextual definition. The former and sterner kind of rational reconstruction might have been represented as a fictitious history in which we imagined our ancestors introducing the terms of physicalistic discourse on a phenomenalistic and set-theoretic basis by a succession of contextual definitions. The new and more liberal kind of rational reconstruction is a fictitious history in which we imagine our ancestors introducing those terms by a succession rather of reduction forms of the weaker sort.

This, however, is a wrong comparison. The fact is rather that the former and sterner kind of rational reconstruction, where definition reigned, embodied no fictitious history at all. It was nothing more nor less than a set of directions—or would have been, if successful—for accomplishing everything in terms of phenomena and set theory that we now accomplish in terms of bodies. It would have been a true reduction by translation, a legitimation by elimination. *Definire est eliminare*. Rational reconstruction by Carnap's later and looser reduction forms does none of this.

To relax the demand for definition, and settle for a kind of reduction that does not eliminate, is to renounce the last remaining advantage that we supposed rational reconstruction to have over straight psychology; namely, the advantage of translational reduction. If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology. Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect.

The empiricist made one major concession when he despaired of deducing the truths of nature from sensory evidence. In despairing now even of translating those truths into terms of observation and logico-mathematical auxiliaries, he makes another major concession. For suppose we hold, with the old empiricist Peirce, that the very meaning of a

*Then psychology can offer base for epistemology*

2. *Philosophy of Science* 3 (1936), pp. 419–471; 4 (1937), pp. 1–40.

*No threat of circularity if no attempt to deduce science from observation*

*Psychology is inept in delivering translations of scientific statements*

*But reductions cannot deliver translations either...*



statement consists in the difference its truth would make to possible experience. Might we not formulate, in a chapter-length sentence in observational language, all the difference that the truth of a given statement might make to experience, and might we not then take all this as the translation? Even if the difference that the truth of the statement would make to experience ramifies indefinitely, we might still hope to embrace it all in the logical implications of our chapter-length formulation, just as we can axiomatize an infinity of theorems. In giving up hope of such translation, then, the empiricist is conceding that the empirical meanings of typical statements about the external world are inaccessible and ineffable.

How is this inaccessibility to be explained? Simply on the ground that the experiential implications of a typical statement about bodies are too complex for finite axiomatization, however lengthy? No; I have a different explanation. It is that the typical statement about bodies has no fund of experiential implications it can call its own. A substantial mass of theory, taken together, will commonly have experiential implications; this is how we make verifiable predictions. We may not be able to explain why we arrive at theories which make successful predictions, but we do arrive at such theories.

Sometimes also an experience implied by a theory fails to come off; and then, ideally, we declare the theory false. But the failure falsifies only a block of theory as a whole, a conjunction of many statements. The failure shows that one or more of those statements is false, but it does not show which. The predicted experiences, true and false, are not implied by any one of the component statements of the theory rather than another. The component statements simply do not have empirical meanings, by Peirce's standard; but a sufficiently inclusive portion of theory does. If we can aspire to a sort of *logischer Aufbau der Welt* at all, it must be to one in which the texts slated for translation into observational and logico-mathematical terms are mostly broad theories taken as wholes, rather than just terms or short sentences. The translation of a theory would be a ponderous axiomatization of all the experiential difference that the truth of the theory would make. It would be a queer translation, for it would translate the whole but none of the parts. We might better speak in such a case not of translation but simply of observational evidence for theories; and we may, following Peirce, still fairly call this the empirical meaning of the theories.

These considerations raise a philosophical question even about ordinary unphilosophical translation, such as from English into Arunta or

Chinese. For, if the English sentences of a theory have their meaning only together as a body, then we can justify their translation into Arunta only together as a body. There will be no justification for pairing off the component English sentences with component Arunta sentences, except as these correlations make the translation of the theory as a whole come out right. Any translations of the English sentences into Arunta sentences will be as correct as any other, so long as the net empirical implications of the theory as a whole are preserved in translation. But it is to be expected that many different ways of translating the component sentences, essentially different individually, would deliver the same empirical implications for the theory as a whole; deviations in the translation of one component sentence could be compensated for in the translation of another component sentence. Insofar, there can be no ground for saying which of two glaringly unlike translations of individual sentences is right.<sup>3</sup>

For an uncritical mentalist, no such indeterminacy threatens. Every term and every sentence is a label attached to an idea, simple or complex, which is stored in the mind. When on the other hand we take a verification theory of meaning seriously, the indeterminacy would appear to be inescapable. The Vienna Circle espoused a verification theory of meaning but did not take it seriously enough. If we recognize with Peirce that the meaning of a sentence turns purely on what would count as evidence for its truth, and if we recognize with Duhem that theoretical sentences have their evidence not as single sentences but only as larger blocks of theory, then the indeterminacy of translation of theoretical sentences is the natural conclusion. And most sentences, apart from observation sentences, are theoretical. This conclusion, conversely, once it is embraced, seals the fate of any general notion of propositional meaning or, for that matter, state of affairs.

Should the unwelcomeness of the conclusion persuade us to abandon the verification theory of meaning? Certainly not. The sort of meaning that is basic to translation, and to the learning of one's own language, is necessarily empirical meaning and nothing more. A child learns his first words and sentences by hearing and using them in the presence of appropriate stimuli. These must be external stimuli, for they must act both on the child and on the speaker from whom he is learning.<sup>4</sup> Language is so-

*Science is underdetermined by evidence*

*Verificationism is intact*

3. See above, pp. 2ff. [Chapter 5].

4. See above, p. 28 (W. V. Quine, "Ontological Relativity," in *Ontological Relativity and Other Essays* [New York: Columbia University Press, 1969]).

