Part I

Method
For Quine, as for many canonical philosophers since Descartes, epistemology stands at the very center of philosophy. Indeed, at the outset of “The Nature of Natural Knowledge,” Quine writes of “those of us who look upon philosophy primarily as the theory of knowledge” (1975, 67). In this chapter, I discuss some central themes in Quine’s epistemology. I attempt to provide some historical context for Quine’s views, in order to make clear why they were seen as such radical challenges to then prevailing orthodoxies within analytic philosophy. I also highlight aspects of his views that I take to be particularly relevant to contemporary epistemology.

1. Empiricism, not “Empiricism”

According to a common dictionary definition, an empiricist is someone who holds that all of our knowledge is empirical knowledge, or knowledge that is based on experience. When one turns to the history of philosophy, however, it is striking that almost none of the philosophers who are called “empiricists” were actually empiricists in this sense. For example, Hume, generally regarded as the greatest of the classical empiricists, drew a sharp distinction between mathematical knowledge (or more generally, our knowledge of “relations of ideas”) and empirical knowledge. In the twentieth century, Russell, Ayer, and the logical positivists all self-consciously identified with the empiricist tradition but followed Hume in insisting that there is a deep difference in kind between
mathematical and empirical knowledge. Of course, mathematics is hardly a trivial exception: from the time of the ancient Greeks to at least the time of Newton, it stood as the paradigm of a successful science, and when its status as such was challenged by the spectacular rise of mathematical physics in the early modern period, its centrality to the latter discipline only increased its perceived importance in the overall scheme of human knowledge.

A notable exception to the tendency of even self-described empiricists to exclude mathematics from the realm of the empirical was John Stuart Mill (1843). On Mill’s view, the claim that \(2 + 2 = 4\) is actually an inductive generalization from experience, albeit an unusually well-confirmed one. The reason why we are so justifiably confident that \(2 + 2 = 4\) is true is simply this: on all of the countless occasions in the past when a collection with two members has been brought together with another two-membered collection, the process has resulted in a collection with four members. Our knowledge of mathematics is thus of a piece with our observational knowledge, and with the knowledge that is delivered by those sciences that are uncontroversially empirical.

However, Mill’s empiricist account of mathematics was subject to withering criticism. Consider, for example, the famous “argument from unrevisability”: If Mill is right that \(2 + 2 = 4\) is actually an empirical generalization, then we should be open, at least in principle, to the possibility that we will one day make an observation that disconfirms or even falsifies this generalization. (Compare the way in which the well-confirmed empirical generalization that “All swans are white” was ultimately falsified by the observation of black swans.) But in fact, we would never treat any observation as providing falsifying or disconfirming evidence against this claim. Suppose that one brought together two two-membered collections but then counted five entities of the relevant kind. In those circumstances, one would conclude that one must have miscounted at some stage of the process. Or else one would adopt some alternative empirical hypothesis: for example, that some process of spontaneous generation had occurred. The one thing that one would never do is conclude that one had observed a counterexample to the claim that \(2 + 2 = 4\). Moreover, it would be perfectly reasonable for one to resolutely maintain one’s belief that \(2 + 2 = 4\) in the circumstances. But this shows that \(2 + 2 = 4\) is not an inductive generalization.

Given the frequency with which this argument is cited by twentieth-century empiricists as a conclusive reason for holding that mathematical knowledge differs in kind from empirical knowledge, it has some claim to being one of the most influential philosophical arguments of the century. More generally, the consensus that Mill’s attempt to extend empiricism to arithmetic was a clear
failure seemed to take empiricism in the philosophy of mathematics off the table once and for all. Remarkably, in the early and mid twentieth century, even empiricists seemed to concede the venerable rationalist charge that Leibniz had pressed so effectively centuries earlier: that empiricism could never do justice to the apparent necessity of mathematics.

Quine, more than any other individual, is responsible for the twentieth-century revival of empiricism as a general theory of knowledge. Much of his epistemology of mathematics and logic can be understood in terms of his attempt to account for how an empiricist can do justice to their apparent un revisability. Quine’s starting point is the observation that logic and mathematics are not completely freestanding disciplines but rather are deeply integrated with the rest of our knowledge. Indeed, Quine famously suggests that we can picture everything that we take to be true (including both theories that are ordinarily taken to be “empirical” as well as those that are ordinarily taken to be “a priori”) as constituting a single, seamless “web of belief.” The nodes of the web represent individual beliefs, and the connections between nodes represent the logical relations between beliefs. (Of course, inasmuch as we have beliefs about logic, these are themselves nodes within the web.)

