**Quine and Naturalism**

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**Introduction**

Willard Van Ormond Quine is rightly regarded as the pivotal figure in the rise of philosophical naturalism in the second half of the twentieth century. Looking back at his work from a contemporary perspective, however, it is striking how far his assumptions and concerns diverge from those of present-day naturalists. In this paper I shall explore these differences, hoping thereby to cast light both on the historical Quine and on contemporary naturalism.

I shall consider three areas where the approaches adopted by Quine and present-day naturalism diverge significantly: first, the nature of philosophy and its relation to science; second, the analytic-synthetic distinction and the rejection of a priori knowledge; third, physicalism.

Before proceeding, let me briefly say something about the concept of philosophical naturalism itself. At first pass, we can think of philosophical naturalism as endorsing an affinity between philosophy and science. This then carries with it two more specific ideas. First, “methodological naturalism”: philosophy has the same aims and methods as science, seeking to establish synthetic truths by appeal to a posteriori evidence. Second, “ontological naturalism”: philosophy should invoke no entities in its analyses beyond those uncovered by scientific investigation. Beyond that, however, it is hard to be specific. As I have said elsewhere, it is pointless to try to lay down necessary and sufficient conditions for a philosopher or doctrine to qualify as “naturalist” (Papineau 2021). There are too many issues, too many options, and too many reasons for embracing or rejecting the label “naturalist” for it to be possible to settle on any precise meaning. Fortunately, we will have no need of a precise definition in what follows. My aim in what follows is to compare Quine with those contemporary philosophers who would normally be classified as “naturalists”. For this purpose a general grasp of the term will be adequate. Our focus will be on the comparison, not on the rationale for the terminology.

**The Nature of Philosophy**

*Beyond First Philosophy*

Let us start with Quine’s rejection of “first philosophy”. This is certainly one issue on which he coincides fully with contemporary naturalist thought. Philosophy should not set itself the task of vindicating our knowledge of the world from some prior vantage point. Rather it should help itself to the findings of science in addressing its problems. If this leads to any correction or improvement of science, this is a move within science. We are in Neurath’s boat, repairing science from within, using the resources that science itself provides (*Word and Object* 1960a section 1).

Still, while Quine’s official theory is that philosophy should help itself to the discoveries of science, there is surprisingly little of this is his actual practice. Contemporary naturalist philosophers draw freely on all matter of scientific findings. In their work we standardly find appeal to resources supplied by psychology, linguistics, neuroscience, social science, biology, evolutionary theory, and so on. But Quine’s writings are far more traditional. A reader of Quine’s philosophical work will be hard put to find definite claims that rest on any authority not available from the philosophical armchair. It is only at one specific point that Quine feels that he must appeal to the resources of empirical science. After that there is scarcely anything that distinguishes his approach from “first philosophy”.

Let me elaborate these points more slowly. We can begin with Quine’s approach to what he considers the central task of philosophy, namely, explaining how humans arrive at knowledge of the world they live in. As Quine saw it, humans start with the deliverances of their sense organs, and then proceed on this basis to develop an understanding of the empirical world, initially in the form of common sense, and then later with the help of systematic science. The classic challenge facing philosophy, said Quine, is to explain how this is possible. As he put it, “The relation between the meagre input and the torrential output is one we are prompted to study for somewhat the same reasons that always prompted epistemology; namely in order to see how evidenced relates to theory, and in what way one’s theory of nature transcends any available evidence” (“Epistemology Naturalized” 1969 83).

In pursuing this epistemological project, Quine was very much following the path set out by his philosophical mentor Rudolf Carnap, whose *Der Logische Aufbau Der Welt* (1928) sought to show how all everyday knowledge and subsequent science can be ultimately grounded in observation sentences about the deliverances of conscious sensory perception. Carnap’s project, of course, was squarely in the tradition of “first philosophy”. Science was grounded in the construction of the world, not vice versa. But Carnap’s project struggled to launch itself. His construction called for an “observation language” that directly describes what is given in conscious experience. Carnap ran into a series of obstacles in constructing this language.

Quine came to view these obstacles as insuperable. In his view, the trouble arose because the most basic observation terms of everyday human language refer, not to sense data, but to ordinary physical objects, like table and trees and stones. As such, ordinary observation language is already committed to matters that transcend our sensory inputs. In effect, said Quine, observation language posits an external world of objects precisely in order to help us anticipate and manipulate sensory stimuli. (“Our talk of external things, our very notion of things, is just a conceptual apparatus that helps us to foresee and control the triggering of our sensory receptors in the light of previous triggering of our sensory receptors. The triggering, first and last, is all we have to go on” 1981 1.) So everyday language offers no natural or immediate way of analysing the sensory inputs that in fact provide the foundation for the construction of our common sense and scientific worlds.

This is the crucial point at which Quine felt philosophy needed to abandon “first philosophy” and to invoke science. Everyday observational descriptions of ordinary physical objects could not provide the starting point for Quine’s project. He needed to begin with the sensory triggerings themselves, not later conceptual conclusions that are built on them. So at this point he invoked the science of perception, and analysed the foundational sensory triggering in physiological terms, as events that occur when the nerve receptors in our sense organs are stimulated by physical contact with surrounding environments.