*Meaning holism explains why theoretical statements cannot be translated individually*

*Language is a  
social and natural  
institution*

cially inculcated and controlled; the inculcation and control turn strictly on the keying of sentences to shared stimulation. Internal factors may vary *ad libitum* without prejudice to communication as long as the keying of language to external stimuli is undisturbed. Surely one has no choice but to be an empiricist so far as one's theory of linguistic meaning is concerned.

What I have said of infant learning applies equally to the linguist's learning of a new language in the field. If the linguist does not lean on related languages for which there are previously accepted translation practices, then obviously he has no data but the concomitances of native utterance and observable stimulus situation. No wonder there is indeterminacy of translation—for of course only a small fraction of our utterances report concurrent external stimulation. Granted, the linguist will end up with unequivocal translations of everything; but only by making many arbitrary choices—arbitrary even though unconscious—along the way. Arbitrary? By this I mean that different choices could still have made everything come out right that is susceptible in principle to any kind of check.

Let me link up, in a different order, some of the points I have made. The crucial consideration behind my argument for the indeterminacy of translation was that a statement about the world does not always or usually have a separable fund of empirical consequences that it can call its own. That consideration served also to account for the impossibility of an epistemological reduction of the sort where every sentence is equated to a sentence in observational and logico-mathematical terms. And the impossibility of that sort of epistemological reduction dissipated the last advantage that rational reconstruction seemed to have over psychology.

Philosophers have rightly despaired of translating everything into observational and logico-mathematical terms. They have despaired of this even when they have not recognized, as the reason for this irreducibility, that the statements largely do not have their private bundles of empirical consequences. And some philosophers have seen in this irreducibility the bankruptcy of epistemology. Carnap and the other logical positivists of the Vienna Circle had already pressed the term “metaphysics” into pejorative use, as connoting meaninglessness; and the term “epistemology” was next. Wittgenstein and his followers, mainly at Oxford, found a residual philosophical vocation in therapy: in curing philosophers of the delusion that there were epistemological problems.

But I think that at this point it may be more useful to say rather that

epistemology still goes on, though in a new setting and a clarified status. Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input—certain patterns of irradiation in assorted frequencies, for instance—and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history. The relation between the meager input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence.

Such a study could still include, even, something like the old rational reconstruction, to whatever degree such reconstruction is practicable; for imaginative constructions can afford hints of actual psychological processes, in much the way that mechanical simulations can. But a conspicuous difference between old epistemology and the epistemological enterprise in this new psychological setting is that we can now make free use of empirical psychology.

The old epistemology aspired to contain, in a sense, natural science; it would construct it somehow from sense data. Epistemology in its new setting, conversely, is contained in natural science, as a chapter of psychology. But the old containment remains valid too, in its way. We are studying how the human subject of our study posits bodies and projects his physics from his data, and we appreciate that our position in the world is just like his. Our very epistemological enterprise, therefore, and the psychology wherein it is a component chapter, and the whole of natural science wherein psychology is a component book—all this is our own construction or projection from stimulations like those we were meting out to our epistemological subject. There is thus reciprocal containment, though containment in different senses: epistemology in natural science and natural science in epistemology.

This interplay is reminiscent again of the old threat of circularity, but it is all right now that we have stopped dreaming of deducing science from sense data. We are after an understanding of science as an institution or process in the world, and we do not intend that understanding to be any better than the science which is its object. This attitude is indeed one that Neurath was already urging in Vienna Circle days, with his parable of the mariner who has to rebuild his boat while staying afloat in it.

*Epistemology is a  
part of psychology*

*Old epistemological  
questions are  
answered in a new  
way*

*No threat of  
circularity: science  
is a process in the  
world*

One effect of seeing epistemology in a psychological setting is that it resolves a stubborn old enigma of epistemological priority. Our retinas are irradiated in two dimensions, yet we see things as three-dimensional without conscious inference. Which is to count as observation—the unconscious two-dimensional reception or the conscious three-dimensional apprehension? In the old epistemological context the conscious form had priority, for we were out to justify our knowledge of the external world by rational reconstruction, and that demands awareness. Awareness ceased to be demanded when we gave up trying to justify our knowledge of the external world by rational reconstruction. What to count as observation now can be settled in terms of the stimulation of sensory receptors, let consciousness fall where it may.

The Gestalt psychologists' challenge to sensory atomism, which seemed so relevant to epistemology forty years ago, is likewise deactivated. Regardless of whether sensory atoms or Gestalten are what favor the forefront of our consciousness, it is simply the stimulations of our sensory receptors that are best looked upon as the input to our cognitive mechanism. Old paradoxes about unconscious data and inference, old problems about chains of inference that would have to be completed too quickly—these no longer matter.

In the old anti-psychologistic days the question of epistemological priority was moot. What is epistemologically prior to what? Are Gestalten prior to sensory atoms because they are noticed, or should we favor sensory atoms on some more subtle ground? Now that we are permitted to appeal to physical stimulation, the problem dissolves; *A* is epistemologically prior to *B* if *A* is causally nearer than *B* to the sensory receptors. Or, what is in some ways better, just talk explicitly in terms of causal proximity to sensory receptors and drop the talk of epistemological priority.