Although there are important epistemic differences among the beliefs in the web, these differences are matters of degree as opposed to kind. From the perspective of the epistemologist, the most important dimension along which beliefs can vary is their centrality within the web: the centrality of a belief corresponds to how fundamental it is to our overall view of the world, or how deeply implicated it is with the rest of what we think. The metaphor of the web of belief thus represents the relevant kind of fundamentality in spatial terms: the more a particular belief is implicated in our overall view of the world, the nearer it is to the center, while less fundamental beliefs are located nearer the periphery of the web. Experience first impinges upon the web at the periphery, but no belief within the web is wholly cut off from experience, inasmuch as even those beliefs at the very center stand in logical relations to beliefs nearer the periphery. Not infrequently, experience shows that our overall theory of the world is wrong in some respect or other and thus stands in need of revision. But logic alone does not mandate some particular revision, as opposed to any number of others: there will inevitably be multiple ways in which the web of belief might be altered so as to render it consistent in the light of recent observation. Quine holds that typically, we seek to restore consistency in the web by giving up beliefs that are located nearer the periphery as opposed to beliefs that are more central – and that it is reasonable for us to proceed in this way.

Notice that the position of a belief within the web need not correlate with how confident one is that it is true. Perhaps I am extremely confident that Team
A will beat Team B in tonight’s game, but when I observe otherwise I will unhesitatingly resolve the conflict by giving up my prior belief about the outcome of the game. This is relatively easy for me to do, inasmuch as my belief that Team A will win the game was hardly fundamental to my view of the world, despite the confidence with which I held it. On the other hand, although almost any tension among my beliefs that might emerge could in principle be resolved by my giving up some fundamental logical belief (about what consistency requires, etc.), I would in practice never resolve the conflict in that way. For the fundamental truths of logic are so deeply implicated with the rest of what I believe that to abandon them would be to give up on almost everything that I currently take to be true. And it is surely reasonable not to do this, given that far less radical alternatives are available.4

Quine holds that mathematics, like logic, lies at the center of the web of belief, for it is deeply bound up with the rest of what we believe, and it is essential to the most successful predictive theories that we have. For this reason, our practice is to retain our mathematical beliefs when our theory of the world proves flawed and to make adjustments elsewhere in the system. But the relative immunity to being undermined possessed by our mathematical beliefs is a de facto as opposed to a de jure matter: it is the same immunity enjoyed by any belief (including paradigmatically empirical ones), to the extent that the belief in question lies near the center of the web. It is true that we would in practice never abandon the belief that $2 + 2 = 4$ in response to some apparent observation or experimental outcome. But by the same token, we would in practice never abandon our best confirmed theories in chemistry in response to some particular observation or experimental outcome, either.5

Of course, there is no absolute guarantee that chemistry will not evolve in such a way that future scientists will see fit to give up on what we now regard as the most fundamental truths of chemistry. But by the same token, there is no transcendental guarantee that we would never give up on what we now take to be a fundamental truth of logic or mathematics, should the overall benefit to the web of belief make such a choice seem worthwhile. Perhaps we can much more easily imagine changing our minds about what we now take to be the fundamental truths of chemistry than about what we now take to be fundamental truths of logic or mathematics. But this simply reflects the fact that the former, although deeply implicated with many beliefs that have stood the test of time, nevertheless pale in comparison to the latter in the same respect. Again, the difference is one of degree and not kind.

In this way, Quine attempts to account for the seeming immunity from empirical disconfirmation of mathematics in a way that preserves the thoroughgoing empiricist idea that there is no deep difference between such truths and truths that are uncontroversially empirical. Whether his account is
ultimately tenable, it is fair to say, I think, that it is the best answer to the challenge that empiricists have yet devised.

2. Overcoming Traditional Distinctions

Quine’s celebrated attack on analyticity in “Two Dogmas of Empiricism” (1951) is in effect an attack on the notion of a priority. Here again the historical context is important. According to the positivist orthodoxy to which Quine was reacting, the concept of analyticity was an extremely significant one for philosophy. Indeed, within the general positivist framework, the category of “the analytic” had taken on an importance that would have astounded Kant, who first introduced the analytic/synthetic distinction. Among the truths that Kant took to be a priori knowable, he famously distinguished between the substantive and philosophically interesting “synthetic a priori” truths and the philosophically uninteresting “analytic a priori” truths. Among the former, Kant included the propositions of arithmetic, geometry, the most fundamental presuppositions of empirical science, and the objects of metaphysical knowledge, if such knowledge were possible at all. In contrast, the analytic a priori truths were those cognitively trivial truths in which (as Kant rather obscurely put it) “the predicate B belongs to the subject A, as something which is (covertly) contained within this concept A.” (Compare the twentieth-century’s favorite example of a putatively analytic truth, “All bachelors are unmarried.”) The driving question of Kant’s first critique is “How is synthetic a priori knowledge possible?”; his purpose in drawing the analytic/synthetic distinction in the opening pages of that work is to set aside the category of the analytic truths as the philosophically uninteresting a priori truths.