And with this move Quine is clearly breaking with the traditional project of reconstructing our world view entirely from within, as it were. The traditional epistemological project takes the deliverances of sensory consciousness as its starting point. But that won’t work for Quine, because prior to science there is no workable language for describing those deliverances. To get started on understanding how humans construct their world view, we need to step outside the boundaries of consciousness, and view human beings as themselves physical entities interacting with the physical world. Quine’s whole project can only get off the ground once it is placed within an empirical science that takes humans as among its objects.

*Sticking to Tradition*

After this specific appeal to sensory physiology, however, science plays little or no role in Quine’s story. In *Word and Object* and elsewhere he tells us how human beings progress, from sensory stimulation to observation sentences, and thence to claims made using more complex constructions, and eventually to the whole of science. And he shows us how the results might be “regimented” so as to make clear the ontological commitments involved. But all this is effectively done from Quine’s armchair, with no real appeal to scientific findings (if we put to one side the use he makes of logic). He makes no attempt to invoke developmental psychology, linguistics, perceptual science, or any other discipline that might cast light on how human actually develop their views of the world.

As I said, Quine presented himself as breaking with those “first philosophers” who seek to justify science from some privileged prior vantage point. Still, from the perspective of the 21st century present, he has an awful lot in common with this traditional enterprise. The central task of modern philosophy, from Descartes and Berkeley to Kant and the logical empiricists, has been to show how an individual’s knowledge of the world can be built up from firm perceptual foundations by a chain of secure inferences. Quine stands squarely in this Cartesian tradition. He might appeal to some elements of science in the course of his story, and thereby render his account not obviously justificatory, but it is still a story about how science can be constructed by inferences from sensory starting points.

It is illuminating to compare Quine’s response to the question “How have we arrived at our body of established knowledge?” with that of contemporary philosophers with naturalist inclinations. For a start, the contemporary philosophers would no doubt invoke many aspects of psychological science ignored by Quine—the detailed workings of our perceptual systems, the built-in presuppositions about the world we inherit from our evolutionary forbears courtesy of our developmental programmes, and so on. Moreover, alongside this appeal to psychological science we would also find an interest in social structures that have little to do with individual psychology. From a contemporary perspective, social institutions are as important to scientific knowledge as any individual reasoning. So a contemporary naturalist account of knowledge would invoke scientific testimony, peer review and reproducibility, instruments, sources of data, statistics, forms of inference in different disciples, and so on. Unaided human observations and other facets of individual psychology would only be one tiny part of a far larger story.

From a modern perspective, it is difficult to avoid impression that Quine is less than fully committed to the possibilities offered by philosophical naturalism. Contemporary naturalism views humans as items *within* the natural realm—indeed as items that are not particularly significant from a cosmic perspective, though of course very interesting to us. Given this, contemporary naturalism sees no reason not to bring the full resources of science to the study of human beings, including the study of how they find out about the world.

By contrast, Quine seems interested in humans primarily as the *source* of our picture of the world. He wants to understand how we construct our world using only the resources provided by our sensory inputs. And from this perspective humans stand almost entirely outside the world they have constructed—they are its creators rather than its denizens. True, Quine does explicitly breach this transcendental wall, so to speak, when he insists that we have to view our epistemological enterprise itself from within science, at the point where we identify our “sensory triggerings” as physical processes. And this breach then opens the way to a whole-heartedly naturalized account of our scientific endeavours. But Quine moves scarcely any distance in this direction. Once he has initially appealed to science to understand human observation, his further explanation of our theorising is in terms of an individual subject’s internal mental powers, very much in the style of any classical epistemologist.

*Reliabilism, Truth and Realism*

Quine’s focus on the construction of the “world” by humans—rather than on humans as inhabitants of the world—is no doubt part of the explanation for another striking way in which he diverges from present-day naturalist thinking about knowledge. Any contemporary naturalist account of knowledge is likely to lay stress on the *reliable truth-conduciveness* of sources of scientific knowledge. In explaining how scientific knowledge is arrived at, it would cite the way that not only individual perception, but also institutional accreditation of results, instrumentation, special forms of reasoning, and many other scientific practices are well-designed to deliver truths that would not otherwise be available.

It is a striking feature of Quine’s version of “epistemology naturalized” that this kind of reliabilist evaluation of the sources of scientific knowledge seems to play no role. In the original paper with that title there is no mention of evaluation, reliabilist or otherwise. When later challenged on this issue of normativity, he retorted in what initially looks like a reliabilist spirit “For me normative epistemology is a branch of engineering. It is the technology of truth-seeking, . . . “. But he quickly backtracked and watered down the reference to truth-seeking by “. . . or, in a more cautious epistemological term, prediction” (1986 665). In line with this, none of the detailed analysis of “Epistemology Naturalized”, or of any of Quine’s other writings, contains any explicit reliabilist evaluations. The construction is all viewed from within, as it were, with no suggestion that we might step outside to check whether the procedures being used are well-suited to guide us to the truth.

Why does Quine not explore the possibilities of reliabilist naturalized epistemology in the contemporary style? It seems such an obvious idea.

One possible explanation that suggests itself is his disquotational theory of truth. For Quine talk of truth is no more than a device of disquotation, and involves no specifiable substantial relation between truths and truthmakers (*Philosophy of Logic* 1970a chapter 3). Perhaps such a thin notion of truth does not lend itself to reliabilist analyses.

