

“SCIENCE ITSELF TEACHES”.
A FRESH LOOK AT QUINE’S NATURALISTIC
METAPHILOSOPHY

Geert KEIL
Humboldt-Universität Berlin

Summary

Quine famously holds that “philosophy is continuous with natural science”. In order to find out what exactly the point of this claim is, I take up one of his preferred phrases and trace it through his writings, i.e., the phrase “Science itself teaches that ...”. Unlike Wittgenstein, Quine did not take much interest in determining what might be distinctive of philosophical investigations, or of the philosophical part of scientific investigations. I find this indifference regrettable, and I take a fresh look at Quine’s metaphilosophy, trying to defuse his avowed naturalism by illustrating how little influence his naturalistic rhetoric has on the way he actually does philosophy.

0. *Introduction*

Over and above its attack on the analytic/synthetic distinction, Quine’s celebrated paper about the “Two Dogmas of Empiricism” reveals some of his more general views on the relationship between philosophy and empirical science. In the opening paragraph, Quine points to “one effect of abandoning” the two dogmas, viz, “a blurring of the supposed boundary between speculative metaphysics and natural science” (1953, 20). He sees a connection between his misgivings about analyticity and what he will later call his *naturalistic* outlook on philosophy. Just as there is no sharp distinction to be drawn between analytic and synthetic truths, there is no clean-cut difference between philosophy and natural science. As he says many years later: “Naturalism brings a salutary blurring of such boundaries. Naturalistic philosophy is continuous with natural science.” (1995a, 256–7, see also 1969, 126–7)

Quine does not use the term ‘naturalism’ until the late sixties. It is

obvious, however, that his line of reasoning in *Two Dogmas* prepares the ground for and even anticipates his naturalism. My paper deals with the question of what exactly Quine's claim means that philosophy is continuous with natural science. For that purpose, I shall take up one of Quine's preferred phrases and trace it through his writings. Strikingly, Quine often introduces tenets usually considered philosophical in nature with the words "Science itself teaches" or "Science tells us". I shall gather some of these claims and evaluate them. Thereafter, I shall bring Quine's broad notion of science into play, and discuss the two continuity theses associated with this sweeping notion of science. Unlike Wittgenstein, Quine did not take much interest in determining what might be distinctive of philosophical investigations, or of the philosophical part of scientific investigations. He was more eager to emphasize what philosophy and natural science have in common. I find this limitation regrettable, and I shall take a fresh look at the few remarks Quine does make to distinguish the philosopher's business. His blurring of the supposed boundary between philosophy and natural science has caused a good deal of alarm among Wittgensteinians and promoters of *a priori* conceptual analysis. Towards the end of this paper, I shall try to defuse Quine's avowed naturalism by illustrating how little influence his naturalistic rhetoric has on the way he actually does philosophy. In evaluating Quine's philosophy of science and his metaphilosophy, we are well advised to try to sort out his scientific avowals from his philosophy at work.¹

1. *What Science Itself Allegedly Teaches*

Examples of philosophical assertions passed off as deliverances of natural science abound in Quine's writings.

(a) A prominent example is his claim that "science itself tells us that our information about the world is limited to irritations of our surfaces" (1981, 72). Quine restates this point repeatedly:

1. In a similar vein, Jonathan Cohen (1987) has suggested "to investigate closely the extent to which Quine's ideas about the method of his philosophical enterprise are coherent with the substance of his philosophical doctrine".

Science tells us that our only source of information about the external world is through the impact of light rays and molecules upon our sensory surfaces. (1975, 68)

[I]t is a finding of natural science itself, however fallible, that our information about the world comes only through impacts on our sensory receptors. (1992, 19)

Science itself teaches that there is no clairvoyance; that the only information that can reach our sensory surfaces from external objects must be limited to two-dimensional optical projections and various impacts of air waves on the eardrums [...]. (1974, 3)

In other words, science allegedly teaches that empiricism is true. The quoted passages are variations on the empiricist credo ‘nihil est in intellectu quod non prius fuerit in sensu’. Unlike the classical empiricists, however, Quine does not describe the sensory input in terms of perceptions, sensations or impressions, but as “irradiation patterns” or “impacts on our surfaces”, or, recently, as “triggerings of our nerve endings”, thus in physiological terms, not in mental ones.

(b) A second example is Quine’s view that *science itself motivates skeptical doubts* and helps to dispel them as well. “[T]he skeptical challenge springs from science itself”, he says, and “in coping with it we are free to use scientific knowledge” (1974, 3). The second half of the statement is a comment on the charge of circularity against naturalized epistemologies. Not claiming to have found a “firmer basis for science than science itself”, Quine feels “free to use the very fruits of science in investigating its roots” (1995, 16). All of this is familiar, and there is no need to go into it.

(c) A third example is his claim that *science tells us what there is*. This view might seem less controversial than the tenets presented above, for most philosophers would admit that there are at least some ways of discovering what there is which are not the philosopher’s business. Quine’s view acquires a bite when reformulated as a characterization of a joint venture of philosophers and scientists:

The question what there is is a shared concern of philosophy and most other non-fiction genres. [...] A representative assortment of land masses, seas, planets, and stars have been individually described in the astronomy books [...] What distinguishes between the ontological philosopher’s concern and all this is only breadth of categories. Given physical objects

in general, the natural scientist is the man to decide about wombats and unicorns. (1960, 275)

Yet the difference between the philosophical and the scientific parts of the joint venture is only one of degree, as Quine says in the famous passage from *Two Dogmas*:

The issue over there being classes seems more a question of convenient conceptual scheme; the issue over there being centaurs, or brick houses on Elm Street, seems more a question of fact. But I have been urging that this difference is only one of degree. (1953, 46)

More specific ontological tenets which Quine poses as findings of natural science could be added, for instance, his substitution of coordinates of spacetime regions for physical objects. In his paper “Whither Physical Objects?” he declares the following to be “an outcome [...] of physics itself”: that “our physical objects have evaporated into mere sets of numerical coordinates” (1976, 502).

