

Naturalism and Normative Science

Henrik Rydenfelt

Abstract: Naturalist views in philosophy often combine a Quinean denial of a first philosophy – the idea that science cannot be based on a foundation more secure than itself – with the (metaphysical) position that science is our best or privileged means of finding out what there is. My task here is to inquire what the status of the latter *normative* claim is in light of the former. Based on a reading of different contemporary “pragmatist” philosophers – most notably Quine, Putnam, Rorty and Price – I will argue that philosophical naturalism lapses either back into foundationalism or to a heavily qualified internal realism. As a remedy, building on Charles S. Peirce’s conception of normative science, I will suggest that the problem is circumvented by opening the possibility of a scientific inquiry into normative issues.

Introduction: The “natural” in naturalism

In its simplest form, *metaphysical* naturalism amounts to pretty much the same as the denial of the supernatural. If this were all there were to naturalism, one way of making sense of the view would be simply to list what there *is* in our “natural” world: to say that there are just physical particles and their properties, or that there are also chairs, tables, people, cats and mats and the like, taking these common sense objects to be in some manner based on the more profound scientific realities. In any case there are no such things as fairies, demons, angels, spirits, souls or Platonic ideas.

However, such an *inventory* conception of metaphysical naturalism would surely be judged strange, and for obvious reasons. It is not a particular list of basic “things” as postulated by contemporary science that naturalism concerns. Instead, metaphysical naturalism, as usually understood, is the *deferral* of ontological questions to science: it is the view that science should decide upon what there is and is not, usually with the additional clause that science already gives a pretty good idea of this. The “supernatural” amounts to exactly those things whose existence cannot be countenanced by (natural) science.

A distinction has often been drawn between metaphysical naturalism and a closely allied but separate philosophical position, *methodological* naturalism. Metaphysical naturalism, it is usually held, concerns the ontological question of what there is, while methodological naturalism maintains that philosophy should (to an extent specified) follow scientific methods (cf. Papineau, 2007). Depending on the reach of scientific method, there are methodological naturalisms of varying strengths. Weaker versions only claim that philosophy should follow or

mimic science, especially the scientific method, in solving its own set of problems; while stronger versions look for answers to philosophical questions from within science, or even suggest abandoning philosophical issues when no scientific answer is forthcoming.

Methodological naturalism might be delimited to a certain area or aspect of philosophy and is thus relatively independent of metaphysical naturalism. Naturalist approaches to philosophical questions can be advanced in any field, but metaphysical naturalism is not a necessary condition for the success of this move. Metaphysical naturalism, however, is dependent on at least one particular and strong standpoint of the methodological variety: metaphysical questions are to be answered by science. In any case, and this is the simple but crucial point, it is, according to metaphysical naturalism, *up to science* and not to philosophy to find out what there really is.

Different combinations of metaphysical and/or methodological naturalism have yielded the different sorts of philosophical naturalisms discussed today. However, it is perhaps not the various positive claims that naturalists have made about the position of science and the dependence of philosophy on science that have attracted most attention. Instead, it is the *negative* naturalist claims concerning philosophy – about what philosophy *cannot* do – that are the source of much of the interesting debate concerning naturalism. The most prominent claim of this sort is, of course, Quine's famous denial of a *first philosophy*: the repudiation of “a foundation for scientific certainty firmer than scientific method itself” (Quine, 1992, p. 19).

Quine's view combines different elements of methodological and metaphysical naturalism. The essential metaphysical claim is that science provides the best available account of what there is. Methodologically speaking, scientific issues are to replace traditional philosophical problems. Science should decide what central epistemological concepts amount to, or what (if anything) they correspond to in the natural realm. In particular, instead of the traditional normative notions of knowledge and justification, and concepts in philosophical psychology such as thought and belief, we should be talking in terms of empirical psychology and neuroscience about such matters. “Epistemology,” in Quine's famous phrase, “is contained in natural science, as a chapter of psychology” (Quine, 1969, 89).