However, it is not obvious that this is the right explanation of Quine’s apparent blindness to the possibilities of reliabilism. Modern advocates of disquotational theories of truth often stress that nothing in their approach blocks the evaluation of belief-forming process for truth-conduciveness. As they see it, they are as well-placed as anyone else to generalise about which methods of enquiry lead to true beliefs and which do not (see for example Horwich 1990 chapter 3, Beebe 2006).

I think we need to dig a bit deeper to understand Quine’s blindness to the possibilities of reliabilism. I am inclined to attribute Quine’s lack of interest in naturalism, not to his disquotationalism about truth per se, but to his use-theoretic account of meaning and consequent lack of any realist conception of truth (by which I mean a conception of truth that is independent of questions of legitimate assertibility).

Such a realist notion of truth seems a precondition of any serious epistemological reliabilism. If we are seriously to evaluate methods of acquiring beliefs for their reliability as sources of truth, then truth has better not collapse circularly into being one of the beliefs delivered by such methods. Checking whether our methods are good at delivering the beliefs they deliver is no check at all.

There is no suggestion in Quine of any realist conception of truth. As we shall see in the next section, for Quine our words have the significance they do, not because they answer to independently constituted truth conditions, but because of our dispositions to use them in response to sensory stimulations and other beliefs. Given this use-theoretic account of meaning, there is then no substantial possibility that the beliefs issuing collectively from those dispositions might turn out to be false.[[1]](#footnote-1)

In this connection, it is noteworthy that Quine has no time for “metaphysical realism”, understood as the idea that an epistemically optimal theory might in actuality be false. While he does not offer any systematic argument against this kind of global scepticism, along the lines of Davidson’s “On the Very Idea of a Conceptual Scheme” (1973), or Putnam’s model-theoretic argument (1981), it is clear enough what he thinks about the issue.

“Our scientific theory can go wrong, and precisely in the familiar way: through failure of predicted observation. But what if… we have achieved a theory that is conformable to every possible observation, past and future? In what sense could the world then be said to deviate from what the theory claims? Clearly in none . . .” (1981 22).

Some readers might be wondering whether there is not a tension between this metaphysical anti-realism and Quine’s enthusiasm for the underdetermination of theory by evidence. If two contradictory theories were both “conformable to every possible observation”, would this not lend substance to the thought that at least one will have things wrong? However, Quine’s support for the underdetermination of theory is very hedged. While he does appeal to it in arguing for the indeterminacy of translation (see especially Quine 1970b 179-81), he expresses severe doubts about its coherence of in “On Empirically Equivalent Systems of the World” (1975).

One final point before proceeding. In this subsection I have explained how Quine’s use-theoretic view of meaning inclined him against reliabilism and towards metaphysical anti-realism. Does this really show that he is at odds with present-day naturalism? After all, there are not a few contemporary philosophers who would count themselves as naturalists and yet agree with Quine that arguments from the nature of meaning can rule out metaphysical realism. We need not pause on this issue. As I said at the beginning of this paper, it is pointless to try to lay down requirements for genuine naturalism. If those who side with Quine on metaphysical realism say they are naturalists, I am happy to let that pass. Let me content myself with observing that many self-identifying contemporary naturalists do take the other side here, rejecting use-theoretic accounts of meaning and upholding reliabilism and metaphysical realism. It is striking, to say the least, that Quine’s approach to philosophy excludes these views from the start without offering any substantial arguments.

**The Analytic-Synthetic Distinction and the Rejection of A Priori Knowledge**

*Use Theory and Analyticity*

Let me now turn to questions of analytic truth and a priori knowledge. Both Quine and contemporary naturalism will agree that there are no significant analytic truths—and in particular no analytic truths that are significant enough to underpin any worthwhile philosophical enterprise of “conceptual analysis”. But I doubt that many contemporary philosophers would endorse Quine’s specific route to this conclusion.

Quine’s argument against purely analytic truths derived from his use-theoretic approach to meaning. For Quine, semantic content is breathed into our terms by our dispositions to *apply* those terms in response to perceptual experience and to other judgments. Once this this use-theoretic starting point is granted, the inseparability of analytic and synthetic theoretical assumptions soon follows. Consider any term in a language. For a use theorist, all accepted claims involving such terms will contribute to fixing their meaning. After all, those claims will all place inferential constraints on the way we use our terms in response to perceptual experience and other judgments. But of course at the same time those assumptions express our factual commitments. Quine’s use-theoretic starting point thus quickly led to the conclusion that all accepted claims have both a conventional and a factual role. (“The lore of our fathers … is a pale grey lore, black with fact and white with convention … I have found no substantial reasons for concluding that there are any quite black threads in it, or any white ones” (Quine 1960b 374; see also Quine 1951 and 1960a).)

Introductions to Quine often present the issue here as basically practical, due to the looseness of definitions. We don’t have a nice division into analytic and synthetic claims because we do not stop to stipulate which parts of our web of belief are supposed to be the revisable empirical assumptions and which the unrevisable conventional ones. If only we had taken more care with such matters, this reading implies, the analytic-synthetic distinction would be preserved. From this perspective, Quine’s main insight is that our practice has no need for the distinction and proceeds perfectly well without it.