Around 1932 there was debate in the Vienna Circle over what to count as observation sentences, or *Protokollsätze*.<sup>5</sup> One position was that they had the form of reports of sense impressions. Another was that they were statements of an elementary sort about the external world, e.g., "A red cube is standing on the table." Another, Neurath's, was that they had the form of reports of relations between percipients and external things: "Otto now sees a red cube on the table." The worst of it was that there seemed to be no objective way of settling the matter: no way of making real sense of the question.

5. Carnap and Neurath in *Erkenntnis* 3 (1932), pp. 204–228.

Let us now try to view the matter unreservedly in the context of the external world. Vaguely speaking, what we want of observation sentences is that they be the ones in closest causal proximity to the sensory receptors. But how is such proximity to be gauged? The idea may be rephrased this way: observation sentences are sentences which, as we learn language, are most strongly conditioned to concurrent sensory stimulation rather than to stored collateral information. Thus let us imagine a sentence queried for our verdict as to whether it is true or false; queried for our assent or dissent. Then the sentence is an observation sentence if our verdict depends only on the sensory stimulation present at the time.

But a verdict cannot depend on present stimulation to the exclusion of stored information. The very fact of our having learned the language evinces much storing of information, and of information without which we should be in no position to give verdicts on sentences however observational. Evidently then we must relax our definition of observation sentence to read thus: a sentence is an observation sentence if all verdicts on it depend on present sensory stimulation and on no stored information beyond what goes into understanding the sentence.

This formulation raises another problem: how are we to distinguish between information that goes into understanding a sentence and information that goes beyond? This is the problem of distinguishing between analytic truth, which issues from the mere meanings of words, and synthetic truth, which depends on more than meanings. Now I have long maintained that this distinction is illusory. There is one step toward such a distinction, however, which does make sense: a sentence that is true by mere meanings of words should be expected, at least if it is simple, to be subscribed to by all fluent speakers in the community. Perhaps the controversial notion of analyticity can be dispensed with, in our definition of observation sentence, in favor of this straightforward attribute of community-wide acceptance.

This attribute is of course no explication of analyticity. The community would agree that there have been black dogs, yet none who talk of analyticity would call this analytic. My rejection of the analyticity notion just means drawing no line between what goes into the mere understanding of the sentences of a language and what else the community sees eye-to-eye on. I doubt that an objective distinction can be made between meaning and such collateral information as is community-wide.

Turning back then to our task of defining observation sentences, we get this: an observation sentence is one on which all speakers of the language give the same verdict when given the same concurrent stimulation.

*They are those conditioned to concurrent stimulations...*

*...but also to their own meanings*

*This leads to the analytic / synthetic distinction*

*Analytic sentences are assented to community-wide*

*Observation sentence defined via community-wide agreement*

*The status of observation sentences*

To put the point negatively, an observation sentence is one that is not sensitive to differences in past experience within the speech community.

This formulation accords perfectly with the traditional role of the observation sentence as the court of appeal of scientific theories. For by our definition the observation sentences are the sentences on which all members of the community will agree under uniform stimulation. And what is the criterion of membership in the same community? Simply general fluency of dialogue. This criterion admits of degrees, and indeed we may usefully take the community more narrowly for some studies than for others. What count as observation sentences for a community of specialists would not always so count for a larger community.

There is generally no subjectivity in the phrasing of observation sentences, as we are now conceiving them; they will usually be about bodies. Since the distinguishing trait of an observation sentence is intersubjective agreement under agreeing stimulation, a corporeal subject matter is likelier than not.

The old tendency to associate observation sentences with a subjective sensory subject matter is rather an irony when we reflect that observation sentences are also meant to be the intersubjective tribunal of scientific hypotheses. The old tendency was due to the drive to base science on something firmer and prior in the subject's experience; but we dropped that project.

The dislodging of epistemology from its old status of first philosophy loosed a wave, we saw, of epistemological nihilism. This mood is reflected somewhat in the tendency of Polányi, Kuhn, and the late Russell Hanson to belittle the role of evidence and to accentuate cultural relativism. Hanson ventured even to discredit the idea of observation, arguing that so-called observations vary from observer to observer with the amount of knowledge that the observers bring with them. The veteran physicist looks at some apparatus and sees an x-ray tube. The neophyte, looking at the same place, observes rather "a glass and metal instrument replete with wires, reflectors, screws, lamps, and pushbuttons."<sup>6</sup> One man's observation is another man's closed book or flight of fancy. The notion of observation as the impartial and objective source of evidence for science is bankrupt. Now my answer to the x-ray example was already hinted a little while back: what counts as an observation sentence

6. N. R. Hanson, "Observation and Interpretation," in S. Morgenbesser (ed.), *Philosophy of Science Today* (New York: Basic Books, 1966).

varies with the width of community considered. But we can also always get an absolute standard by taking in all speakers of the language, or most.<sup>7</sup> It is ironical that philosophers, finding the old epistemology untenable as a whole, should react by repudiating a part which has only now moved into clear focus.

Clarification of the notion of observation sentence is a good thing, for the notion is fundamental in two connections. These two correspond to the duality that I remarked upon early in this lecture: the duality between concept and doctrine, between knowing what a sentence means and knowing whether it is true. The observation sentence is basic to both enterprises. Its relation to doctrine, to our knowledge of what is true, is very much the traditional one: observation sentences are the repository of evidence for scientific hypotheses. Its relation to meaning is fundamental too, since observation sentences are the ones we are in a position to learn to understand first, both as children and as field linguists. For observation sentences are precisely the ones that we can correlate with observable circumstances of the occasion of utterance or assent, independently of variations in the past histories of individual informants. They afford the only entry to a language.