Methodologically speaking, then, Quinean naturalism involves a commitment to a strong form of naturalism: philosophical questions are to be solved by scientific means, if there are such questions – if philosophical concepts have a place in the naturalist world-view – in the first place. Some philosophical notions are to be studied in light of advances in (natural) science while still leaving some room for distinctly philosophical inquiries – defined by their broad compass rather than a specifically philosophical standpoint – whilst others are to be

replaced by scientific notions studied by different (special) sciences, and others again to be rejected, since they are not worthy of scientific study at all.

Combined with the negative thesis of first philosophy, metaphysical naturalism is inevitably faced with two questions. The first concerns the nature of science itself. Perhaps we can defer questions about what there is to science, but then what is science? The second concerns the position of the naturalist thesis itself. Perhaps science should act as an arbiter of what there is, but what sort of status does this claim itself have? Both of these questions turn out to be difficult to answer without a theory in philosophy proper. For the time being, I will set the first question aside, understanding science as simply the actual project of inquiry we usually call by that name, and concentrate on the latter problem: the status of the naturalist thesis.

Where does the naturalist's negative thesis stem from? Obviously, such a view is easily refuted if it is put forth as a distinctly philosophical claim – as a doctrine in methodological naturalism. For then it would be the *philosophical* position *that* philosophy has no place in deciding what there is. Such a denial of first philosophy would be self-undermining – indeed itself a thesis *in* first philosophy. The remaining option – the one taken by Quine – is to say that the denial of first philosophy is itself a scientific view. What needs to be examined, then, is what sort of a scientific claim that could be.

The point so far is plain and simple. Metaphysical naturalism as such is not problematic: it leaves room for philosophy to develop views on why exactly it is up to science to decide what there is. However, when combined with a denial of first philosophy – in effect the methodological naturalist claim that philosophy is in no position to decide *about* the position of science – we are left with two choices: either the claim is philosophical, and thus self-undermining, or it is scientific. The first choice is untenable, but what of the second?

Science and justification

It is clear that Quine thought that his naturalism is not guilty of self-referential problems; instead it is a scientific claim itself. But such a view is immediately faced with a number of interrelated problems, three of which I will elaborate on here.

Firstly and most importantly, there are the limitations of Quine's view of science and, hence, philosophy turned science. Quine however emphasizes that his view retains a place for the normativity of epistemology:

To emphasize my dissociation from the Cartesian dream [of a foundation for scientific certainty firmer than scientific method itself], I have written of neural receptors and their stimulation rather than of sense or sensibilia. I call the pursuit naturalized epistemology, but I have no quarrel with traditionalists who protest my retention of the latter

word. I agree with them that repudiation of the Cartesian dream is no minor deviation. But they are wrong in protesting that the normative element, so characteristic of epistemology, goes by the board. Insofar as theoretical epistemology gets naturalized into a chapter of theoretical science, so normative epistemology gets naturalized into a chapter of engineering: the technology of anticipating sensory stimulation. (Quine, 1992, p. 19)

Although prediction (or anticipation) is perhaps not the most central business of science, in Quine's view good science is still defined by its successes in predicting sensory stimuli. We can perhaps agree with Quine (with some qualifications) that science is concerned with the explanation and understanding of sensory stimuli. But if this is all science does, his critics are certainly correct that normativity goes by the board. The rejection of philosophical foundationalism itself does not imply that we cannot have a *non*-foundationalist yet normative epistemology. But as Quinean epistemology is the “technology of anticipating sensory stimulation,” it simply has no answers to questions concerning justification – the sort of questions that are of interest here.

Secondly, and more practically speaking, it seems actual scientists are hardly occupied with an inquiry into whether and why their project enjoys the privileged position naturalism claims for it. Either they take this as a matter of course, or simply refrain from considering the issue at all. It often seems as though science refers such questions back to *philosophy*, either as something of very limited interest from the scientific perspective, or as something that is better left to those who wish to engage in such sophistry. Scientists, as Quine would surely have to admit, usually have no interest in such normative issues.