Ontology set aside, there are many further *prima facie* philosophical insights which Quine ascribes to science. For example, he considers the “question of unity of science [...] a question within science itself” (1995a, 260). His notion that epistemology is “only science self-applied” (1969a, 293) falls into line with (a), i.e., with his scientific justification of empiricism. Similarly, his behavioristic speculations about the process of language acquisition have been dubbed an attempt at “naturalizing empiricism” (Gibson 1999, 461). I shall finish this brief survey, however, in order to make a few comments on the tenets (a), (b) and (c). These comments will be made, for the time being, in disregard of Quine’s broad and somewhat idiolectal use of the term “science”.

ad (a) As to the first claim about science establishing the truth of empiricism: It is tempting to dig more deeply and enquire *which* scientific discipline has found out that “our information about the world is limited to irritations of our surfaces”. It has also been asked how natural science *could* demonstrate that sensory evidence is the only evidence (Koppelberg 2000, 71). This is a good question to ask. After all, to assert that there is no other kind of evidence amounts to a non-existence claim, and non-existence claims are notoriously hard to verify empirically.

It seems to me not quite correct to call it empirical findings that, for example, the phenomena of clairvoyance and telepathy do not exist,

or that they are no source of information about the world. The better thing to say would be that there is *no empirical support* for assuming that people with such talents exist, and that we, good empiricists that we are, might *conclude* from the lack of empirical evidence that there are no such phenomena. A conclusion drawn from a lack of evidence is a more indirect discovery than an empirical verification. As soon as someone opens a debate about what *counts* as evidence, though, the empiricist would no longer get away with simply citing what he deems to be evidence. Giving empirical evidence for a claim is one thing, reflecting about what counts as evidence is quite another. It is the latter that empiricism, as a philosophical doctrine, has to deal with. And, while being a scientifically minded person arguably promotes being converted to empiricism, it is certainly not an empirical finding that empiricism is true.

Quine holds, famously:

The question how we know what there is is simply part of the question [...] of the evidence for truth about the world. The last arbiter is so-called scientific method, however amorphous. (1960, 22–3)

Here, I would suggest that the phrase “the question of the evidence for truth about the world” is ambiguous, in the way just described. As to the question what counts as evidence, it is simply not true that this question can be *settled* by scientific method alone, as the phrase “the last arbiter” suggests. An appeal to scientific method(s), amorphous or unified, plays an important role in such debates, but the relation is less direct than Quine suggests, more of which below.

ad (b) Quine explains his second claim, viz., that “the skeptical challenge springs from science itself”, as follows: “The skeptics cited familiar illusions to show the fallibility of the senses; but this concept of illusion itself rested on natural science, since the quality of illusion consisted simply in deviation from external scientific reality” (1974, 3).

This passage invites the reply that not all doubts are skeptical doubts. Even doubts concerning the reliability of our senses need not amount to skepticism proper. If everyone who has become aware of the fallibility of our senses were reckoned to be a skeptic, non-skeptics could only be recruited among very young children. Skeptical doubt, Cartesian or Pyrrhonian, is a more serious affair, and it could well be the case that, while science motivates doubts, it does not motivate skeptical doubts. Furthermore, it is hard to see why the external reality from which illu-

sions deviate should be described as “scientific reality”, so that the “concept of illusion itself rest[s] on natural science”. Common sense realism as a backdrop should suffice.

When it comes to the question of how the skeptical challenge can be met, it is worth noting how modest Quine’s claim actually is. It is one thing to hold that science itself answers skeptical doubts, but it is quite another thing to hold, as Quine does, that in coping with skepticism “we are free to use scientific knowledge” (ibid.). The latter claim is much weaker, and it prompts me to draw the same moral as before, viz. that the connections between scientific findings and philosophical conclusions are not as direct as it initially seems. In particular, Quine’s view that skepticism is a pointless exercise with regard to natural science as a whole does not constitute a scientific result, but is rather an upshot of his *philosophy* of science. Quine is known to argue that global skepticism is generally out of place because it rests on the faulty assumption that natural science is “answerable” to “a supra-scientific tribunal” (1981, 72). If science is not in need of a philosophical justification or foundation in the first place, the skeptic’s worry that this foundation is crumbling turns out to be pointless. Whatever the merits of this line of reasoning, it is clearly philosophical in nature.

ad (c) As Quine urges in *Two Dogmas*, determining what there is should be seen as a joint venture of philosophical ontology and natural science because the difference between the conceptual issue over there being classes and the empirical issue over there being unicorns is only one of degree. To him, the difference between philosophical ontology and empirical science boils down to a difference in the breadth of the categories involved.

In general, it is unrewarding to dispute continuity claims in philosophy. They sound so wise and serene, and they are very hard to refute. Even the distinction between a heap and a non-heap seems to admit of degrees. On the other hand, nobody denies that different methods are employed for tracking down unicorns than for deciding whether there are classes. Russell once tried to convince his student Wittgenstein that there is no rhinoceros in the room. Wittgenstein, who held, at that time, that nothing empirical is knowable, would not admit this. “In later life Russell made great play of these discussions and claimed he had looked under all the tables and chairs in the lecture room in an effort to convince Wittgenstein that there was no rhinoceros present ” (Monk 1990, 40). You can bet that Wittgenstein, whose sense of humour was underde-

veloped, was not amused. A familiar way of expressing the misgivings about Russell's verification procedure is to say that "for Wittgenstein the issue was metaphysical rather than empirical" (ibid.). Now let us assume that on Wittgenstein's making such a claim, Russell would had retorted that this difference is only one of degree. Wittgenstein would have gone wild, understandably enough.

Quine's claim that ontology differs only gradually from empirical science exploits an ambiguity in the question of what there is. In the case of rhinoceros, or of brick houses on Elm Street, the question is whether physical objects of a given kind are to be found in a certain place, the answer being a matter of betaking oneself to go there and to have a look. In the case of classes or numbers, the question is whether to include a certain category in one's ontology or not. Establishing the existence of numbers would amount to a *justification* of an ontological commitment. Both such justifications and empirical discoveries about the presence of brick houses or rhinoceros may count as answers to the question 'what there is'; yet in calling the difference only a matter of "breadth of categories", Quine deliberately ignores the more significant differences.

In general, calling a difference one of gradation does not smooth out the difference. Rather, it indicates the speaker's reluctance to regard the difference as relevant to the present context. We must distinguish, of course, between differences and distinctions, the former residing in the world, the latter being drawn in language. Every difference is as big as it happens to be, and how many degrees there are in between depends on how many distinctions one is prepared to draw, i.e., how many predicates one finds in order to draw finer distinctions. (Besides, there may be distinctions without a difference, but this is not what Quine seems to have in mind when he calls the difference between ontology and empirical science one of degree.) In view of this consideration, it seems wise not to attach too much importance to the question whether the difference between philosophy and the rest of science is one of degree or one in kind. In a way, every difference admits to gradation, and in another way, everything is *sui generis*, if the genera are cut to a suitable size.