The observation sentence is the cornerstone of semantics. For it is, as we just saw, fundamental to the learning of meaning. Also, it is where meaning is firmest. Sentences higher up in theories have no empirical consequences they can call their own; they confront the tribunal of sensory evidence only in more or less inclusive aggregates. The observation sentence, situated at the sensory periphery of the body scientific, is the minimal verifiable aggregate; it has an empirical content all its own and wears it on its sleeve.

The predicament of the indeterminacy of translation has little bearing on observation sentences. The equating of an observation sentence of our language to an observation sentence of another language is mostly a matter of empirical generalization; it is a matter of identity between the range of stimulations that would prompt assent to the one sentence and the range of stimulations that would prompt assent to the other.<sup>8</sup>

7. This qualification allows for occasional deviants such as the insane or the blind. Alternatively, such cases might be excluded by adjusting the level of fluency of dialogue whereby we define sameness of language. (For prompting this note and influencing the development of this paper also in more substantial ways, I am indebted to Burton Dreben.)

8. See Quine, *Word and Object*, pp. 31–46, 68.

*But there is a privileged class of observation sentences!*

*We need to distinguish observation sentences from the rest to protect the notion of scientific evidence and to explain language acquisition*

*Observation sentences are meaningful in isolation from the rest of sentences in a language*

*Criterion of community membership*

*Is every observation theory-laden?*



It is no shock to the preconceptions of old Vienna to say that epistemology now becomes semantics. For epistemology remains centered as always on evidence, and meaning remains centered as always on verification; and evidence is verification. What is likelier to shock preconceptions is that meaning, once we get beyond observation sentences, ceases in general to have any clear applicability to single sentences; also that epistemology merges with psychology, as well as with linguistics.

This rubbing out of boundaries could contribute to progress, it seems to me, in philosophically interesting inquiries of a scientific nature. One possible area is perceptual norms. Consider, to begin with, the linguistic phenomenon of phonemes. We form the habit, in hearing the myriad variations of spoken sounds, of treating each as an approximation to one or another of a limited number of norms—around thirty altogether—constituting so to speak a spoken alphabet. All speech in our language can be treated in practice as sequences of just those thirty elements, thus rectifying small deviations. Now outside the realm of language also there is probably only a rather limited alphabet of perceptual norms altogether, toward which we tend unconsciously to rectify all perceptions. These, if experimentally identified, could be taken as epistemological building blocks, the working elements of experience. They might prove in part to be culturally variable, as phonemes are, and in part universal.

Again there is the area that the psychologist Donald T. Campbell calls evolutionary epistemology.<sup>9</sup> In this area there is work by Hüseyin Yilmaz, who shows how some structural traits of color perception could have been predicted from survival value.<sup>10</sup> And a more emphatically epistemological topic that evolution helps to clarify is induction, now that we are allowing epistemology the resources of natural science.<sup>11</sup>

9. D. T. Campbell, "Methodological Suggestions from a Comparative Psychology of Knowledge Processes," *Inquiry* 2 (1959), pp. 152–182.

10. Hüseyin Yilmaz, "On Color Vision and a New Approach to General Perception," in E. E. Bernard and M. R. Kare (eds.), *Biological Prototypes and Synthetic Systems* (New York: Plenum, 1962); "Perceptual Invariance and the Psychophysical Law," *Perception and Psychophysics* 2 (1967), pp. 533–538.

11. See "Natural Kinds," in Quine, *Ontological Relativity*.

# 16

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## NATURALISM; OR, LIVING WITHIN ONE'S MEANS

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Names of philosophical positions are a necessary evil. They are necessary because we need to refer to a stated position or doctrine from time to time, and it would be tiresome to keep restating it. They are evil in that they come to be conceived as designating schools of thought, objects of loyalty from within and objects of obloquy from without, and hence obstacles, within and without, to the pursuit of truth.

In identifying the philosophical position that I call naturalism, then, I shall just be describing my own position, without prejudice to possibly divergent uses of the term. In *Theories and Things* I wrote that naturalism is "the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described"; again that it is "abandonment of the goal of a first philosophy prior to natural science" (pp. 21, 67). These characterizations convey the right mood, but they would fare poorly in a debate. How much qualifies as "science itself" and not "some prior philosophy"?

In science itself I certainly want to include the farthest flights of physics and cosmology, as well as experimental psychology, history, and the social sciences. Also mathematics, insofar as it is applied, for it is indispensable to natural science. What then am I excluding as "some prior philosophy," and why? Descartes' dualism between mind and body is called metaphysics, but it could as well be reckoned as science, however false. He even had a causal theory of the interaction of mind and body through the pineal gland. If I saw indirect explanatory benefit in positing sensibilia, possibilia, spirits, a Creator, I would joyfully accord them scientific status too, on a par with such avowedly scientific posits as quarks and black holes. What then *have* I banned under the name of prior philosophy?

*Naturalism:  
Reality to be  
described in science  
alone*

*Math is included  
in science*

Demarcation is not my purpose. My point in the characterizations of naturalism that I quoted is just that the *most* we can reasonably seek in support of an inventory and description of reality is testability of its observable consequences in the time-honored hypothetico-deductive way—whereof more anon. Naturalism need not cast aspersions on irresponsible metaphysics, however deserved, much less on soft sciences or on the speculative reaches of the hard ones, except insofar as a firmer basis is claimed for them than the experimental method itself.