Thirdly and finally, what makes matters even more complicated, is Quine's consistent commitment to a strong form of ontological physicalism throughout his writings. His ontology consists of physical objects and sets. Physicalism effectively excludes normativity, which is especially evident in Quine's replacement of traditional epistemological notions such as thought and belief with ‘scientifically respectable’ talk of neural states and receptors. Such a view leaves no room for normative notions of truth and knowledge. To be clear, there is nothing paradoxical in theories of physics or a physicalist world-view as such. But the claim that the physicalist world-view has a status that is privileged, or is better than its alternatives, cannot be a part of these theories or world-view.

The crucial lesson of these three claims is perhaps best put in reverse order. Within a physicalistic world-view, normativity has no role – matters of correctness and justification do not figure in the explanations of the physical sciences from which epistemological “engineering” draws. Scientists, then, have usually had no *explicit* concern with such issues. And thus by extension is epistemology naturalized only concerned with explaining the way the stimulus of neural receptors gives rise to theories about the world, or the procession from stimulus to science, rather than the justification of that process. There is no scientific theory

that would justify science – the sort of theory that would be required as a *scientific* equivalent or replacement of the project of first philosophy.

An evident remedy to this condition is to say that science itself could be such that it explicitly takes up normative issues – the possibility I will below discuss under the label of *normative science*. Almost needless to say, what has made the possibility of such a remedy seem eminently implausible to philosophers, it seems, is the basic assumption that normative questions do not appear in science, for there are no normative “facts” to be discovered – an assumption that looms behind Quinean naturalized epistemology. While making sense of such normative facts is beyond the scope of this paper, this is actually my preferred version of naturalism. But before proceeding to the normative question itself, a different way of looking at naturalism should be considered.

Naturalism as science

In the foregoing, the problems with Quinean denial of first philosophy and the privileged position of science were largely due to the fact that while those claims concern the position of science itself, it is difficult to find a place for them inside science. But perhaps there is another way of thinking about Quinean naturalized epistemology. Instead of making any normative claims, Quine’s suggestion can be read as offering a *scientific theory* about those concepts and their place in the natural order. Stripped of any normative claims, epistemology naturalized might amount to nothing more than a first-order, scientific view about central epistemological concepts and ideas. Such a view would not face the problems inherent in a “*philosophical*” view of naturalism, as it would not purport to say anything philosophical in the first place.

Obviously, this is a somewhat artificial approach to Quine’s position. Naturalized epistemology is not a *particular* scientific theory – e.g. a psychological and neuroscientific theory about perception – but rather a view about what sort of theories are to solve questions that philosophers have previously been occupied with. On the other hand, sometimes Quine’s views, in spite of their forming part of professional philosophical discourse, seem to amount to just such first-order theories. Far from advancing traditional epistemological notions, Quine’s task is to present a scientific theory of how we understand, learn and use language. The naturalist project could be viewed as the attempt simply to replace philosophical notions by talk in scientifically respectable terms.

In Quine’s behavioural theory of meaning, understanding is a matter of linguistic behaviour, and learning language the assuming of behavioural dispositions. Meanings are not items in a “mental museum,” Quine held: “There is nothing in linguistic meaning beyond what is to be gleaned from overt behavior in observable circumstances” (Quine, 1992, p. 37; cf. Quine, 1992, p. 110).

What people mean by terms and sentences are, then, to be discovered by observing their behaviour, especially the acts of assent and dissent. The behavioural theory of meaning results in Quinean indeterminacy of reference: translations are indeterminate when we cannot, by the speaker's behaviour, tell exactly what is meant – in Quine's famous example, when we can translate a speaker's asserting “gavagai,” when rabbits are present, as “rabbit” or “undetached rabbit parts.”