In order to reach firmer ground again, let us turn to Quine's more specific claim about replacing spacetime regions for physical objects, since physics itself has done so (cf. 1976, 502). On closer examination, Quine's ontological revision is not *motivated* by the fact that matter goes by the board in modern physics. His real argument is that

if we specify the wormlike region of four-dimensional spacetime that a certain physical object takes up in the course of its career, we have fixed the object uniquely. Therefore, he thinks, we are well advised to *identify* the object with the region in order to avoid “the inelegance of a tandem ontology: matter and space” (1974, 133). Quine admits that the identification “is artificial, but actually it confers a bit of economy, if we are going to have the space-time anyway” (1995a, 259). In other words: *It’s economy, stupid*. It goes without saying that physicists may *adopt* this policy of not multiplying entities sine necessitate. This fact, though, makes neither Occam’s maxim nor Quine’s worries about a heavily overpopulated universe a result of physics.²

2. *Quine’s Sweeping Notion of Science*

Hitherto, I have disregarded Quine’s unusually wide use of the term ‘science’. It is now time to make up what I have missed.

It is instructive to contrast Quine’s views on the relationship between philosophy and science with Wittgenstein’s. In his middle period, Wittgenstein used to say things like these:

Philosophers constantly see the method of science before their eyes, and are irresistibly tempted to ask and answer questions in the way science does. This tendency is the real source of metaphysics, and leads the philosopher into complete darkness. (Wittgenstein 1972, 18)

This remark of Wittgenstein’s seems to contrast sharply with Quine’s views that philosophy is continuous with science, and that the last arbiter is always scientific method. But wait. When Wittgenstein speaks of ‘science’, or ‘Wissenschaft’, he has in mind *the sum total of the natural sciences*, which he considers to be equivalent with “the totality of true propositions” (*Tract.* 4.11). In the *Tractatus*, Wittgenstein makes it clear that “[p]hilosophy is not one of the natural sciences” (4.111). Nobody would take exception to this, not even Quine. Philosophy is not physics, nor is it biology, chemistry, etc.

2. A closer look also reveals that Quine’s move from a three-dimensional to a four-dimensional conception of physical objects is not motivated by the theory of relativity with its spacelike treatment of time, but rather by age-old philosophical puzzles such as Zenon’s paradoxes and Heraclitus’ problem about stepping into the same river twice. See Quine 1960, 171–2.

When Quine looks upon philosophy as part of the scientific enterprise, he employs a different notion of science. When he uses ‘science’ in the singular, the term is not a collective name for a bunch of scientific disciplines. Rather, it is supposed to mean ‘our scientific world view’ or ‘our overall theory of the world’. This shift in meaning changes the situation considerably. In the face of Quine’s embracing notion of science, the issue over a certain assertion’s being scientific or philosophical in character tends to lose its point, and it becomes hard to understand why I was making such heavy weather of Quine’s assertions about “science itself” telling us all these things.

Previously, I asked *which* science has found out that our information about the world is limited to irritations of our surfaces, and I suggested that no theory of physics and no biological theory could ever discover what *counts* as evidence for our beliefs about the world. Expecting otherwise, however, rests on a tendentious interpretation of Quine’s empiricist tenet. For if the term ‘science’, here and in related places, stands for ‘our overall theory of the world’, it should come as no surprise that Quine has no particular scientific theory up his sleeve to substantiate his claim. Given his sweeping notion of science, “science has found out” does no longer mean “*a* science has found out”. I hasten to add that, by the same token, Quine’s formulations are stripped of their naturalistic bite. The same holds true for his slogan “Philosophy of science is philosophy enough”: Against whom is this slogan directed if science includes philosophy?

A characteristic passage goes like this: “Even our appreciation of the [...] under-determination of our overall theory of nature is not a higher-level intuition; it is integral to our under-determined theory of nature itself, and of ourselves as natural objects” (1969, 303). Only on an uncharitable reading does Quine suggest here that even the underdetermination thesis is written in the Book of Nature, or that it is a finding of natural science. This passage is perhaps not very fortunate, but clearly enough it invokes once more the inclusive notion of ‘our overall theory of nature’ which excludes nothing but “higher-level intuitions”, whatever this may be. Quine’s terminological strategy is different from Wittgenstein’s. He uses the term ‘science’ in a sense so wide that it covers the cognitive enterprises of both the natural scientist and the philosopher. They are both in the same boat, as Quine so often says. To him, science is not even to be identified with a set of true propositions, as Wittgenstein would have it. Rather, he sees science as an ongoing

truth-seeking enterprise, being defined by the procedures it employs rather than by the propositions it yields.

In his last book, Quine has something to say about the “softer sciences, from psychology and economics through sociology to history”, to which he adds in parentheses: “I use ‘science’ broadly” (1995, 49). I wish to go into the last sentence only. Using ‘science’ broadly could mean different things. In the given context, the issue is which academic subjects count as sciences. Quine’s broad notion of science indicates his willingness to accept the so-called soft sciences as sciences. “It is awkward”, he says, “that ‘science’, unlike *scientia* and *Wissenschaft*, so strongly connotes natural science nowadays” (2000, 411). His own liberal use of ‘science’ is in line with his “casual attitude toward the demarcation of disciplines. Names of disciplines should be seen only as technical aids in the organization of curricula and libraries” (1981, 88). In his view, “all sciences interlock to some extent” (ibid., 71), and his main reason for this view has always been that all sciences “share a common logic and generally some common part of mathematics, even when nothing else” (ibid.). Of the three recalcitrant ‘M’s (modality, the mental, and mathematics), only the last is indispensable to Quine, since it is indispensable to theory in physics.

Yet there is a further sense of “using ‘science’ broadly”, which is associated with the shift from plural to singular, i.e., from ‘the sciences’ to ‘science’ in the sense of ‘our overall scientific world-view’. What is sometimes overlooked is that there are *two distinct continuity theses* concerning science in Quine’s work. Both are to be found already in *Two Dogmas*. Firstly, there is his claim to continuity between philosophy and natural science, which I quoted at the beginning. Secondly, Quine holds that “[s]cience is a continuation of common sense” (1953, 45, see also 1966, 220). I submit that his sweeping notion of science in the sense of ‘our overall theory of the world’ reflects this latter continuity. Drawing his holistic conclusions in the final section of *Two Dogmas*, Quine speaks of “the totality of our so-called knowledge or beliefs” which constitute “a man-made fabric which impinges on experience only along the edges”. This fabric is what he refers to as “total science” (1953, 42).