Where naturalistic renunciation shows itself most clearly and significantly is in naturalistic epistemology. Various epistemologists, from Descartes to Carnap, had sought a foundation for natural science in mental entities, the flux of raw sense data. It was as if we might first fashion a self-sufficient and infallible lore of sense data, innocent of reference to physical things, and then build our theory of the external world somehow on that finished foundation. The naturalistic epistemologist dismisses this dream of prior sense-datum language, arguing that the positing of physical things is itself our indispensable tool for organizing and remembering what is otherwise, in James' words, a "blooming, buzzing confusion."

To account for knowledge of an external thing or event, accordingly, the naturalistic epistemologist looks rather to the external thing or event itself and the causal chain of stimulation from it to one's brain. In a paradigm case, light rays are reflected from the object to one's retina, activating a patch of nerve endings, each of which initiates a neural impulse to one or another center of the brain. Through intricate processes within the brain, finally, and abetted by imitation of other people or by instruction, a child comes in time to utter or assent to some rudimentary sentence at the end of such a causal chain. I call it an observation sentence. Examples are "It's cold," "It's raining," "(That's) milk," "(That's a) dog."

Customarily the experimental psychologist chooses one or another object or event, from somewhere along such a causal chain, to represent the chain, and this he calls the stimulus. Usually it is an event of his own devising. In one experiment it will be a flash or a buzz in the subject's vicinity, and in another it will be an ice cube or a shock at the subject's surface. For our more general purposes, not linked to any particular experiment, an economical strategy in defining the stimulus is to intercept the causal chains just at the subject's surface. Nothing is lost, for it is only from that point inward that the chains contribute to the subject's knowledge of the external world.

Indeed, even what reaches the subject's surface is relevant only if it triggers neural receptors. So we might for our purposes simply identify the subject's stimulus, over a given brief moment, with the temporally ordered set of sensory receptors triggered in that moment.

Still further economy might be sought by intercepting the causal chains rather at a deeper level—somewhere within the brain; for even the surface receptors that are triggered on any given occasion are largely without relevant effect on the subject's behavior. However, our knowledge of these deeper levels is still too sketchy. Moreover, as research increasingly penetrates these depths, we become aware of complexity and heterogeneity radically at variance with the neat simplicity at the surface. Each receptor, after all, admits of just two clean-cut states: triggered or not.

Moreover, the behaviorally irrelevant triggerings in a global stimulus can be defined out anyway, in due course, by appeal to perceptual similarity of stimuli. The receptors whose firing is *salient* in a given stimulus are the ones that it shares with all perceptually similar stimuli. Perceptual similarity itself can be measured, for a given individual, by reinforcement and extinction of responses.

So it seems best for present purposes to construe the subject's stimulus on a given occasion simply as his global neural intake on that occasion. But I shall refer to it only as neural intake, not stimulus, for other notions of stimulus are wanted in other studies, particularly where different subjects are to get the same stimulus. Neural intake is private, for subjects do not share receptors.

Perceptual similarity, then, is a relation between a subject's neural intakes. Though testable, it is a private affair; the intakes are *his*, and are perceptually more or less similar for *him*. Perceptual similarity is the basis of all learning, all habit formation, all expectation by induction from past experience; for we are innately disposed to expect similar events to have sequels that are similar to each other.

The association of observation sentences with neural intakes is many-many. Any one of a range of perceptually fairly similar intakes may prompt the subject's assent to any one of a range of semantically kindred sentences. But in contrast to the privacy of neural intakes, and the privacy of their perceptual similarity, observation sentences and their semantics are a public matter, since the child has to learn these from her elders. Her learning then depends indeed both on the public currency of the observation sentences and on a preestablished harmony of people's private scales of perceptual similarity. The harmony is formal, in this

*No sense-data language*

*Knowledge of reality is explained by looking at the workings of the brain*

*Stimulus constitutes the causal chain before it reaches the body's surface*

*Stimulus is to be associated with total neural intake*

*Perceptual similarity, cashed in terms of neural intakes, is the basis of all learning and inductive inferences*

sense: if a witness finds the first of three scenes less similar to the second than to the third, another witness is apt to do likewise. This approximate harmony is preestablished in a shared gene pool. Different people's feelings still might not match, whatever that might mean.

This much is a naturalistic analogue or counterpart of the traditional epistemologist's phenomenalist foundation in sense data. However, it pretends to plausibility in psychology, in genetics, and even in prehistory. Observation sentences have their antecedents in birdcalls and in the signal cries of the apes.

Building on this naturalistic foundation, then, in parallel to the old epistemologist's proposed construction of science on a foundation of sense data, the naturalist would venture a psychologically and historically plausible sketch of the individual's acquisition of science and perhaps the evolution of science down the ages, with an eye primarily to the logic of evidence. I will spare you most of that, for I have gone into it in *Word and Object* and better in *The Roots of Reference, Pursuit of Truth*, and elsewhere. There are just a couple of aspects that I want to remind you of.