Meanings being just behaviours, Quine's project of naturalized epistemology largely turns out to be the explanation of language, or a child's learning a language, ultimately arriving at scientific theories and the whole of science itself. In his view, a child learns a language by coming to master appropriate responses, or dispositions to such responses, to sensory stimulation. Such responses Quine calls observation sentences. To master an observation sentence is to be prepared to assent to it when the appropriate stimulus is present. But when is a response appropriate to a stimulus? In brief, Quine's answer: when the speaker is prepared to assent to the observation sentence (cf. Quine 1992, pp. 62-65)! As there is no notion of “appropriateness” other than the actual stimulus-responses of language-users, Quine must draw from the actual behaviour of the agents. This takes his view beyond the mere idea that behaviour is all there is to “meaning” by adding that such behaviour is constituted by dispositions to react to stimuli.¹

This Quinean image of science effectively eschews normative notions. Indeterminacy of reference as presented above is pretty much the same as the under-determination of our *theory* of what the terms of a language refer to: the linguistic and other behaviour of language-users allows several empirically adequate theories of what they mean. Talk about the “correct” theory has no place in this picture. Indeed, set alongside the view that language is explained by showing how we come to be disposed to certain responses to certain “surface irritations,” it is not because of under-determination that there is “nothing to scrute”; it is because there are no (“cognitive”) meanings in the first place. As a consequence, there is no more question of making mistakes and errors in using language and proceeding from stimulus to science than in receding ice's eroding a canyon or a massive star's exploding into a supernova. In such a view words are not *signs* of objects; they are reactions or responses to stimuli.

Internal successes

Quine's naturalized epistemology gives a rudimentary image of how we arrive at language and the assertion of the whole of science – an image that is then hoped to turn into a full-fledged explanation after considerable advances in neu-

1 To be precise, in Quine's view, the *explanation* of meaning – the behavioural responses to stimuli – is supposed to be given on a more fundamental neuro-scientific level.

roscience. Such a view involves no genuinely normative notions. To repeat what was already said in connection with physicalism, such views are by no means contradictory. Indeed, what Quine did with meaning and reference could be done, and has been attempted, with other philosophical and epistemological concepts, such as belief, truth, intention, value, etc. But from a slightly more philosophical standpoint, there still remains a problem which is perhaps best put forth by considering the conclusions drawn from a similar background view due to Hilary Putnam.

In the early 1970s, Kuhn (1970) (at least as he is commonly read) questioned the idea that science progresses, in any simple terms, towards ever ‘truer’ theories. Instead, when a background theory (or paradigm) changes, Kuhn argued, the meanings of central terms in that theory undergo similar changes, and hence two competing theorists would be talking of different things instead of disagreeing over the same thing. This criticism was reinforced by the likes of Feyerabend (1975), who questioned the idea of any kind of progress in science. In response, the so-called causal theories of reference of Kripke (1980) and Putnam (e.g. Putnam, 1988) were used to make sense of how we fix the reference of scientific terms so that the meaning of such terms is not merely up to the theories that involve these terms. According to these theories, meanings are not just “in the head”; instead, things in the world have caused us to have certain words for them, and using these words, we manage to refer back to those things in the world, despite differences in our theories of what those things are. The causal theory of reference is, then, essentially a scientific theory about the causal relationship between language and reality.

However, as Putnam (1978, pp. 123–125; 1983, pp. 17–18) noted, the fact that our words are causally linked to our expressions is itself a postulation of scientific theory. The situation at hand is exactly analogous to scientific theories explaining the success of science – science explaining itself. As theories of perception may explain the reliability of a certain type of perception by relying on their lawful connections to the world, theories of reference make sense of how our concepts denote independent realities by referring to the causal connections between such realities and concepts.² But the success of those theories is dependent on those perceptions and concepts themselves. From this, Putnam drew the conclusion that such realism is “merely” *internal*. The causal theory of reference relies on itself: *by* that theory, the terms of that theory refer by their causal connections to the world. It was in this manner that Putnam, in the late 1970s and early 1980s, combined his “semantic externalism” with ontological internalism.