Perhaps no commitment was more central to logical positivism than the rejection of the Kantian synthetic a priori: for the positivists, any truth that is a priori knowable is also analytic. Not only did the logical positivists insist that the analytic and the a priori knowable were coextensive, but they saw an explanatory relationship between the two: the reason why some truths are a priori knowable is that they are artifacts of our linguistic conventions or representational frameworks. Empirical inquiry always takes place within some linguistic framework or other, and certain truths simply fall out of the framework that one has adopted. The reason why such truths are a priori knowable is that one in effect commits to treating them as true in adopting one linguistic framework rather than another, so knowledge of their truth can be had simply by engaging in armchair reflection upon the properties of the framework itself.

Because the positivists viewed empiricism about mathematics and logic as hopeless, the brand of empiricism to which they subscribed was limited in its
scope. But understanding a priority in terms of analyticity seemed to preserve the spirit of empiricism, inasmuch as one who does so avoids the need to posit (e.g.) some faculty of rational intuition alongside the familiar five senses, whose job it is to detect the truth of a special class of propositions. Rather, a priori knowledge is available to anyone who understands the language that is being employed. It is thus common ground between Quine and the philosophers to whom “Two Dogmas” is addressed that the received doctrine of analyticity is the only hope for making the traditional notion of a priority philosophically respectable. One message of Quine’s critique, then, is that even this relatively deflationary conception of the a priori is untenable.

Quine’s specific arguments against the received view of analyticity are discussed in detail elsewhere in this volume. Here I want to emphasize an aspect of his critique that is perhaps equally important: the idea that positing a distinction between analytic and synthetic truths is pointless, because such a distinction corresponds to nothing in scientific inquiry as it actually proceeds. We have already discussed Quine’s insistence that our practice of holding fast to mathematics and logic does not require us to posit a deep distinction between empirical and non-empirical truths. But the present point is perhaps best illustrated by considering a third category of truths for which the positivists claimed the status of “a priori knowable in virtue of being analytic”, viz., so-called “meaning postulates” (Carnap 1936; 1937a) or “coordinating definitions” (Reichenbach 1924 [1969]) or “reduction sentences.”

A problem in the theory of knowledge to which the logical positivists devoted a great deal of attention is this: given that scientific hypotheses are shot through with theoretical terms such as “electron” and “proton,” terms that never occur in the observation reports that are cited as evidence for those hypotheses, how do the observation reports come to bear on the hypotheses in the first place? That is, how can statements that make use of one vocabulary confirm or disconfirm statements made in an entirely different vocabulary? The positivist solution to this problem was to invoke the apparatus of “coordinating definitions,” “meaning postulates,” or “reduction sentences.” These statements are neither observation reports nor theoretical hypotheses, but rather definitional or quasi-definitional bridge principles that link the observational vocabulary with the theoretical vocabulary. In connecting theory with evidence in this way, these bridge principles allow for the confirmation and disconfirmation of the former by the latter. Although the bridge principles are thus intimately involved in any instance of empirical confirmation of a theoretical hypothesis by an observation or experimental outcome, the truth of these analytic postulates is never at issue in any empirical inquiry: in the language of the final section of “Two Dogmas” they are taken to be “confirmed come what may.” Of course, one could adopt different coordinating definitions, but there could never be any
empirical reason to do this, in the way that one could have empirical reasons to change one’s mind about which theoretical hypotheses are true. (Giving up a coordinating definition would be like ceasing to count the sentence “All bachelors are unmarried” as true because one had decided to start using a language other than English, a language in which these strings of letters had an entirely different meaning.)

Quine rejects the idea that there is some privileged class of truths that plays the role that the positivists assigned to coordinating definitions. There is no sacrosanct body of principles that underwrite empirical confirmation, whose truth could never be called into question by empirical considerations. Quine himself thinks that evidence confirms a hypothesis relative to some background theory, but this background theory enjoys no special status: it is made up of the very kinds of claims for which empirical evidence is given. Of course, in the context of a particular experiment, a scientist might simply take the truth of the background theory for granted, and in that sense its truth is not at issue. But claims that everyone will agree are empirical can and do play the role of unquestioned background theory in this sense. Moreover, a scientist might change her mind about how an experimental outcome or observation bears on a hypothesis, and in this sense revise her beliefs about the “bridging principles” that mediate between the evidence and the theory. But this does not mean that the scientist is adopting a new linguistic or conceptual framework, as opposed to changing her mind about how the world works. That is, empirical considerations might lead the scientist to revise her view about how a given body of evidence bears on the theory, which suggests that the principles that connect evidence and theory are not analytic.