One is reification, or the positing of objects. Observation sentences commonly contain words that refer to objects when used in mature discourse, but the infant first acquires such a sentence only as a seamless whole, conditioned—like the signal cry of the ape—to an appropriate range of global neural intakes. But there is a harbinger of reification already in our innate propensity, and that of other animals, to confer salience on those components of a neural intake that transmit corporeal patches of the visual field. It is what Donald Campbell calls our innate reification of bodies, but I construe reification rather in degrees. Special ways of compounding observation sentences mark further steps in the reification of bodies, and the job is complete only when the speaker has mastered past and future tense and knows about the unseen but continuous translation of an identical body through space between observations. It is only then that she makes sense of a body's being the same body from one observation to another despite intervening changes in appearance.

At that point the reification of bodies is full fledged. Reification of less conspicuous objects, notably abstract ones such as numbers and classes, takes further explaining, and admits of it. A crucial step there, as I see it, is mastery of relative clauses and pronouns.

This burgeoning language of science is a direct extension of the falter-

ing language of observation. Segments of observation sentences carry over and become—some of them—terms for objects. Conversely, sentences learned only later by grammatical synthesis from a sophisticated vocabulary can come to qualify as observation sentences as well. For, what I take as definitive of observation sentences is just this pair of conditions: first, the speaker must be disposed to assent to the sentence or dissent from it outright on making the appropriate observation, irrespective of his interrupted line of thought if any, and second, the verdict must command the agreement of any witnesses from the appropriate language community. This second requirement, intersubjectivity, is needed in order that the child be able to learn observation sentences from his elders; and those sentences, some of them, are his indispensable entering wedge in acquiring cognitive language. Intersubjectivity of observation sentences is likewise essential at the other end, to assure objectivity of science.

The sharing of vocabulary by observation sentences and sentences of science was necessary not only for the emergence of scientific language; it is necessary also as a channel for the empirical testing of scientific hypotheses. The primordial hypotheses are what I call observation categoricals, compounded of pairs of observation sentences: thus "When it snows, it's cold." To check such a hypothesis experimentally, we contrive to put ourselves in a situation where the first component, "It's snowing," is observably fulfilled, and then we check for fulfillment of the second component. If it is fulfilled, the categorical remains standing until further notice. If it is not fulfilled, the categorical is refuted once for all.

I see this as the key to the empirical testing also of more sophisticated hypotheses. We conjoin the hypothesis in question to a set of already previously accepted statements, sufficient together to imply some observation categorical that was not implied by the previous set alone. Then we check the observation categorical.

The appeal to logical implication here presents no problem. The basic laws of logic are internalized in learning the use of the logical particles. For instance, the child learns by observation and parental correction that it is misuse of the conjunction "and" to affirm an "and" compound and then deny one of the components. The child has thus internalized one simple logical implication, namely that an "and" compound implies its components, on pain of simply getting a word wrong. Correspondingly for other basic implications, up to and including the laws of quantifiers and identity. Insofar I am with Lauener in recognizing analyticity.

Observation sentences, naturalistically construed, are analogues of sense-data

Reification naturalistically construed

Science is an extension of observation

Universal assent and intersubjectivity characterise observation

Observation sentences provide for learning and testability

Scientists of course do not trace all these links of implication from hypothesis to observation categorical. It would mean filling in all the logically requisite supporting statements, most of which are so familiar to him or so trivial as to go without saying. In practice, moreover, many tacit premisses often express mere statistical trends or probabilities, which he will take in stride unless unexpected results prompt him to reconsider.

Still the deduction and checking of observation categoricals is the essence, surely, of the experimental method, the hypothetico-deductive method, the method, in Popper's words, of conjecture and refutation. It brings out that prediction of observable events is the ultimate test of scientific theory.

*Purposes of science* I speak of test, not purpose. The purpose of science is to be sought rather in intellectual curiosity and technology. In our prehistoric beginnings, however, the purpose of the first glimmerings of scientific theory *was* presumably prediction, insofar as purpose can be despiritualized into natural selection and survival value. This takes us back to our innate sense or standard of perceptual similarity, and the innate expectation that similars will have mutually similar sequels. In short, primitive induction.

*Natural selection accounts for prediction* Prediction is verbalized expectation. Conditional expectation, when correct, has survival value. Natural selection has accordingly favored innate standards of perceptual similarity that have harmonized with trends in our environment. Natural science, finally, is conditional expectation hypertrophied.

I said that prediction is not the main purpose of science, but only the test. It is a negative test at that, a test by refutation. As a further disavowal let me add, contrary to positivism, that a sentence does not even need to be testable in order to qualify as a respectable sentence of science. A sentence is testable, in my liberal or holistic sense, if adding it to previously accepted sentences clinches an observation categorical that was not implied by those previous sentences alone; but much good science is untestable even in this liberal sense. We believe many things because they fit in smoothly by analogy, or they symmetrize and simplify the overall design. Surely much history and social science is of this sort, and some hard science. Moreover, such acceptations are not idle fancy; their proliferation generates, every here and there, a hypothesis that can indeed be tested. Surely this is the major source of testable hypotheses and the growth of science.

The naturalization of epistemology, as I have been sketching it, is both a limitation and a liberation. The old quest for a foundation for natural science, firmer than science itself, is abandoned: that much is the limitation. The liberation is free access to the resources of natural science, without fear of circularity. The naturalistic epistemologist settles for what he can learn about the strategy, logic, and mechanics by which our elaborate theory of the physical world is in fact projected, or might be, or should be, from just that amorphous neural intake.