Quine’s sweeping notion of science has some odd consequences. It has been remarked, for example, that “[i]f I want to know what time the meeting begins, or where I left my copy of *Word and Object*, what I am concerned with is very oddly described as a question of ‘science’.

Quine accepts this oddity, however“ (Hylton 1994, 265–6). We may safely reckon that most truths ever discovered were not discovered by professional scientists. So when we hear a Quinean credo such as: “We naturalists say that science is the highest path to truth” (1995a, 261), his words should be still ringing in our ears that “science is self-conscious common sense” (1960, 3). Elsewhere, Quine comments on the scientific or proto-scientific character of common-sensical investigations as follows:

The scientist is indistinguishable from the common man in his sense of evidence, except that the scientist is more careful. This increased care is not a revision of evidential standards, but only the more patient and systematic collection and use of what anyone would deem to be evidence. (1966, 233)

A good example of how the common man and the scientist share evidential standards is provided by the quarrel between orthodox and alternative medicine. An adherent of natural healing may declare: ‘I don’t care what the orthodox medical practitioners and their scientific studies say. The herb has cured my aunt, that’s enough for me.’ The disagreement here is not as sharp as it seems. Both parties plausibly do care for the effectiveness of a drug. It is merely that the scientist is not so easily convinced. The fact that aunt Mary recovered after taking the herb does not exactly meet the standards of evidence-based medicine. Strikingly, non-scientists often draw conclusions on an extremely narrow inductive base. But even for an adherent of natural healing, *post hoc* is not the same as *propter hoc*. Witnessing the next time that aunt Mary recovers without being treated at all will make him suspicious. It is just as Quine says: Both the common man and the scientist do care for evidence, but the scientist is more careful and systematic.

While Quine’s views about the continuity between philosophy and natural science are characteristic of his *naturalism*, his view that science is a continuation of common sense is very much in the spirit of American *pragmatism*. Some writers, though, regard the latter continuity thesis as a defining feature of naturalism as well.³ This view is unfelicitous. Plausibly, the naturalist’s distinguishing trait is his reac-

3. “Naturalism, as a philosophy, is a systematic reflection upon, and elaboration of, the procedures man employs in the successful resolution of the problems and difficulties of human experience.” (Hook 1961, 195)

tion in the case of *conflict* between common sense and science. Since the Quinean naturalist holds that “science is the highest path to truth” (1995a, 261), scientific investigations must in some way be *superior* to common-sensical ways of determining truths about the world. As Danto saliently points out:

Science reflects while it refines upon the very methods primitively exemplified in common life and practice. [...] Should there be a conflict between common sense and science, it must be decided in favor of science, inasmuch as it employs, but more rigorously, the same method that common sense does and cannot, therefore, be repudiated without repudiating common sense itself. (Danto 1967, 448)

For the naturalist, science is not only a continual extension of common sense, but at the same time its better half. Everything that common sense can find out science can find out as well, but science is more reliable and more accurate. Besides, scientists have special methods and tools at their disposal when things get complicated. This is why science has the last word in cases of conflict.

I return to the first continuity thesis, the naturalistic claim about science and philosophy. As explained above, it is always hard and unrewarding to dispute continuity claims. In a way, everything is a matter of degree, and every distinction can be questioned, if necessary by Sorites arguments. The issue over there being a gradual difference or a difference in kind between philosophy and science is ill-defined. Being pressed to choose between the two, I would prefer not to. Wittgenstein and Quine may both have points here. It is just that Quine always focuses on what philosophy and the rest of science have in common, while Wittgenstein is tremendously interested in determining what is *distinctive* of philosophical investigations (or of the philosophical part of scientific investigations, in Quinespeak). Unquestionably, this is a natural and legitimate concern for a philosopher. For Quine, however, it seems to be a matter of indifference.

“If Quine is right, then philosophy is an extension of science [...]. If Wittgenstein is right, then philosophy is *sui generis*.” (Hacker 1996, 33) This brusque way of confronting both philosophers seems a bit simplistic to me. Ideologies set aside, nothing prevents us from investigating both the similarities and the differences between philosophy and the rest of science. Moreover, nothing prevents us from confronting Quine’s naturalistic rhetoric with the way he actually does philosophy.

For there is a serious problem with Quine's sweeping notion of science. Wittgenstein's insistence on the non-scientific character of philosophy is a trivial consequence of his narrow and stipulative definition of 'science' as "the sum of the natural sciences". But if the word 'science' is supposed to have any determinate meaning, it must neither be used too broadly. *Omnis determinatio* being *negatio*, Quine should be prepared to explain what his inclusive notion of science does exclude. Simply declaring that "demarcation is not my purpose" (1995a, 252) will not do, since the intelligibility of his naturalism hinges on such a demarcation. Quine famously holds that "[t]he world is as natural science says it is, insofar as natural science is right" (1992a, 9). Elsewhere: "We naturalists say that science is the highest path to truth" (1995a, 261). Clearly, these declarations do not contribute to distinguishing science from non-science, but rather presuppose such a distinction. What is needed is a positive characterization of the notion of science invoked in Quine's naturalistic creeds. And, he must resist the temptation to leave it up to "science itself" to tell us what science is.

Others have not resisted this temptation. Arthur Fine, for example, holds that science will take care of itself in every respect. He advocates a "natural ontological attitude" which abstains from any "essentialist premises about the 'nature' of science" (Fine 1996, 175). Fine recommends to take science at its face value, rejecting "the mistaken idea that one must add distinctively philosophical overlays to science in order to make sense of it" (*ibid.*, 188). We may say that his anti-essentialistic scientism boils down to the maxim: *Wherever science leads, I will follow*.⁴

Now, it is not easy to distinguish between the scorning of "essentialist premises about the 'nature' of science" and the refusal to explain what one is talking about. Even avowed anti-essentialists should be prepared to clearly articulate what they mean with the words they use. Surely, Fine would not accept as science just any cognitive endeavour that anyone has ever *called* science. His maxim, "follow good science as far as science goes but do not demand that science do more" (Fine 1996, 184) at least indicates that he feels able to tell apart good science from bad science. Presumably, he would advise us to ask the good scientists about what good science is. But a charlatan or a fraud, passing himself off as a scientist, would not shy away from passing himself off as a good

4. Cf. Keil and Schnädelbach 2000, esp. 20–31.

scientist. We may hope that in the long term, he will not be accepted by the scientific community, but if he is clever enough in faking and cheating, it may take some time until he gets unmasked. What about his status up until then? Is he a good scientist just as long as the majority of his research group accepts him as a peer? Or worse yet: What if one of the next ‘science wars’ is won by the social constructivists, so that the ‘good scientists’ Fine relies on find themselves in a minority?