In short, if what we seek is an illuminating account of how inquiry progresses, imposing a distinction between (i) those statements that scientists accept because they have the status of coordinating definitions or analytic truths and (ii) those statements that scientists accept as “synthetic” truths illuminates nothing. Indeed, it misrepresents the phenomena.

Recent philosophy has witnessed the resurgence, not only of full-blooded forms of empiricism, but also of full-blooded forms of rationalism. The new rationalists reject the deflationary conception of a priori knowledge endorsed by the positivists; like the classical rationalists, they insist that a priori insight can reveal substantial truths about the world itself. Thus, what was common ground between Quine and the positivists – the assumption that if there is a priori knowledge, then it must be understood in terms of analyticity – is no longer generally accepted.

Although the new rationalism is full-blooded in this respect, it differs from classical rationalism in another way: proponents of the new rationalism typically go out of their way to emphasize that our a priori justification for believing
certain propositions is defeasible, and indeed, defeasible by empirical considerations. Thus, even the deliverances of a priori intuition or insight are not “confirmed come what may.” Given this emphasis on empirical defeasibility, what separates the new rationalists from Quine? The new rationalists posit some *sui generis* faculty or normative mechanism that delivers a distinctive kind of justification, ‘a priori justification’, which only select propositions are eligible to receive. For Quine, there are no such faculties or normative mechanisms. What the epistemologist actually finds when he examines the phenomena is this: there are some things that strike us as obviously true, or true from the armchair. But what strikes one as obvious, or what one is in a position to recognize as true from the armchair, depends a great deal on one’s past experiences and includes many things that would count as ‘empirical’ on anyone’s view. For example, the claims that “2 + 2 = 4” and “there are people” are in this sense both obvious from the armchair.

### 3. Naturalized Epistemology

In the opening pages of “Epistemology Naturalized” (1969a), Quine briefly discusses an epistemological program that had already been abandoned by the time he wrote: the project of showing that material objects are “logical constructions” out of sense data. This program, first outlined by Russell (1914) and pursued most assiduously by Carnap (1928), involved showing that statements about material objects could be translated into logically equivalent statements about sense data. Proponents of the program eschewed any suggestion that the envisaged reduction corresponded to anything psychologically real in the minds of human beings when they spoke or thought about material objects. Rather, the task was to show that our putative knowledge of the external world could at least in principle be made scientifically respectable, according to standards of scientific respectability inspired by work on the foundations of mathematics and the axiomatization of physical theories.

In a famous passage, Quine commented on this program as follows:

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 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 links with observation we are seeking to understand. (1969a, 75–76)

In this passage, we see a number of central and closely related Quinean themes. Among them are the following: (i) a rejection of the logical empiricist idea that a primary task for philosophy is to provide “rational reconstructions” of other subject matters or domains of discourse; (ii) the assimilation of epistemology to empirical science, and in particular, to psychology; and (iii) the rejection of what Quine elsewhere calls “first philosophy.” In the remainder of this section, I will discuss the first and second themes; the third is discussed at somewhat greater length in the following section.

The logical empiricists’ enthusiasm for the task of rational reconstruction was rooted in their rejection of psychologism in the theory of knowledge. In his critique of psychologism in mathematics, Frege (1884) had urged the importance of distinguishing sharply between what passes through the mind of the mathematician when he thinks about mathematics, and what, if anything, justifies or grounds the mathematical claims that he makes. As Frege emphasized, any number of idiosyncratic psychological associations might pass through the mind of a mathematician as he constructs a proof, but these are irrelevant to the justification of what is proved; rather, the justification consists in the proof itself. A rational reconstruction of a body of knowledge is a kind of idealized foundationalist account of that knowledge, in which the less fundamental notions are defined in terms of the more fundamental notions, and the justificatory relations between different statements are made explicit. However, there is no pretense that the structure of justification thus exhibited corresponds to the order of actual cognition. Consider, for example, the rigorization of the calculus that was achieved in the nineteenth century, in which the fundamental notions of the calculus were rigorously defined in other terms, thus eliminating the need to rely on the problematic notion of the “infinitesimal.” The project is generally taken to be a tremendous intellectual achievement, despite the facts that: (i) the calculus was already a spectacular success, and (ii) even today, this work typically plays no role in the thought of those relatively few people who are familiar with it, on those occasions when they use calculus. The logical empiricists’ enthusiasm for the program of rational reconstruction extended beyond mathematics to human knowledge as a whole. Indeed, inasmuch as the philosopher is concerned with knowledge, the activity of rational reconstruction is the proper manifestation of that concern, as opposed to exploring how individuals actually reason or acquire knowledge.
One crucial upshot of this picture is that there is a sharp division of labor between the philosopher who investigates knowledge and those who pursue empirical investigations of knowledge acquisition (e.g., those who study the psychology of reasoning, or the history and sociology of science). 11