Is this sort of thing still philosophy? Naturalism brings a salutary blurring of such boundaries. Naturalistic philosophy is continuous with natural science. It undertakes to clarify, organize, and simplify the broadest and most basic concepts, and to analyze scientific method and evidence within the framework of science itself. The boundary between naturalistic philosophy and the rest of science is just a vague matter of degree.

Naturalism is naturally associated with physicalism, or materialism. I do not equate them, as witness my earlier remark on Cartesian dualism. I do embrace physicalism as a scientific position, but I could be dissuaded of it on future scientific grounds without being dissuaded of naturalism. Quantum mechanics today, indeed, in its neoclassical or Copenhagen interpretation, has a distinctly mentalistic ring.

My naturalism has evidently been boiling down to the claim that in our pursuit of truth about the world we cannot do better than our traditional scientific procedure, the hypothetico-deductive method. A rebuttal suggests itself here: surely mathematicians. The obvious defense against that rebuttal is to say that mathematical truths are not about the world. But this is not a defense of my choosing. In my view applied mathematics *is* about the world.

Thus consider again a case where we are testing a scientific hypothesis by conjoining it to some already accepted statements and deducing an observation categorical. Likely as not, some of those already accepted statements are purely mathematical. This is how pure mathematics gets applied. Whatever empirical content those already accepted statements can claim, then, from being needed in implying the observation categorical, is imbibed in particular by the mathematical ones.

Thus it is that I am inclined to blur the boundary between mathematics and natural science, no less than the boundary between philosophy and natural science. If it is protested that proved mathematical truths are not subject to subsequent refutation, my answer is that we safeguard them by choosing to revoke non-mathematical statements instead,

*The project of naturalised epistemology reviewed*

*Physicalism is just another scientific hypothesis*

*Mathematics naturalised*

*It has empirical content by virtue of its use in scientific theories*

*Mathematics is irrefutable by our own (reasonable) choice*



in cases where a set of statements has been found conjointly to imply a false observation categorical. Reasons can be adduced for doing so; but enough.

That leaves open the vast proliferations of mathematics that there is no thought or prospect of applying. I see these domains as integral to our overall theory of reality only on sufferance: they are expressed in the same syntax and lexicon as applicable mathematics, and to exclude them as meaningless by *ad hoc* gerrymandering of our syntax would be thankless at best. So it is left to us to try to assess these sentences also as true or false, if we care to. Many are settled by the same laws that settle applicable mathematics. For the rest, I would settle them as far as practicable by considerations of economy, on a par with the decisions we make in natural science when trying to frame empirical hypotheses worthy of experimental testing.

Traditional epistemology was in part normative in intent. Naturalistic epistemology, in contrast, is viewed by Henri Lauener and others as purely descriptive. I disagree. Just as traditional epistemology on its speculative side gets naturalized into science, or next of kin, so on its normative side it gets naturalized into technology, the technology of scientizing.

What might be offered first of all as a norm of naturalized epistemology is *prediction of observation* as a test of a hypothesis. I think of this as more than a norm: as the name of the game. Science cannot all be tested, and the softer the science the sparser the tests; but when it *is* tested, the test is prediction of observation. Moreover, naturalism has no special claims on the principle, which is rather the crux of empiricism.

What are more distinctively naturalistic and technological are norms based on scientific findings. Thus science has pretty well established—subject to future disestablishment, as always—that our information about distant events and other people reaches us only through impact of rays and particles on our sensory receptors. A normative corollary is that we should be wary of astrologers, palmists, and other soothsayers. Think twice about E.S.P.

For a richer array of norms, vague in various degrees, we may look to the heuristics of hypothesis: how to think up a hypothesis worth testing. This is where considerations of conservatism and simplicity come in, and, at a more technical level, probability theory and statistics. In practice those technical matters spill over also, as I remarked, to complicate the hypothetico-deductive method itself.

I said at the beginning of this paper that according to naturalism is it within science itself and not some prior philosophy that reality is to be identified. Farther along in a more narrowly scientific spirit, I speculated on how we round out our recognition of objects *as* objects, bit by bit, with our acquisition of language and science. These matters call now for some more broadly philosophical reflections.

Let us recall, to begin with, that the association of observation sentences with neural intake is holophrastic. What objects the component words may designate in other contexts is irrelevant to the association. This is obviously so if the observation sentence is to be acquired as a first step in language learning; but the association is equally direct and holophrastic in its operation even if the sentence was acquired through synthesis of its words, and gained its immediacy only through subsequent familiarization.

Moreover, the specifics of designation and denotation are not only indifferent to the association of observation sentences to neural intake; they are indifferent also to the implication of observation categoricals by scientific theory. It is logical implication; and logic, unlike set theory and the rest of mathematics, responds to no traits of objects beyond sameness and difference. So we must conclude that objects of any sort figure only as neutral nodes in the structure of scientific theory, so far as empirical evidence is concerned. We can arbitrarily change the values of our variables, the designata of our names, and the denotata of our predicates without disturbing the evidence, so long anyway as the new objects are explicitly correlated one to one with the old. Such is the indeterminacy of reference, as I have come to call it.