But in truth Quine’s denial of an analytic-synthetic distinction goes far deeper than this. It is forced on him by his underlying assumption that meanings derive from dispositions to use terms, and does not depend on any assumptions about the stipulative looseness of our linguistic practice. Once this use-theoretic commitment is in place, then there is no question of separating fact from convention. No amount of terminological care would do the trick. It is the use-theory per se that undermines the analytic-synthetic distinction, not any sloppiness in our use of language.

This is because even minimal sets of dispositions for using terms will have factual presuppositions. So taking meanings to be fixed by such dispositions inevitably runs together fact and convention. Let me illustrate. Suppose that I am disposed to make judgments about *temperature* in response to readings on thermometers. This might seem to render *thermometers measure temperature* analytic and unrevisable. However, if my disposition relates to both mercury and alcohol thermometers, then it commits me to the manifestly factual and falsifiable consequence that mercury and alcohol thermometers will give the same result when simultaneously applied. So *thermometers measure temperature* turns out not to be purely analytic after all, but to contain factual as well as conventional content.

Might we not avoid mixing fact with meaning by introducing two distinct terms, *mercury temperature* and *alcohol temperature* and abjuring talk of *temperature* per se? But even that wouldn’t do the trick, since there are many numerically different mercury thermometers and alcohol thermometers, and it is still a synthetic presupposition that all the different particular thermometers will give consistent results. So if verificationists want to keep fact and convention separate they will be driven to an unworkable caricature of language which distinguishes *mercury-thermometer1 temperature* from *mercury-thermometer2 temperature* . . . This was the lesson of Percy Bridgman’s failed ‘operationalism’ from the 1920s (Bridgman 1927).

So a use-theoretic analysis of meaning effectively precludes any distinguished set of purely analytic sentences. Any workable rules for using terms will harbour substantial empirical presuppositions. In line with this, Quine emphatically rejected Carnap’s distinction between “internal” and “external” questions. Carnap envisaged alternative frameworks for describing reality, each with its own analytic linguistic rules. Within each framework we can appeal to sensory evidence and the analytic rules to answer internal questions. By contrast, the external question of which framework to adopt is not so answerable to empirical evidence, but a matter for conventional or at most pragmatic decision. Quine saw no basis for this internal-external distinction. As he saw it, decisions about analytic frameworks and decisions about synthetic claims all answer to the same overall requirements of coherence and consonance with sensory data. So for Quine our “web of belief” is an undifferentiated whole, not a conjoining of analytic definitions and synthetic claims. All assumptions are in principle revisable in response to recalcitrant sensory data, and by the same coin all our assumptions are confirmed when sensory data conform to our predictions.

A qualification is in order. As it happens, in his later work the mature Quine did not rule out analytic statements altogether. Following Putnam’s arguments in “The Analytic and The Synthetic” (1962), he was prepared to allow that “one-criterion” terms like “bachelor” could enter into analytic statements. Since the sentence “bachelors are unmarried males” exhausts what you need to know to use the term “bachelor” competently, it can happily be viewed as analytic. But this works precisely because the term “bachelor” lacks systematic import. Any theoretically interesting term, said Quine, will have multiple criteria of application (“law-cluster terms”) which will thus prevent any given statement involving it counting as analytic (1960a section 12)[[2]](#footnote-2).

Quine’s use-theoretic view of language has many strange consequences. In the hands of philosophers of science like Kuhn and Feyerabend, it gave rise to the doctrines of “meaning variance” and “incommensurability”. “Meaning variance” says that theoretical change inevitably leads to conceptual change. If you alter your theoretical assumptions involving some concept, perhaps because empirical evidence has shown that these assumptions are mistaken, then you will change your dispositions to apply that concept—and so will end up with a new concept. “Incommensurability” then quickly follows. Adherents of different theories must mean different things even when they use the same words, and so cannot communicate with each other in a common language. In the extreme case, this implies that those who reject the ontological commitments of some theory cannot use the language of that theory to convey this. If you do not accept the phlogiston theory, you cannot mean the same by “phlogiston” as the theory’s adherents, and so cannot communicate your theoretical disagreement to them by saying “There is no phlogiston”.

Quine himself was less bull-headed. He took difficulties of these kinds to undermine the whole idea of terms having definite *meanings*, and so avoided any outright implication that different views of the world force different meanings on their adherents. But he did not avoid odd consequences altogether. In place of meaning variance and incommensurability he had the “indeterminacy of translation”—the thesis that, for a radical interpreter of an alien language, different manuals of translation might be equally correct. As Quine saw it, in the absence of a rigid framework of analytic linguistic rules, there is nothing to tie down our interpretation of the utterances of speakers of an alien languages. With the possible exception of observational sentences, we can interpret their words in any number of different ways. Each interpretation will credit them with a different view of the world, yet all will account satisfactorily account for their verbal behaviour (1960a sections 7-16).

I view the indeterminacy of translation, along with meaning variance and incommensurability, not as exciting insights into the workings of language, but rather as *reductiones ad absurdum* of the use-theoretic account of meaning. They are indeed valid consequences of that account, but this does the account no credit. If that is where it leads us, we would do well to seek some alternative. (Quine himself considers whether draw this moral from his analysis, but dismisses the possibility without offering any serious argument: “Should the unwelcomeness of the conclusion [the indeterminacy of translation] 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” 1969 81.)[[3]](#footnote-3)

*Beyond Use Theory*

Despite Quine’s confident insistence, use-theoretic accounts of meaning are no longer widely accepted. This is not the place to offer any positive account of the sources of meaning, but a common theme of many current approaches to the topic, ranging from casual and informational theories to teleosemantics and success semantics, is that representational content comes first, fixed independently of the way words are used, and that usage then aims to conform to the truth conditions thus fixed. We need only think of Kripke’s examples of the causal theory of reference to see the point. The reference of names like “Aristotle”, “Gödel” and so on is fixed in ways that owe little or nothing to the ways we use the terms. We might be wrong in nearly all the descriptive claims we make about Aristotle, yet still be referring to him (Kripke 1980).