Quine rejects this picture as unfruitful: “... Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect” (1969a, 78). In one of his last published essays, “Naturalism: Or, Living within One’s Means” (1995), he offers the following description of his preferred vision: “... 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” (254). In aspiring to a “psychologically and historically plausible” account of knowledge acquisition, Quine more closely resembles classical empiricists such as Locke and Hume than he does the logical empiricists. In this respect, then, Quine is more ambitious than the logical empiricists: he is vulnerable to objections of the form “that’s not how we actually do it,” in a way that they are not.

For the logical empiricists, psychology is irrelevant to the theory of knowledge. On Quine’s naturalized conception, not only is psychology relevant to epistemology, but the latter is a branch of the former. We saw above that Quine advocates “a surrender of the epistemological burden to psychology.” Compare that remark with another famous passage from “Epistemology Naturalized”:

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 way one’s theory of nature transcends any available evidence. (pp. 82–83)

... 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. (p. 83)

There are at least two objections that might be raised to Quine’s assimilation of epistemology to empirical psychology. The first is the objection that he himself takes up in “Epistemology Naturalized”: that making use of the findings of
psychology (or any other special science) in one’s epistemological theorizing is circular, inasmuch as it is part of the business of epistemology to show that such claims can be justifiably relied upon. We will discuss this objection, and Quine’s reply, in the context of discussing his rejection of “first philosophy” in the following section. The most common objection to Quine’s proposal, however, is not that it leads to circularity but that it completely neglects the fact that epistemology is a normative inquiry.\textsuperscript{12}

As traditionally understood, epistemology is concerned with normative questions about what we should believe, or how we ought to revise our beliefs. In contrast, psychology is a purely descriptive, non-normative inquiry: the psychologist investigates how people actually reason or revise their beliefs. So the psychologist and the epistemologist seem to be in different lines of work, and the suggestion that psychology could take over “the epistemological burden” seems to be confused. (Compare: the normative ethicist is concerned with questions about how people should behave. We can imagine a philosopher who radically proposes to “naturalize” normative ethics by conducting empirical research into how people actually behave. But that proposal seems to miss the point of the original inquiry.) Indeed, Quine’s suggestion that the closest worthwhile successor subject to traditional epistemology is a kind of psychology has led many philosophers to interpret him as an eliminativist about epistemic normativity.\textsuperscript{13}

In later writings, however, Quine repeatedly insists that his naturalism does not entail the abandonment of the normative dimension of epistemology. Consider, for example, the following passage:

Quine’s view is thus that the normativity of epistemology is simply the normativity of instrumental reason. The fact that a person can have reasons to revise her beliefs in some ways rather than others is of a piece with the fact that she has reasons to take the means to her ends. This account of epistemic normativity has proven popular among those seeking to naturalize epistemology and the philosophy of science (Giere 1989; Kitcher 1992; Kornblith 1993; Laudan 1990, 318; Maffie 1990a; 1990b). But it is also explicitly embraced by some who are not motivated by naturalistic considerations (see, e.g., Foley 1987). Whether this is the correct way of thinking about epistemic normativity remains an open question.\textsuperscript{14}
4. The Rejection of “First Philosophy”

Quine famously rejects what he calls “first philosophy.” Perhaps the paradigm of a philosopher engaged in first philosophy is Descartes in the *Meditations*. Consider the picture of philosophical inquiry presented there. In order to engage in philosophical inquiry properly, a meditator must consciously and actively distance himself from his pre-philosophical opinions. After all, many of these opinions have the status of mere prejudices, having been uncritically inherited in one’s youth. It is only when such pre-philosophical opinions have been, if not literally discarded, at least *bracketed*, that philosophical inquiry can be responsibly conducted. Once an opinion has been independently substantiated in the course of the inquiry, it can be employed as a basis for further theorizing; prior to such independent substantiation, however, it would be illegitimate to allow that opinion to influence the conclusions that one reaches. Thus, Descartes’ procedure in the *Meditations* suggests a particularly austere answer to the crucial methodological question: “What are the proper starting points for philosophy?”

Quine emphatically rejects the Cartesian answer. Consider, for example, the concluding sentence of “Five Milestones of Empiricism” (1981):

> The naturalistic philosopher begins his reasoning within the inherited world theory as a going concern. He tentatively believes all of it, but believes also that some unidentified portions are wrong. He tries to improve, clarify, and understand the system from within. He is the busy sailor adrift on Neurath’s boat.