For a naturalist, it is not advisable to regard membership in the scientific community as a *brute* sociological or institutional fact. The truth is that the peer group has reasons for accepting or not accepting someone as a member. And such reasons will be needed as soon as the charlatan takes the university to court because of his dismissal. Fine’s defeatism regarding the definability of the science game would simply leave the scientific community empty-handed in such quarrels.

Another scientifically-minded philosopher who has not resisted the temptation in question is Stephen Stich. Stich takes exception to a widespread “puritanical naturalism” in the philosophy of mind. The search for a naturalistic criterion of acceptable properties or predicates, as carried out in the various armchair projects of naturalizing the intentional, is “misunderstanding the way that science works”, Stich says (1996, 198). According to him, there is no way of identifying the naturalistically acceptable predicates in advance, i.e., independent of the role they play in science as practiced. “What ‘legitimates’ certain properties (or predicates, if you prefer) and makes others scientifically suspect is that the former, but not the latter, are invoked in successful scientific theories. [...] [B]eing invoked in a successful science is all that it takes to render a property scientifically legitimate” (ibid., 199).

Just as Fine speaks of “good” science, Stich speaks of “successful” science. And just like Fine, Stich becomes quite taciturn when pressed for an explanation of what being a successful science amounts to. He says: “I don’t claim to have an account of what it takes to be a successful scientific theory. Indeed, I suspect that that, too, is a pluralistic, open-ended, and evolving notion.” And as to “the question of whether successful science can be constructed using intentional categories [...], it is working scientists [...] who will resolve this question, not philosophers of the puritan persuasion” (ibid.).

Again, this result is disappointing. Stich, too, seems to resort to the maxim: *Wherever science leads, I will follow*. But whatever the philosophical merits of anti-apriorism and anti-essentialism are, the advice

‘Ask the working scientists!’ cannot settle the question what counts as good or successful science. Abstaining, for fear of apriorism, from setting any methodological standards or criteria that distinguish science from humbug and charlatanism leaves us with nothing but a sociological notion of science: *Everything that can be studied at a university is a science. Or: Science is what professors are paid for. Or: Science is what you can get money for from the National Science Foundation.* But, as is well-known, weird things are taught at universities, for example that science is just another genre of literature, or that reality is but a social construction. Naturalism cannot be so liberal as to embrace these claims as *scientific* doctrines, just because they are taught at universities by tenured professors. Quine’s tenet that “the world is as natural science says it is” assumes a more ambitious, non-institutional account of what (natural) science is, even if some naturalists are reluctant to spell this out. After all, the doctrines taught at universities include apriorist epistemologies, and *ex hypothesi*, such doctrines cannot be reconciled with Quinean naturalism.

Quine once defined his naturalism as the “abandonment of the goal of a first philosophy prior to natural science” (1981, 67), and as “the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described” (ibid., 21). Late in his career, he acknowledged that “these characterizations convey the right mood, but they would fare poorly in a debate” (1995a, 251). That’s what I say as well. Eventually, Quine seems to have an inkling of how vaguely he has always described his naturalism. At last, he puts the appropriate questions: “How much qualifies as ‘science itself’ and not ‘some prior philosophy’? [...] What then *have* I banned under the name of prior philosophy?” (ibid., 251–2)

3. *Philosophy as Conceptual Analysis Within the Framework of Science: A Fresh Look at Quine’s Alleged Naturalism*

I shall now take a fresh look at Quine’s job description for naturalistic philosophers. In a survey article from the 90s, he asks himself, after giving a short description of his naturalistic epistemology, whether “this sort of thing” is “still philosophy”. His answer is that

Naturalism brings a salutary blurring of such boundaries. Naturalistic philosophy [...] 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. (1995a, 256–7)

With suitable omissions, the passage reads: ‘Naturalistic philosophy clarifies the most basic concepts and analyzes scientific method’. This is a remarkably traditional job description for philosophers. Philosophy seems to be, in a word, conceptual analysis, though “within the framework of science itself”. What precisely this addition means is anything but obvious. Does it, for example, make Quine’s job description incompatible with inherited conceptions of philosophy as involving conceptual analysis of the aprioristic kind? Quine’s earlier explications of his naturalism suggest that the addition “within the framework of science itself” is chiefly made in opposition to the epithet ‘a priori’.

The expression ‘conceptual analysis’ is not a registered trademark. There are so many kinds of conceptual analysis. Regarding Quine, the main challenge is to harmonize his job description for philosophy with what he considers to be the defining mark of the scientific enterprise, namely that it yields empirically testable predictions. For unlike Fine and Stich, Quine has a robust empiricist account of what it takes to participate in the game of science. The *prediction of observations* is the name of the game:

[W]hen I cite predictions as the checkpoints of science, I do not see that as normative. I see it as defining a particular language game, in Wittgenstein’s phrase, the game of science, in contrast to other good language games such as fiction and poetry (1992, 20; see also 1987, 159–162).

So, in Quine’s view, the fixing of empirical checkpoints is *constitutive* of the science game. Now philosophy is supposed to be part and parcel of the science game as just defined, while at the same time it is said to clarify, organize and simplify the most basic concepts, and to analyze scientific method. This double job description creates a certain tension, for arguably, the clarification of concepts and the analysis of methods do not provide empirical checkpoints of their own.