Once we move away from a use-theoretic account of meaning, Quine’s central argument against analytic truths falls away. As we shall shortly see, on non-use approaches to meaning there is no reason why the meanings of terms should be bound up with commitments to factual claims. In consequence, there is no barrier to a wide range of analyticities, and no reason why they should be restricted to “one-criterion” terms. Still, Quine’s was at least right about one thing. To the extent that analyticities exist, they are philosophical unimportant. A proper understanding of their nature shows that they play no significant role in philosophical theorising.

How exactly do analyticities arise once we move away from use theories of meaning? Not from use, of course, but rather from more or less implicit stipulations on reference. If it is laid down that the referent of some term N must satisfy some requirement R, then it will be analytic that *N, if it exists, will be R*.

Let me illustrate the idea with scientific terms that are introduced, prior to any direct contact with their referents, entirely by stipulating theoretical conditions on those referents. Plausible examples would include “Higgs boson”, “mitochondrial Eve” and “phlogiston”. The distinction between analytic and synthetic truth comes out most clearly in cases like these. I shall generalise the discussion to cover other kinds of terms at the end of this subsection.

As I observed above, on the use theory of meaning it is hard to see how we modern-day thinkers can attach the same meaning to “phlogiston” as did adherents of the phlogiston theory. They applied “emission of phlogiston” to oxidative process, and “absorbtion of phlogiston” to reductive processes, whereas we do not. However, this divergence of use doesn’t imply that we can’t *mean* the same by the term as they did. The crucial point is that, to appreciate the meaning of “phlogiston”, we only need to *understand* the theory that defines it, not *accept* it. Since we don’t accept that theory, we don’t *use* the term as its adherents did. But that doesn’t stop us understanding the theory, and in particular understanding that *phlogiston is that substance, if any, that is emitted in oxidation and absorbed in reduction*—and therewith attaching just the same meaning “phlogiston” as did the adherents of the phlogiston theory.

The apparatus of “Ramsey” and “Carnap sentences” is helpful here. The *Ramsey sentence* of a theory expresses its synthetic commitments. For example, “There is a substance emitted in oxidation and absorbed in reduction”*.* This Ramsey sentence is accepted by a theory’s adherents but not by those who reject the theory. Note that this Ramsey sentence does not itself employ any theoretical terms, but rather eliminates them. “Phlogiston” does not appear in “There is a substance emitted in oxidation and absorbed in reduction”.

It is a theory’s *Carnap sentence* that introduces and fixes a meaning for its terms. In effect, it specifies what the entities posited by the theory are to be called. Thus, the Carnap sentence for the phlogiston theory is: “If there’s a substance emitted in oxidation and absorbed in reduction, then it’s phlogiston.”

Given that Carnap sentences implicitly define their terms in this way, they render themselves analytic and devoid of any synthetic commitments. Because of this, you don’t need to accept a theory to accept its Carnap sentence. Even those who reject the phlogiston theory can accept its Carnap sentence, and therewith attach the same meanings to its terms as its adherents. They won’t *use* those terms the same way, but that doesn’t stop them grasping the conditions the theory places on its term’s referents.

I said earlier that, even if Quine was wrong to resist the analytic-synthetic distinction, he was right in thinking that analytic truths are of no substantial interest. Carnap sentences illustrate the point nicely. In implicitly defining its terms, any analytic Carnap sentence reduces itself to a logical truth. “If there’s a substance emitted in oxidation and absorbed in reduction, then it’s a substance emitted in oxidation and absorbed in reduction.” Analytic truths like these tell us nothing that we don’t already know from logic.

I have explained these issues in connection with scientific terms whose reference is fixed entirely by theoretical description. But the points I have made generalise. Consider an ordinary proper name like “Aristotle”. If Kripke is right, then the reference of this name is largely fixed by causal chains connecting current uses with Aristotle’s original acquisition of the name. Still, it is arguable that competence with this name requires at least some understanding of minimal conditions on its referent. Someone who didn’t at least appreciate that “Aristotle” is a name for a human being would arguably not have mastered the term. In line with this, we can take it that “Aristotle, if such exists, is human” is an analytic truth. At the same time, we can also see that this truth is empty, informing us of nothing except a logical truism. If human is built into the meaning of “Aristotle”, then that name is definitionally equivalent to “Aristotle, a human”, and so “Aristotle, if such exists, is human” collapses into “Aristotle, a human, if such exists, is human”.