In referring to “the busy sailor adrift on Neurath’s boat,” Quine alludes to a metaphor due to Otto Neurath, a metaphor to which Quine returns again and again. According to the metaphor, as theorists attempting to improve our view of the world, we are like sailors who must repair our ship on the open sea. In particular, we do not have the luxury of dismantling our ship in a dry dock in order to reconstruct it from the best materials. Similarly, the Cartesian project of reconstructing our view of reality from the ground up is not a viable option.

Because he subscribes to this picture of how inquiry proceeds, Quine takes an extremely liberal view of the kinds of considerations to which we can legitimately appeal in our philosophizing: “All scientific findings, all scientific conjectures that are at present plausible, are therefore in my view as welcome for use in philosophy as elsewhere” (1969b). This permissive answer conflicts with various traditional answers. For example, the suggestion that “any plausible scientific conjecture” is fair game conflicts with the idea that a consideration must meet some very demanding epistemic standard (e.g., ‘what cannot be
doubted’) in order for appeal to it to be legitimate. Nor is there any restriction to considerations that are available “from the armchair” as opposed to those whose discovery requires empirical observation. Indeed, not only is the philosopher free to make use of observational knowledge, but he can and should make use of high-level theoretical claims from the empirical sciences when these promise to illuminate the topic under investigation. Thus, in his discussions of induction, Quine repeatedly appeals to Darwin’s theory of evolution (1969b, 126–127; 1975, 70). If part of the task of epistemology were to justify the use of induction against the Humean skeptic, then such an appeal would be objectionably circular, since our knowledge of evolutionary theory requires the use of induction. But Quine abjures this conception of the epistemological task: the task is rather to explain why induction works as well as it does, from within our current view of the world. For Quine, there are no general restrictions on the content of the claims to which the philosopher can appeal.\footnote{17}

Quine thus holds that philosophy is continuous with natural 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 naturalist 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 (1995, 256–257).

This view about the relationship between philosophy and science contrasts with at least two rival accounts that are worthy of note. The first is the view to which Quine explicitly alludes in this passage: that a central task of philosophy is to justify or validate natural science. However, while it is clear enough that Quine rejects this picture of the relationship between philosophy and natural science, it is doubtful that any significant contemporary of his accepted it.

On the other hand, Quine’s view that philosophy is continuous with science is incompatible with a third view about their relationship that was ubiquitous during much of his career. Indeed, in the middle decades of the twentieth century, few philosophers working within the broadly Anglo-American tradition would have doubted that there is a sharp distinction between philosophical questions and scientific questions. Despite their differences, both the logical
positivists and the “ordinary language” school centered at Oxford shared a conviction that philosophy is essentially concerned with language and with the concepts that we use to speak about reality; it is the task of science, on the other hand, to answer first-order questions about nonlinguistic reality itself.

A classic statement of this view is contained in Ayer’s *Language, Truth, and Logic*, a book that was responsible for introducing the central ideas of logical positivism to the English-speaking world:

> [T]he philosopher, as an analyst, is not directly concerned with the physical properties of things. He is concerned only with the way in which we speak about them.

In other words, the propositions of philosophy are not factual, but linguistic in character—that is, they do not describe the behaviour of physical, or even mental, objects; they express definitions, or the formal consequences of definitions. Accordingly, we may say that philosophy is a department of logic...

It follows that philosophy does not in any way compete with science. The difference in type between philosophical and scientific propositions is such that they cannot conceivably contradict one another. And this makes clear that the possibility of philosophical analysis is independent of any empirical assumptions.

Elsewhere in the same chapter, Ayer gives two examples of legitimate projects for the philosophers: (i) the project of defining the English word “rational,” and (ii) the project of showing how sentences that make reference to material objects can be translated into logically equivalent sentences about sense data.