At first it is perhaps alarming. We are left with no basis, it would seem, for judging whether we are talking about familiar things or some arbitrary proxies. The shock subsides, however, when we reflect on a homely example or two. Thus think of a body in the scientific framework of space and time. Insofar as you specify the precise sinuous filament of four-dimensional space-time that the body takes up in the course of its career, you have fixed the object uniquely. We could go farther and *identify* the object, a chipmunk perhaps, with its portion of space-time, thus saying that it is tiny at its early end and bigger at its late end. The move is artificial, but actually it confers a bit of economy, if we are going to have the space-time anyway. Subjective connotations of brownness, softness, swift and erratic movement, and the rest simply carry over. Surely all matters of evidence remain undisturbed. We are even prepared to say

*Ontological debate*

*Association of observation sentences with neural input is not unique across different contexts*

*Indeterminacy of reference*

*The status of non-applied mathematics*

that it was what a body was all along, an appropriately filled-in portion of space-time as over against empty ones.

Next we might identify space-time regions in turn with the sets of quadruples of numbers that determine them in some arbitrarily adopted frame of coordinates. We can transfer sensory connotations now to this abstract mathematical object, and still there is no violence to scientific evidence. To speak intuitively, nothing really happened.

Thus we can come to terms somewhat with the indeterminacy of reference, as applied to bodies and other sensible substances, by just letting the sensory connotations of the observation sentences carry over from the old objects to their proxies.

In the case of abstract objects such as numbers, devoid of sensory connotations, the indeterminacy of reference is already familiar. It is seen in Frege's so-called Caesar problem: the number five may be Julius Caesar. We happily use numbers without caring whether they be taken according to the Frege-Russell constructions or Ackermann's or von Neumann's. The point was dramatized long ago by F.P. Ramsey with his expedient of Ramsey sentences, as they have come to be called. Instead of invoking the abstract objects specifically, when certain of their properties are needed in an argument, the Ramsey sentence just says that there *are* objects with the properties, and then invokes the objects by variables without further identification. This expedient only works for abstract objects, however, used as auxiliaries here and there without regard to whether they remain the same objects from one context to another.

The indeterminacy of reference can be seen again in its full generality, as Davidson once remarked, by an examination of Tarski's classical truth definition. If a sentence comes out true under that definition, it continues to do so when objects are reassigned to its predicates in any one-to-one way.

These reflections on ontology are a salutary reminder that the ultimate data of science are limited to our neural intake, and that the very notion of object, concrete or abstract, is of our own making, along with the rest of natural science and mathematics. It is our overwhelmingly ingenious apparatus for systematizing, predicting, and partially controlling our intake, and we may take pride.

This conventionalist view of ontology appeals, I expect, to Henri Lauener. He in his pragmatism even settles for a plurality of scientific specialties, each with its working ontology, and no dream of an overarching, unifying fact of the matter.

Naturalism itself is noncommittal on this question of unity of science. Naturalism just sees it as a question within science itself, albeit a question more remote from observational checkpoints than the most speculative questions of the hard and soft sciences ordinarily so called.

Naturalism can still respect the drive, on the part of some of us, for a unified, all-purpose ontology. The drive is typical of the scientific temper, and of a piece with the drive for simplicity that shapes scientific hypotheses generally. Physicalism is its familiar manifestation, and physicalism is bound to have had important side effects in the framing of more special hypotheses in various branches of science; for physicalism puts a premium on hypotheses favorable to closer integration with physics itself. We have here a conspicuous case of what I touched on earlier: scientific hypotheses which, though not themselves testable, help to elicit others that are.

In any event, we are now seeing ontology as more utterly a human option than we used to. We are drawn to Lauener's pragmatism. Must we then conclude that true reality is beyond our ken? No, that would be to forsake naturalism. Rather, the notion of reality is itself part of the apparatus; and sticks, stones, atoms, quarks, numbers, and classes all are utterly real denizens of an ultimate real world, except insofar as our present science may prove false on further testing.

What then is naturalism's line on truth and falsity themselves? The truth predicate raises no problem in its normal daily use as an instrument of what I have called semantic ascent. Tarski's disquotational account accommodates it, so long as what are called true are sentences in our own language; and we then extend the predicate to sentences of other languages that we accept as translations of truths of our own. However, paradoxes arise when the truth predicate is applied to sentences that contain that very predicate or related ones; so we are called upon to recognize rather a hierarchy of truth predicates, each of which behaves properly only in application to sentences that do not contain that predicate itself or higher ones. It is a hierarchy of better and better truth predicates but no best. In practice, except in contexts such as these philosophical ones, occasions seldom arise for venturing above the first rung of the ladder. Truth *off* the hierarchy, absolute truth, would indeed be transcendent; bringing it down into scientific theory of the world engenders paradox. So naturalism has no place for that.

Still, our concept of truth strains at its naturalistic moorings in another way. We naturalists say that science is the highest path to truth, but still we do not say that everything on which scientists agree is true. Nor

*Unified ontology possible*

*Transcendent reality dismissed*

*Objecthood is of our own making*

do we say that something that was true became false when scientists changed their minds. What we say is that they and we *thought* it was true, but it wasn't. We have scientists pursuing truth, not decreeing it. Truth thus stands forth as an ideal of pure reason, in Kant's apt phrase, and transcendent indeed. On this score I am again with Lauener.

C.S. Peirce tried to naturalize truth by identifying it with the limit that scientific progress approaches. This depends on optimistic assumptions, but if we reconstrue it as mere metaphor it does epitomize the scientists' persistent give and take of conjecture and refutation. Truth as goal remains the established usage of the term, and I acquiesce in it as just a vivid metaphor for our continued adjustment of our world picture to our neural intake. Metaphor is perhaps a handy category in which to accommodate transcendental concepts from a naturalist point of view.