2 This idea of reliable processes has had its famous applications in the theory of knowledge such as Alvin Goldman’s (1967) reliabilism about justification. But Putnam opposes, I think rightly, the idea that such views could give a non-circular account of justification.

Putnam's internal realism seems to be largely due to two considerations. The first has to do with issues of reference more generally. Nothing precludes that there could be other scientific accounts or sets of theories of reference that can successfully explain themselves. Perhaps such self-explaining theories only describe the world as it is "for us." What is missing from the picture are meaningful ideas of how scientific terms can refer to an reality independent of ourselves, and how scientific theories can discuss that which is as of yet unknown to us. A more basic philosophical theory of reference to what is independent of and unknown to us would be required to make this connection intelligible. This is no easy feat in its own right, and cannot occupy us here.³

Of more importance in this discussion is a second (less "metaphysical") consideration, which in the broadest of terms concerns the connection between the success and "privilege" (or *truth*) of science. The fact that scientific theories have turned out to mutually support and converge with one another was by no means inevitable. In addition, the success of science is not a wholly internal issue. Science explains more than just its own theories: its predictions can successfully be applied outside of its investigative core in, e.g., engineering and technology. Such success, it could be argued, cannot be coincidental. And thus it has been suggested that the success of science is explained by the fact that its theories are true (Boyd, 1983). By Putnam's own, earlier "miracle argument", realism is the only philosophical view that does not render the success of science a miracle (Putnam, 1975, p. 73). By analogy, then, it would be a miracle if our best scientific theories were not at least approximately true. However, such an argument is contingent upon the acceptance of the theory that what *explains* the success of science is truth, or more precisely, that truth is that which (perhaps among other things) explains convergence and success. In Putnam's terms, this is "just more theory".⁴

In more general terms, the success of science and the question of its privilege are separate issues – and this is just to return to the original problem of justification. The fact that science has been successful in this way does not as such have bearings on the more pertinent question of the status of science as the

3 Putnam (1978, pp. 123-125) distinguishes three ideas – or three denials – that go together in the mixed and difficult doctrine of internal realism: (1) we cannot conceive of a reality completely unknown to us; (2) we cannot conceive of how the whole of our "representation" or scientific body of theory can refer to an independent reality; and (3) truth cannot exceed our best theory. My discussion here is mainly related to (2) and (3). For Putnam of the internal realist period, these notions seem to go hand in hand, but it seems feasible to accept only one or two of them while renouncing one or two (cf. Short 2007, pp. 199-200).

4 The phrase "just adding more *theory*" appears in connection of the causal theory of reference in Putnam's so-called model-theoretical argument (Putnam, 1980). Instead of drawing from its specific use in that still hotly debated argument, here the phrase is employed more broadly.

(cognitively) privileged human project, in particular as our best way of finding out what there is. Quine himself seemed to understand this predicament, which he however seems to have found rather trivial and relatively benign:

[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. A sentence's claim to scientific status rests on what it contributes to a theory whose checkpoints are in prediction. (Quine, 1992, p. 20)

Practically speaking, science “works”. But without the premise that what works indeed is privileged, we cannot infer that its game is to be preferred. A moment's philosophical reflection spoils the nice scientific picture. What is to be said in favour of the scientific view in comparison to “other good language games”?

A loud quietism

Replacing the traditional epistemological project with a naturalized one of Quine will, at its best, admit and explain the success of science but will not completely satisfy our philosophical questioning of the justification of the privilege of the scientific project. To this recurrence of the normative problem, however, there is an obvious alternative: one that simply will not allow that moment of philosophical reflection. One influential position in recent philosophy comes close to just such view – Richard Rorty's “neo-pragmatism”.