*Semantic Indeterminacies*

I have illustrated how analytic truths arise from more or less implicit requirements imposed on referents for terms. However, it will often be unclear *which* of the generally accepted assumptions involving a term will be so implicated in its meaning. Perhaps it is semantically required that Aristotle be a human being, but it is required that he be male? It’s not clear that anything in usage will determine an answer to these questions. Why should anybody have ever exercised themselves on the matter? To the extent that nobody has ever really doubted that Aristotle was a male, why should anybody have bothered to consider what they would say in a hypothetical circumstance where someone otherwise fitting the requirements for Aristotle is not male? Given that Aristotle indeed was a male, there’s no need to resolve the indeterminacy in order to ensure that the right person will be picked out in the actual world.

The same applies to scientific terms whose reference is fixed in whole or in part by assumptions from some surrounding theory. Take the term “atom” and let us suppose that atoms are analytically specified to be small particles, not divisible by chemical means, one sort for each element. Still, what about atomic nuclei? Does contemporary physical theory make it a definitional requirement on atoms that they have nuclei, or not? Given that physicists are sure that atoms do have nuclei, it’s not clear why they should stop to answer this question. As before, the term “atom” will pick out the same actual entities whether we pack nuclei into the definition or not.

It is possible that this kind of indeterminacy is what many have in mind when they cite Quine’s repudiation of the analytic-synthetic distinction. If so, well and good. This kind of indeterminacy is widespread and genuine enough. Moreover, it is generally harmless. As I said, why should anybody exercise themselves to eliminate it, when it doesn’t make any difference to which actual-world entities get referred to? As it is sometimes put, terms of this kind are no doubt determinate in extension, even if they are indeterminate in extension. They have definite referents in actuality, even if there’s no fact of the matter about what they would pick out in various non-actual scenarios.

This kind of indeterminacy is real enough. But it is worth emphasizing that it does not signify any deficiency in the analytic-distinction itself. Note that it owes nothing to Quine’s use-theoretic view of meaning—which, as observed above, really does undermine any distinction between black factual claims and white conventional one. Rather, this indeterminacy stems solely from a looseness in definitions of specific terms, occasioned, as I have explained, by the lack of any need to be more precise. Given this, it seems wrong to attribute this indeterminacy to some unclarity in the contrast between analytic and synthetic claims. If some terms don’t have definite meanings, it’s only to be expected that it be indeterminate which statements involving them are analytic. That doesn’t show there is anything wrong with the general idea of a claim being true solely in virtue of its meaning, given logic. It just shows that there is something lacking in the meanings of certain specific terms.

In some cases semantic indeterminacies of the kind at issue turn out not to be inconsequential after all. Sometimes firmly held assumptions prove to be flawed, and then problems about actual usage can arise. For example, until the nineteenth century it was universally supposed that straight lines are not only the shortest distance between two points, but also that they respect the axioms of Euclidean geometry. In consequence, for the reasons I have explained, nobody exercised themselves as to whether or not both these criteria were built into the meaning of “straight line”. The same items, they supposed, would be picked out either way. But then it turned out that lines satisfying the first criterion do not generally satisfy the second. And so then there was an issue to address. Was it right to say that "there are no straight lines in physical space", or rather that "there are physically straight lines, but they are not Euclidean"? Previous usage, I would say, left it open. Yet a decision was needed, lest different scientists end up contradicting each other for no good reason.

Examples can be multiplied. If heat transport does not involve fluid motion, does that mean there is no caloric? If resistance to acceleration varies with velocity, does that mean there is no mass? If there is no absolute space, does that mean there is no ether? If disorder only increases with high probability and not inevitably, does that mean there is no entropy? In all these cases, it is arguable that antecedent usage left the issue open, prior to the discoveries cited in the antecedents of the questions, yet a decision was needed on the consequents.

A decision was needed, but nothing dictated a particular answer. It was simply a matter for terminological decision. A semantic indeterminacy that had been previously been viewed as inconsequential now turned out to threaten contradictory judgements on different understandings. Nothing substantial was at issue, but the terminology needed to be refined. (Thus “caloric” and “ether” were deemed to require conditions that are unsatisfied in actuality, while “mass” and “entropy” went the other way.)

*Implications for Methodological Naturalism*

These last points carry a moral for philosophical methodology. Quine rejected the idea that philosophy consists of the conceptual analysis of analytic truths because he thought that there aren’t any analytic truths to speak of (once we put trivial one-criterion terms like “bachelor” to one side). Instead philosophy is continuous with science, in seeking to articulate substantial theories of reality that accord with the empirical evidence. The account of analyticity I have outlined leads us to the same non-exceptionalist view of philosophy, but by a different route. On the account I have offered, there are plenty of analytic truths, covering theoretically complex law-cluster terms as well one-criterion ones. But even so these analyticities are of no serious interest to philosophy, which is focused instead on the substantial issues that lie beyond them.

Take any philosophically significant term like “knowledge” or “free action”, or “consciousness”, or “belief”, or so on. As with theoretical terms in science, our language will lay down requirements for falling under these terms, and so will uphold a corresponding range of analytic truths involving. *Knowledge requires truth*, *free actions stem from motives*, and so on. However, these truths will not convey any substantial information about the world, or at least nothing that we don’t already know from logic. Rather they convey only empty conditional Carnap-sentence-type claims like “Knowledge, a veridical mental state, if such exists, is a veridical mental state”. As such, analytic truths will be of no philosophical interest, precisely because they are empty of any substantial significance.