To be sure, there are varieties of conceptual analysis which do bear relations to empirical checkpoints. Strawson (1959, 9) describes his “descriptive metaphysics” as “aiming to lay bare the most general features of our conceptual structure”, and as “describ[ing] the actual structure of our thought about the world”. In Austin’s linguistic phe-

nomenology, the empirical bearings are even more obvious. Austin used to check his philosophical claims against the actual usage of the terms in question by nonphilosophical speakers. These kinds of conceptual analysis license the remark that “[q]uestions about the actual structure of our concepts are *in principle* as empirical as questions about the actual structure of iron“ (Bishop 1992, 269). The same point has recently been made by Frank Jackson:

It is an empirical fact that we use a certain term for the kinds of situation and particulars that we do in fact use it for, and the conclusions we come to on the subjects are fallible—as Gettier made vivid for us when he showed us that fine conceptual analysts like Ayer and Chisholm got it wrong in the case of the word ‘knowledge’. (Jackson 1998, 47)

Seen this way, there is a fairly direct connection between conceptual analysis and empirical checkpoints, the empirical data being the linguistic behavior of ordinary speakers.⁵ Of course, this approach is not Quine’s. Ironically, it’s just his own reservations about ordinary language philosophy which set off his preferred kind of conceptual analysis against the partly empirical project just described. Instead, Quine advocates what he calls *don’t-care analyses*, which he explains thus:

We do not claim synonymy. [...] We do not expose hidden meanings, as the words ‘analysis’ and ‘explication’ would suggest; we lack supplies. We fix on the particular functions of the unclear expression that make it worth troubling about, and then devise a substitute, clear and couched in terms to our liking, that fills those functions. Beyond these conditions of partial agreement [...] any traits of the explicans come under the head of ‘don’t cares’. (1960, 258–9, cf. 182)

It is, in other words, the *stipulative* or *legislative* character of Quine’s explications and paraphrases that prevents him from substantiating his continuity thesis in a straightforward manner. It is, of course, far from me to make Austin’s way of integrating the philosopher’s business into the science game palatable to Quine. He has good reasons to think of

5. Strawson, by contrast, speaks of “the actual structure of our thought about the world”, which might, after all, “not readily display itself on the surface of language” (1959, 10). This is why conceptual structures, unlike linguistic ones, are hardly ever reckoned empirical data. Strawson says that the descriptive metaphysician “must abandon his only sure guide when the guide cannot take him as far as he wishes to go” (ibid.). It is doubtful, however, whether a fact’s being less accessible makes it less empirical.

the way in which conceptual analysis contributes to testable predictions as being more devious.

The tension between the way Quine describes the science game and his job description for scientific philosophy is still unresolved. Which role exactly does the clarification, organization and simplification of our most basic concepts play for the business of predicting observations? To find an answer to this question, I wish to return to Quine's remarks about the so-called soft sciences in his last book:

In softer sciences [...] checkpoints are sparser and sparser, to the point where their absence becomes rather the rule than the exception [...]. Observation categoricals are implicit still in the predicting of archaeological finds and the deciphering of inscriptions, but the glories of history would be lost if we stopped and stayed at the checkpoints. (1995, 49)

Quine makes it clear that even theories in the soft sciences have *some* testable consequences. Empirical checkpoints are sparser here, but still there are some. Elsewhere, he puts it in a nutshell: “[T]he softer the science the sparser the tests” (1995a, 258).

It should be noted that the empiricism contained in Quine's characterization of the science game relates only to the output, not to the input. This is why he surprisingly is prepared to accept, in his later writings, even telepathy and clairvoyance as “scientific options, however moribund. It would take some extraordinary evidence to enliven them, but, if that were to happen”, then this “collapse of empiricism would admit extra input by telepathy or revelation, but the test of the resulting science would still be predicted sensation” (1992, 21). This is a remarkable passage. Taken as a theory about the input, empiricism would be falsified if it turned out that some information about the world reaches us via other channels than impacts on our sensory receptors. The immediate question is, though, what the “extraordinary evidence” for telepathy could consist of. What does Quine have in mind here? In order for us to *accept* any evidence for the existence of telepathic phenomena, it seems that our views about what counts as evidence must have changed beforehand. As long as the triggering of sensory receptors is the only thing that counts as a source of information, scientific evidence for telepathy is simply ruled out from the start. In Quine's view, empiricism as a scientific doctrine is itself “fallible and corrigible” (ibid.). But, the trouble with accepting any findings as “extraordinary evidence” for telepathy is that such acceptance would at the same time

change our views of what *counts* as evidence. It is difficult to see how both revisions could be undertaken with a single blow. Quine's liberalism seems to be caught up in a methodological circle.

Let us return to what Quine has to say about the soft sciences. We are told that theories in economics, sociology or history, do have some testable consequences. But now for the big question: What about philosophy? How soft a science is philosophy? Clarifying, organizing and simplifying our most basic concepts, and analyzing scientific method—which testable consequences do these activities have? Are the tests so sparse here that philosophy is even softer than, say, sociology?

This is an odd suggestion. Philosophy plays a part in the science game, but its role seems to differ in kind from the role that sociology or economics play. A parallel to the role of logic and mathematics is more easily drawn. According to the Quinean picture, logic and mathematics do contribute to the fabric of total science, but not by fixing the edges, i.e., not by supplying additional checkpoints. Rather they contribute by enabling us to knit the fabric more closely. The same could hold true for philosophy: It does contribute, but it does not contribute additional checkpoints. Therefore, *even if* philosophy is part and parcel of the scientific enterprise, and *even if* prediction of observations is the name of the game, philosophy does not belong to *that* part of science which is subject to direct empirical test. This is in principle Quine's own picture, which is sometimes obscured by his needlessly bold and crude naturalistic avowals.

In *Two Dogmas*, he famously holds that the fabric of science “impinges on experience only along the edges” (1953, 42). Yet, if we take the criterion of distance from the checkpoints at face value, mathematics and logic come out as much softer sciences than even sociology does. This result cannot be welcome to Quine. I suggest, anyway, that we do not attach too much importance to the determination of the grade of hardness of a science. The systematic significance of the soft/hard distinction as applied to scientific disciplines seems rather limited. Calling a science “hard” or “soft” is more a matter of exploiting dubious connotations.⁶ There may be kindred distinctions which are more useful, for example that between exact and inexact sciences, or that between sciences which invoke intentional notions and those which do not.

6. In the hard sciences, hard-working people discover hard facts, while soft scientists are probably soft characters, perhaps even soft in the head ...

As Quine's use of the fabric metaphor shows, the yielding of testable predictions is not a *conditio sine qua non* for participation in the science game, contrary to his suggestion in *Pursuit of Truth*. The question, *in which way* the clarification and analysis of our most basic concepts and methods contributes to our weaving the fabric of total science, is still without a positive answer.