Quine rejects this picture on every count. There is no “difference in type” between philosophical and scientific propositions. The philosopher, like the scientist, is interested in the world itself, and not merely in the way in which we speak about the world – indeed, from a Quinean perspective, it is a mistake to think that the two can be separated in the way in that Ayer supposes. Quine’s rejection of the idea that philosophy is a higher-order discipline with respect to science is intimately connected with his rejection of a theoretically significant analytic/synthetic distinction. On Ayer’s view, there is a body of theoretically important analytic or conceptual truths that it is the distinctive task of philosophy to discover; the philosopher employs a distinctive method (“philosophical analysis”) in order to uncover these truths. On the other hand, it is the task of empirical science to discover synthetic truths about the world, using its distinctive methods. Because the analytic propositions that constitute the subject matter of philosophy are of a “different type” from the synthetic truths that science aims to discover, the two kinds of truths “cannot conceivably contradict one another.” So no empirical discovery by science could overturn or even bear on the acceptability of a philosophical analysis. Quine denies that any pronouncements of philosophy have such status. A fortiori, there is no distinctive philosophical method that outputs such truths.
The picture of philosophy as a higher-order discipline had earlier been championed by Carnap. In *The Logical Syntax of Language* (1937b), Carnap had argued that traditional philosophy “is to be replaced by the logic of science . . . that is to say, by the logical analysis of the concepts and sentences of the sciences” (p. xiii). In keeping with this vision, the logical empiricists devoted tremendous intellectual energy to the task of providing formal analyses or explications of key scientific concepts such as *confirmation* (Carnap 1950; Hempel 1945 [1983]), *explanation* (Hempel and Oppenheim 1948; Hempel 1965) and *reduction* (Nagel 1961). Here again the philosopher is understood not as someone professionally concerned to describe and understand the world itself, but rather with the concepts that the scientist employs in attempting to describe and understand the world.

Consider the ambitions of philosophers like Carnap and Hempel to provide purely *formal* models of explanation and confirmation. 18 While Hempel would agree that determining whether a proposed explanation is the actual explanation of some phenomenon requires empirical investigation, that it qualifies as a potential explanation in the first place is primarily a matter of its having the right logical form. In particular, one can recognize that it is a potential explanation without making any substantive assumptions about how the world works. An analogous point holds with respect to formal accounts of confirmation: while it is an empirical question whether such-and-such observation statements are true, the judgment that those observation statements would confirm a given hypothesis is independent of any assumptions about the world itself.

From a Quinean perspective, there is no reason to think that there are purely formal, content-free models of explanation and confirmation to be had. Perhaps sufficiently radical shifts in our view of how the world works would lead us to change our minds, not simply about which explanations are true (that much is banal), but about what counts as a legitimate explanation. For example, when Cartesian physics replaced Aristotelian physics, the claim of the Cartesians was not simply that the teleological explanations offered by the Aristotelians were wrong, but that they were not even potentially explanatory. According to the Cartesians, the only genuine explanations were mechanical explanations that appealed to the displacement of one material body by the contact of another. Putative explanations that appealed to final causes were pseudo-explanations. But when Descartes’ physics was replaced by Newton’s (with its postulation of action-at-a-distance), the standards for explanatoriness changed again. It is clear enough that the Cartesians were wrong to hold that all genuine explanations are mechanical explanations; in this case, their mistaken metaphysics and physics led them to a mistaken account of explanation. But what lesson should we draw from this? One possible response is to attempt to provide an account of explanation that is so abstract that it presupposes *nothing* about how the
world actually works. This was the route pursued by Hempel and the logical empiricists. But another possibility is this: our views about how the world works and what it takes to explain something are inevitably linked, and it is a mistake to pursue the theory of explanation as though this were not the case. If we have a deeply inadequate view about how the world works, then we will almost certainly have a deeply inadequate account of explanation. As our view of the world improves, our view about what it takes to explain something is likely to improve as well. But if this is correct, then theorizing about explanation is not a “higher order” philosophical activity to which one’s theory of the world is irrelevant.

Consider also the analogous issue about confirmation. Goodman (1983) pointed out that the predicates “green” and “grue” apply to all of the emeralds that we have observed thus far; in this respect, the two predicates are completely on a par. Why then do we predict that future emeralds will be green but not grue? A natural answer is the following: the predicate ‘green’ picks out a genuine property: green things have a substantive feature in common, and in that respect the class of all and only green things makes up a natural kind. On the other hand, ‘grue’ does not correspond to a genuine property: there is no substantive feature that all and only the grue things have in common, and in that respect the class of the things to which the predicate ‘grue’ applies does not make a natural kind. We take our past observations to confirm “All emeralds are green” and not “All emeralds are grue” because of our view about what is a natural kind and what isn’t. But to make judgments about what the natural kinds are is to engage in substantive theorizing about the world: it is the kind of thing that scientists do in the course of constructing their theories. If anything like this picture of confirmation is correct, then it is a mistake to search for purely formal, “content-free” principles of confirmation. Rather, even our most basic judgments about what confirms what depend on our first-order views about what the world is like. If science led one to change one’s mind about what the natural kinds are, then this would have implications for which inductive inferences one would judge acceptable.