This emptiness can be obscured, however, by the kind of semantic indeterminacy discussed in the last subsection. Take the terminology of “freely willed” action. Are we to understand this term as covering any action which stems from an agent’s motives? Or does it require in addition that the action not be governed by natural law? It is not implausible that this question was left undecided by those who originally introduced the term, no doubt because they assumed that any actions satisfying the first requirement would satisfy the second too. But in the modern period this assumption has become questionable, under pressure from the view that everything is subject to natural law, including humans and their activities. And this has meant that this terminological issue has become crucial to the terms of the debate. Should we deem stemming from motives to be sufficient for free action, with the compatibilists, or should we require in addition, with the libertarians, that the action be undetermined?

This choice might look like a substantial issue and might thus foster the impression that the analytic claims matters to philosophy. (If the choice between compatibilism and libertarianism doesn’t matter to philosophy, what does?) But in truth the issue here is just conventional, an arbitrary choice about how to understand the term “free will” and itself of no philosophical consequence whatever. Of course there are substantial and serious issues in the vicinity. *Are* any of the actions that stem from beliefs and desires actually undetermined? And, if none is, should this make a difference to the way we *blame* or *praise* agents? But these substantial issues are quite independent of the terminological choice and can be posed however we choose to understand “free will”—or indeed without using the term at all, in the way I have just done.

More generally, there are plenty of philosophical terms whose meaning is indeterminate, and this can create the impression that charting analytic truths is an important part of philosophy. Is non-physical built into the meaning of “consciousness”? Is compositional structure built into the meaning of “belief”? And so on. But, as before, in these cases the terminological options make no difference to the real issues, and philosophy would do far better to by-pass them by.

In this section I have been writing as if the non-Quinean approach to analyticity is an agreed part of contemporary philosophical orthodoxy. But I rather doubt that this is so. In truth, these matters often appear to be little understood or even discussed. This is a pity, because they are directly relevant to debates about the role of supposedly a priori knowledge in philosophy. I have the impression that many contemporary philosophers feel that Quinean considerations somehow legitimate a reliance on a priori “intuitions”. As they see it, Quine showed that concepts and theories are tied up together. There is no way of cleansing our concepts of empirical content. Yet the structure of our concepts is surely available to us a priori, or at least introspectively. So we can appeal to a priori methods in philosophy, yet remain engaged with substantial features of reality.

But we can now see why doesn’t work at all. Once we have freed the analytic-synthetic distinction from Quine’s use theory of meaning, this confused defence of philosophical intuitions is hostage to the traditional question: are the deliverances of intuition supposed to be analytic or synthetic? If the former, they are of no substantial interest; if the latter, they call for a posteriori backing. There is some irony in the idea that Quine’s “attack on the analytic-synthetic distinction” might have encouraged a generation of contemporary philosophers to by-pass this traditional challenge and persist in the idea that philosophy enjoys a distinctive source of a priori knowledge (cf Papineau 2015).

**Physicalism and Ontology**

*Minds*

In Quine’s view, we should be ontologically committed to just those entities that are referred to or quantified over in our best (and regimented) theories of the world. He argued that this boils down to a commitment to physical objects and sets.

This committed him to physicalism about mental entities, insofar as he accepted their existence. He was sceptical about *beliefs*, dismissing the idea that they should be conceived of as relations to dubious “propositions”, and despairing of clear criteria of identify if they are viewed as relations to sentences. But he was happy enough to countenance such less propositional mental states as pains, sensations, emotions, and so on.

Here his treatment is generally in line with contemporary physicalist approaches to the mind. Mental states add nothing to their accompanying brain states in respect of “systematic efficacy in development of theory”. So they should either be eliminated or reduced to the brain states—and “the latter version sounds less drastic” (1960a 265).

As it happens, reductive physicalism about the mind is not a central issue for Quine. In *Word and Object* it is discussed only right at the end, in part of a short section “Number, Mind and Body” (section 54). He says “This brief for physicalism adds . . . nothing to what others have said” and refers readers to Carnap’s *The Unity of Science*[[4]](#footnote-4) and Feigl’s “The ‘Mental’ and the ‘Physical’”.

Quine also refers briefly to Davidson in this section, and in later writings endorses his “anomalous monism”. His mature view seems to be a combination of token-identity, physical supervenience (“nothing happens in the world . . . without some redistribution of micro-physical states . . .” 1981 99), and the non-reducibility of special predicates to physical predicates.

Throughout his career, Quine seemed little concerned with precise arguments for these physicalist doctrines, or with worries about whether they respect the causal significance of mental states. Given the extensive discussion of these matters in recent decades, this might seem surprising. Perhaps part of the explanation lies in Quine’s lack of theoretic interest in *causal* matters.

Quine shared the general logical empiricist suspicion of causation, as opposed to lawlike co-variance (“. . . the notion of cause itself has no firm place in science. The disappearance of causal terminology from the jargon of one branch of science and anther has seemed to mark the progress in the understanding of the branches concerned” 1957 12). Since much of the recent debate about physicalism has focused specifically on the causal significance of mental and other prima facie non-physical states, perhaps this is enough to explain his lack of engagement with detailed aspects of the mind-body problem..

*Numbers*

Let me finish by noting one last divergence between Quine and contemporary naturalism—one which again might plausibly be attributed to his lack of concern with causal considerations. Quine observes that quantification over numbers and sets plays an indispensable role in our best scientific theories. Given his general attitude to ontological commitment, he thus embraces the conclusion that numbers and sets exist (1960a section 54).