The parallel between philosophy and mathematics is very instructive, particularly since Quine changed his view about the empirical content of mathematics late in his life. While in *Pursuit of Truth* he still held that "[h]olism lets mathematics share empirical content where it is applied" (1992, 55–6), he writes in his last book that

the participation of mathematics in implying the [observation] categoricals [...] does not confer empirical content. The content belongs to the implying set, and is unshared by its members. I do, then, accept the accepted wisdom. No mathematical sentence has empirical content, nor does any set of them. (1995, 53)

In other words, no mathematical sentence has the capacity of turning a semantic mass into what Quine dubs a critical semantic mass. Regarding the results of conceptual analyses, would therefore an analogous concession be appropriate? This much is certain: the fact that words mean what they mean contributes to the *truth* of empirical statements, in the obvious way: "The statement 'Brutus killed Caesar' would be false [...] if the word 'killed' happened rather to have the sense of 'begat'" (Quine 1953, 36). Or, if I make a random guess at the number of coins in my pocket, and the guess turns out to be correct, it would be false, according to the standard analysis, to say that I *knew* the right number. If, however, 'knowledge' happened to have the sense of 'true belief' instead of 'justified true belief (plus x)', then it would have been true that I *knew* the number.⁷

This kind of affecting truth value, however, does not by itself confer empirical content to analytical definitions. Nor does the fact that word meanings are subject to historical change, as it has been the case, for example, with the German word "Junggeselle". The fact, emphasized

7. As to the notion of knowledge: Quine's own view, that "for scientific or philosophical purposes the best we can do is give up the notion of knowledge as a bad job and make do rather with its separate ingredients" (1987, 109), is motivated by his insight that "knowing is a hybrid of warranted belief, which is mental, and truth, which is not" (2000, 415). This insight is clearly a result of good-old-fashioned conceptual analysis.

by Quine, that “no statement is immune to revision” (1953, 43) has no relevance whatsoever to the issue of analyticity. A change in meaning cannot retrospectively deprive a sentence of its analytical character. Quine himself conceded in his later years that analyticity must be dissociated from incorrigibility (cf. 2000, 415).

In a way, the results of conceptual analyses do make a difference to the truth-value of synthetic statements. However, they do so because they *reflect* semantic facts, not because they *generate* them. Conceptual analyses can only *reveal* facts about meaning which have been previously established by the speech community.⁸ Or facts about usage, as Quine and Wittgenstein would prefer to say: Conceptual analysis well-understood is a matter of “fluctuant usage to be averaged out”, not of “intrinsic meanings to be teased out” (Quine 1992, 55). And, of course, an analytical definition alone never *implies* a synthetic truth. It is just as Quine says with respect to mathematics: “No conjunction or class of purely mathematical truths, however large, could ever imply a synthetic observation categorical. [...] Every critically massive set of truths has some nonmathematical members.” (1995, 53)

Recognizing the difference between yielding testable predictions and merely belonging to an implying set is crucially important to the re-evaluation of Quine’s tenets about the deliverances of science. The clarification and analysis of our most basic concepts may be viewed as part and parcel of the scientific enterprise, if we embrace Quinespeak, but still it does not belong to that part of science which directly faces the tribunal of experience.⁹ It belongs to the more general and more abstract part of science which some non-naturalist philosophers like to call *a priori*. The empirical checkpoints being miles away (below the horizon, as it were), philosophers have to *give arguments* for their claims instead. As Quine has put it: “Having reasonable grounds is one thing, and implying an observation categorical is another” (1994, 497). Devising “reasonable grounds” is essential to the philosopher’s business, while coming up with analytical definitions is little more than

8. See Hanjo Glock’s contribution to this volume, sect. 5, where he rephrases what Yablo has called “the Lewy point” as the insight that conventions and meanings do not *create* truths.

9. This line of reasoning assumes that the epistemological holism from *Two Dogmas* is an exaggeration, as Quine himself often granted in later years: “Excessive holism surfaced only back at midcentury in my pioneer ‘Two Dogmas’, in a regrettably eloquent passage that readers never tire of quoting” (1997, 572).

preparatory work in this business. A philosopher's job is by no means accomplished when he has framed a sentence like 'Knowledge is justified true belief', or 'An action is a bodily movement caused in the right way by rationalizing beliefs and desires'. After having written down such a definition, the philosopher's job has only just begun. He has to argue in its favour, defend it against competing proposals, coordinate it both with the linguistic data and with speakers' intuitions about "what we should say when" (Austin) and check its implications for previously accepted analyses of other concepts. Sometimes, he may even have to explain why some basic concepts, perhaps 'truth', 'identity', or 'existence', are too elementary as to admit of analytic definitions. Whatever the fate of analytic definitions proper, conceptual analysis in a wider sense will continue to be a major part of the philosopher's business, which is a business of devising reasonable grounds for claims abstract enough to count as philosophical in nature.

Quine's talk about "science itself telling us" remains puzzling because it covers up the difference between being *an empirical finding* and being *a reason which has some bearing on science*. One last example from ontology will illustrate my point: As to the similarities between existence statements such as 'There are tigers' and allegedly philosophical existence statements such as 'There are numbers', Quine says:

Existence statements in this philosophical vein do admit of evidence, in the sense that we can have reasons, and essentially scientific reasons, for including numbers and classes or the like in the range of our variables. [...] Numbers and classes are favoured by the power and facility which they contribute to theoretical physics and other systematic discourse about nature. (1969, 97–8)

Saying that "we have reasons, and essentially scientific reasons" for accepting numbers and classes sounds much better than taking it from "science itself" that such entities exist. But still: In which sense are the reasons scientific ones? A moment's reflection reveals an ambiguity in calling a reason 'scientific'. A scientific reason might be *a justification which is somehow related to scientific purposes*. This is obviously what Quine has in mind here.¹⁰ A scientific reason could as well consist of

10. In general, Quine is far from maintaining that all questions about what there is are questions of empirical fact. Rather, the "issue over there being classes" is aligned with "the issue over there being brick houses on Elm street" in the name of "a more thorough prag-

a direct appeal to an empirical finding, that is, to a fact discovered by empirical science. While it's clearly the former that Quine has in mind, it's the latter that everyone thinks of, and rightly so, when getting informed that science itself tells us this and that, or when being told, as we are in *Word and Object*, that it's "the business of scientists" to determine "what is real", and that "scientific method is the last arbiter".