As we have seen, on Quine’s view, there is no sharp division of labor between science and philosophy: it is not as though there is some special set of questions that it is philosophy’s job to answer, to which empirical findings are irrelevant. Even if the philosopher conducts his inquiries from the armchair – as Quine himself did – findings from the special sciences are likely to be highly relevant to those inquiries. For the philosopher, this situation presents both an opportunity and a hazard. The opportunity is one that Quine frequently emphasizes: the philosopher can opportunistically avail himself of the putative findings of other disciplines. The hazard is that a philosophical system that depends on theorizing in other disciplines is only as sound as that theorizing itself is. Perhaps
Quine’s own positive philosophical views provide an example of the hazards of epistemology naturalized. Arguably, a good deal of Quine’s own system presupposes a behaviorist psychology; and while behaviorism passed for cutting-edge science at the outset of Quine’s career, it has not stood the test of time. Unsurprisingly, philosophical views at which one arrives by practicing “naturalized epistemology” tend to stand and fall with the science on which they are based. But to suppose that this is an objection to naturalized epistemology is to suppose that philosophers should seek results that are more secure than the best science of their day; and surely the track record of that enterprise is uninspiring.\(^\text{19}\)

**Notes**

1. Particularly notable is Frege’s discussion in the *Foundations of Arithmetic* (1884 [1980]).
2. For example, the argument is endorsed by almost every leading logical positivist at some point or other. Representative statements include Ayer (1936 [1946], 75–76) and Hempel (1945 [1983], 378–379). In his “Intellectual Autobiography” (1963, 64), Carnap cites this as the consideration that led members of the Vienna Circle to deny that empiricism could account for mathematical knowledge.
4. This follows from Quine’s “Maxim of Minimum Mutilation.” as discussed in his *The Pursuit of Truth* (1990, 14–15) and elsewhere.
5. Kuhn (1960, 1963) emphasizes that the most fundamental assumptions of scientific research programs will in practice be regarded as not subject to empirical disconfirmation or falsification in the context of “normal science” (even when no one would think that the claims in question are *a priori* truths in the philosopher’s sense).
6. Cf. Hylton (2007, 53): “As important as any specific argument against Carnapian analyticity is the fact that Quine thinks it is unnecessary.” In a retrospective comment on his early work, Quine wrote that “I now perceive that the philosophically important question about analyticity and the linguistic doctrine of logical truth is not how to explicate them; it is the question of their relevance to epistemology” (1986a, 207, emphasis his).
7. While there are some significant differences between these notions, they are immaterial for present purposes, so I will ignore them in what follows.
8. Another phenomenon that is relevant here: It is sometimes observed that certain scientific laws – for example, Newton’s laws – seem to be treated by scientists as empirical generalizations in some contexts but as definitions in other contexts. (On this, see, e.g., Hanson (1965) and Buchdahl (1951).) This is further grist for Quine’s mill.
10 A typical disavowal is Carnap (1928), section 54.
11 A manifestation of this within the philosophy of science was the drawing of a very sharp distinction between the “context of discovery” and the “context of justification,” and the insistence that the philosopher is professionally concerned only with the latter. See, e.g., Reichenbach (1938 [2006], 6–7).

The term “rational reconstruction” is apparently due to Carnap (1928), who wrote there of *rationale Nachkonstruktion*.

12 Perhaps the *locus classicus* of this charge is Jaegwon Kim, “What is ‘Naturalized Epistemology’?” (1988).
13 Again, Kim (1988) is both representative of and influential in propagating such an understanding.
14 For criticism, see Kelly (2003), and also the exchange between Leite (2007) and Kelly (2007).
15 “Some years ago I was struck by the large number of falsehoods that I had accepted as true in my childhood, and by the highly doubtful nature of the whole edifice that I had subsequently based on them. I realized that it was necessary, once in the course of my life, to demolish everything completely and start again right from the foundations . . .” (AT VII 17, the opening words of the First Meditation).

Compare the following passage from the *Discourse on Method*:

But regarding the opinions to which I had hitherto given credence, I thought that I could not do better than undertake to get rid of them, all at one go, in order to replace them afterwards with better ones, or with the same ones once I had squared them with the standards of reason. I firmly believe that in this way I would succeed in conducting my life much better than if I built only upon old foundations and relied only upon principles that I had accepted in my youth without ever examining whether they were true. (AT VI 13–14)

16 Notably, the metaphor provides the epigraph for *Word and Object* (1960). The original source of the metaphor is Neurath (1932[1959], 201).
17 Compare Harman’s (2001; 2010) “general foundationalism” and Williamson’s (2007) insistence that, in philosophy as everywhere else, our evidence consists of nothing less than everything we know.
18 An excellent discussion of this aspect of their thought (focusing particularly on the case of Hempel) is Sklar (1999).
19 For comments on an earlier version of this paper, I am grateful to Paul Benacerraf, Bredo Johnsen, and Gary Kemp.

References