To those of us who have followed subsequent debates about indispensability arguments in the philosophy of mathematics, Quine’s argument here will now seem too quick. An initial challenge to Quine’s stance was posed by Hartry Field’s *Science without Numbers* (1980). Field queried whether references to numbers and other abstract objects do in fact play an essential role in our best theories. He appealed to standard “representation theorems” to argue that theories that refer to abstract numbers can in principle be replaced by “nominalist” theories that refer only to concrete relations between physical objects. These nominalist theories, argued Field, would be theoretically superior even if practically unwieldly. While in practice we invoke the realm of abstract numbers to simplify inferences between nominalist claims, these inferences can always be made by logic alone, which then allows us to view the abstract numbers themselves as useful fictions that are employed solely for pragmatic purposes.

Some critics have queried whether Field’s nominalist replacements for orthodox scientific theories will always be available. In *Science Without Numbers* Field explained in some detail how Newtonian gravitational theory might be nominalized. But it is less clear how this might be done for certain other branches of physics including quantum mechanics (Malament 1982). Moreover, Field’s programme faces a number of other technical challenges (Resnik 1985).

Still, even Field’s project founders on technical grounds, it is doubtful that Quine’s indispensability argument can be saved. Field’s motivation for doing science without abstract numbers wasn’t just that they *could* be dispensed with. It was also that they *ought* to be dispensed with—on the grounds that they are ill-suited to play a role in causal explanations of concrete facts. Consider a body that undergoes a given acceleration because it has a certain mass. That mass might conventionally be represented by the body bearing the mass-in-grams relation to the real number 2. But that relation to the number 2 can’t really be the cause of the body accelerating as it does. Apart from anything else, the body also bears the mass-in-pounds relation to the real number 0.00440925. It must rather be some intrinsic property of the body that is responsible for its motion, not its conventional relation to some real number outside space and time. That intrinsic property might conveniently be labelled by various conventional relations to real numbers, but those numbers play no serious explanatory role in the concrete world of masses and accelerations.

This casts Quine’s indispensability argument in a new light. Suppose that advocates of Field’s programme do find themselves unable to nominalize certain branches of successful science. In that case our best theories will indeed quantify over abstract numbers and sets. But does this force us to believe in the abstract numbers? We still face the argument that relations to abstract numbers can play no causal role in concrete reality. A natural response is thus that our best theories are at most imperfect representations of that reality and the abstract numbers they invoke do not really exist (Balaguer 1998, Melia 2000, Leng 2010).

Note how this stance still respects the Quinean principle that we are ontologically committed to any entities quantified over in our beliefs. Where it differs from Quine is simply in denying that we should believe our best theories. Rather we should restrict our beliefs to their implications for concrete reality. Perhaps in time we will develop better theories that do eliminate the abstract numbers. Or perhaps there are deep reasons why our human representational devices are incapable of so being fully nominalized. But either way we should withhold belief from our current best theories and commit ourselves only to their nominalist consequences.

Much more could be said on this topic. The post-Quinean debate has made it clear that there is no quick route from the appearance of natural numbers in our best scientific theories to ontological commitment. Causal considerations, as I said, did not play a significant role in Quine’s thinking. But once they are introduced they cast suspicion on the appearance of numbers and other abstract objects in our scientific theories. It makes more sense to suppose that the theories are imperfect than that concrete reality involves abstract numbers. Still, even if abstract numbers play no part in the concrete world of causes and effects, does that necessarily mean that they are not real? Perhaps there are independent arguments for supposing that they exist outside the concrete spatiotemporal realm. Any such arguments, however, would have to proceed quite differently from Quine’s appeal to the role played by numbers in our best theories of concrete reality.

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1. Is it possible to be a disquotationalist about truth without embracing a use-theoretic account of meaning? if not, this argues that disquotationalism leaves no room for substantial epistemological reliabilism after all. I myself am persuaded by this line of argument. A major challenge facing disquotationalist accounts is to explain truth for sentences in foreign languages. The natural answer is to equate such truth attributions with quotation-free home language sentences that *translate* the relevant foreign sentences. But “translate” can’t here be equated with “same *truth* conditions” on pain of circularity. It’s not obvious what else can fill the gap, however, except an appeal to the foreign and home sentences having the *same use*. See Horwich 1990 1998. [↑](#footnote-ref-1)
2. In *Word and Object* 1960a Quine cited Putnam’s “The Analytic and the Synthetic” 1962 when it was in press. [↑](#footnote-ref-2)
3. One might well view Wittgenstein’s “rule-following considerations” in the same spirit (1953 sections 138-242). If the meanings of words derive from rules for using them, what lends authority to the rules? The unavailability of any satisfactory answer is not some deep revelation about the nature of meaning, but rather another reason to reject the starting idea that meaning derives from rules for use. [↑](#footnote-ref-3)
4. In fact Carnap’s *The Unity of Science* says nothing about mind-brain identity. Insofar as it touches on mind-body issues, it is concerned rather to show that a language for physical observables (eg “red surface”) rather than for mental states is the appropriate basis for all sciences. He does refer the reader, however, to his “Psychologie in Physikalischer Sprache” 1932, in which he does specifically argue for reductive mind-brain identity. Interestingly, Schlick was already endorsing this view in the 1920s (Schlick 1925). [↑](#footnote-ref-4)