So, more attention must be paid to the difference between *citing an empirical finding* as if it spoke for itself, and *giving an argument* which is somehow *related to scientific purposes*. (Some philosophers go so far as to call facts themselves reasons, but that is another story. I adhere to the view that scientific findings and facts are never justifications by themselves, though they can, of course, be *appealed to* when *giving* a reason, in philosophy and elsewhere.) I find Quine's declaration unobjectionable that in coping with philosophical problems we are always "free to use scientific knowledge", when interpreted along these lines. "All scientific findings" he says, are "as welcome for use in philosophy as elsewhere" (1969, 127). This avowal stems from his paper "Natural Kinds", where he appeals to Darwin's natural selection as "a plausible partial explanation" (*ibid.*) of the fact that human beings are innately disposed to make, by and large, inductions which are correct. On closer scrutiny, however, his appeal to the findings of evolutionary theory plays a minor role than it seems. Quine does refer to innate similarity standards, but the crucial argument in his paper is about the *projectibility of predicates*, thus "entirely philosophical in character", as Susan Haack noticed: Quine "proposes a solution—that only natural kind predicates [...] are projectible—which involves no appeal to evolution (or to any scientific work), but is entirely philosophical in character" (Haack 1993, 133).

Hence, Quine's appeals to 'scientific findings' and 'scientific reasons' are in need of a careful interpretation. More often than not, these appeals do not consist in merely citing an empirical finding, but are embedded in a philosophical argument which is in some way related to scientific purposes.

matism", since ultimately, *all* kinds of entities "enter our conception only as cultural posits" (1953, 46 and 44). At the same time, he accepts all entities posited by science as "utterly real denizens of an ultimate real world" (1995a, 260). To him, the posits are nonetheless real. Quine sees no clash between his constructivist pragmatism and a robust realism. Ontological questions inevitably being answered from within a scientific theory, calling the entities posits and calling them real is just a matter of changing the perspective.

4. Conclusion

Hence, as long as we keep aware of the difference just explained, we need not be intimidated by Quine's frequent appeals to "science itself". It sometimes may look as if Quine appealed to the authority of natural science to establish philosophical tenets, but this impression is due to a superficial reading, one which is encouraged, though, by some bold formulations of Quine's.¹¹

Quine holds that the yielding of empirically testable predictions is constitutive of the science game, and that nonempirical truths are scientifically respectable if and only if they belong to sets of truths that imply observation categoricals. This view is of course not a deliverance of empirical science, but rather stems from his *philosophy* of science, a philosophy which even the formula "Empiricism without dogmas" is not an adequate expression of. For when it comes to the question whether and how nonempirical disciplines such as mathematics or philosophy contribute to our overall scientific picture of the world, *empiricism itself* is a dogma. Quine's concession that some distance from the checkpoint is allowed does not change the situation, because the concession, and his reasons for it, are not by themselves empiricist tenets. If any -ism is called for, they are more rationalistic than empiricist in spirit.

I have argued that, given Quine's inclusive notion of science, his addition "within the framework of science itself" to the job description for philosophy has an innocuous reading, which at the same time strips his avowed naturalism of real bite. In general, Quine's notion of naturalism is not as elaborate as, say, his notion of observation sentence. He regarded names of philosophical positions as "a necessary evil" (1995a, 251), and he "tend[ed] to be impatient with the quest for

11. Here I agree with Pihlström and Koskinen (2001, 14) who suggest "a more modest interpretation of the metaphilosophical implications of [Quine's] naturalized epistemology". They characterize Quine's naturalism as "a holistically empiricist metaphilosophical view about the nature and role of philosophical knowledge" (ibid., 2). In particular, they discuss the issue of *normativity*, which I have neglected. In case we are forced to revise our web of belief in the face of recalcitrant experience, Quine is known to appeal to the principles of *simplicity* and *conservatism*. Both are clearly normative principles, which cannot be read off from the descriptive content of scientific theories. Quine's rejoinder that this normative element "gets naturalized as a chapter of engineering: the technology of anticipating sensory stimulation" (1992, 19) simply misses the point, for it cannot answer the crucial Humean question of where on earth the normative 'ought' is to be taken from.

precision in the names for disciplines and schools of thought: in asking what really counts as naturalism, epistemology, physics” (2000, 411). We should not, however, let him get away with this nonchalance, given the huge impact his naturalistic avowals had on the scientific community. When Quine defined naturalism as “abandonment of the goal of a first philosophy prior to natural science”, it was far from clear what he did ban under the name of prior philosophy. He did not ban speculative metaphysics, nor conceptual analysis per se, nor the soft sciences. What one finds are mainly tendentious and polemical formulations such as having “higher-level intuitions” (1969a, 303), speaking from a “cosmic exile” (1960, 275) or establishing “a supra-scientific tribunal” (1981, 72). (The last remark might be an misconceived allusion to the Kantian metaphor of the court of reason.)

The upshot of my discussion is that Quine’s naturalistic rhetoric does not show him at his best. There are not many philosophers left at whom Quine’s criticism of philosophical apriorism could be aimed. Hardly any first-rank philosopher does appeal to “higher level intuitions” when making *a priori* assertions—neither did Leibniz, nor Kant, nor Wittgenstein. On the other hand, Quine’s own writings clearly belong to philosophy and logic, and not to some other scientific discipline. Even if names of disciplines should be nothing but “technical aids in the organization of libraries” (1981, 88), librarians know pretty well on which shelf Quine’s books are to be placed. “Why not settle for psychology?”, he suggestively asks in “Epistemology Naturalized” (1969, 75). Now, did he settle for psychology? Did he accord “a physical human subject [...] certain patterns of irradiation in assorted frequencies” as an “experimentally controlled input”, so that his epistemology “simply falls into place as a chapter for psychology, and hence of natural science” (*ibid.*, 82–3)? No, he settled for philosophical speculations about what behavioristic psychology *might* find out about the roots of reference.¹²

12. This has been noted before: that “Quine’s attempt to trace a path from stimulation to science is carried out with almost no concern for the psychological reality of the process, that is, with almost no references to empirical investigations of how language is actually acquired. [...] Quine’s inspiration comes from the library, not the laboratory. [...] Launched from a starting point of debatable empirical significance—observation sentences taken holophrastically—Quine’s program proceeds with almost no contact with the empirical checkpoints central to doing philosophy in a naturalistic spirit” (Fogelin 1997, 561–2).

Steve Stich has emphasized that “Quine offered a new job description for philosophy”.¹³ I agree with him only under the qualification that changing a job description is one thing, while providing somebody with a new job is quite another. Quine’s celebrated blurring of the boundary between philosophy and natural science is a piece of naturalistic rhetoric which fortunately had little effect on the way he actually did philosophy. The real tension exists between his empiricist definition of the science game and the non-empirical nature of mathematics, logic, and major parts of philosophy. These parts of the scientific enterprise enable us to knit our web of belief more closely without contributing additional empirical checkpoints.

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