Rorty's background motivation derives from problems that have their origins in philosophy of language – questions of reference, meaning and representation of “reality” (cf. Gustafsson, this volume). However, his view can arguably be seen as turning on the normative question of how some particular human project, like science, could have the sort of privileged position already discussed (Rorty, 1979, pp. 176-179). In a nutshell, as no answer to this question seems forthcoming, Rorty suggested abandoning the whole idea of a privileged perspective, or faithful representation of reality. This is Rorty's doctrine of anti-representationalism, which is essentially the suggestion that there is no privileged language game or, in Rorty's terms, “final vocabulary” – there is only the game that prevails.

To some, like Putnam (1990, pp. 22-23), Rorty's suggestion seemed to amount to a variant of relativism. Rorty however protested, positioning his view resolutely beyond a realism vs. relativism debate: such debates, from his perspective, can only arise within a representationalist framework (Rorty, 1991, pp. 50-54). It would be tempting to call Rorty's view about such matters ‘quietism’,

were it not for his anti-representationalism not being exactly silent about how our language games are related to reality: *not* as faithful representations. Neither is Rorty silent about traditional epistemological notions; instead, he proposes several different accounts of what truth (and related normative concepts) will amount to in an anti-representationalist framework. Both of these two “non-quietist” facts about Rorty, beginning with the latter, will be central to my discussion here.

There is an ongoing discussion about what Frank Jackson and Huw Price among others have called *placement problems*. By these they mean the contemporary naturalist’s problem of fitting all of our different vocabularies or language games – such as those of morality, modality, causality etc. – in with the “truthmakers” that scientific language, our privileged language game according to many metaphysical naturalists, supposedly represents. I will soon return to these problems in my discussion of Price’s subject naturalism. But here there is another sort of problem, which I would like to call the *replacement problem*: if *none* of our vocabularies “represent,” what are we to say about notions such as truth and reference in the anti-representationalist framework?

Rorty obviously has plenty of say on truth, and his position on the issue developed over the years. There’s Rorty’s *pragmatist* view about truth as a matter of coping from the early 80s (e.g. Rorty, 1982, pp. xv-xvii, 162-166). Then there is the *ethnocentrist* view of the late 80s and early 90s (e.g. Rorty, 1989, pp. 50-53, 196-198). In his later writings of the 1990s, Rorty attempted to advance more consistently an explicitly “minimalist” account of truth largely derived from Donald Davidson, despite his admission that he (as a Jamesian pragmatist) “swings back and forth between trying to reduce truth to justification and propounding some sort of minimalism about truth” (Rorty, 1998, p. 21; cf. Ramberg 2007). Because of this, Rorty argues that truth offers no goal to which we may strive over and above warrant (or *justification*, which is something like truth of the ethnocentrist view), and thus that the whole notion of truth is redundant, and better left out of our vocabulary. Again, Rorty’s motive for this move lies in his commitment to liberalism: the redundant goal of truth is to be *replaced* by the more important goal of liberalism and solidarity.

However, if Rorty were right and dropping the notion of truth would make no difference in practice, the implication seems to be that nothing really changes if we stop talking about it.⁵ It becomes unclear what anti-representationalism amounts to in addition to this suggestion. Perhaps we can drop the *word* truth, but this, by Rorty’s own admission, makes no difference in practice. It seems

5 Of course, a more plausible view would maintain that the notion of truth *does* play an important role in our practices. But if this is so, the implication seems to be that the notion of truth must, after all, be a meaningful one, not something completely redundant, and not quite as easily replaced by another notion as Rorty would like to maintain. (Cf. also Price 1998; 2003.)

justification does all the normative work needed to account for our (conversational) practices. Is the whole anti-representationalist position redundant? What does replacing truth with solidarity amount to? The Rortian anti-representationalist's response to this questioning would probably be that there is something important that the whole position is meant to deny. According to anti-representationalism, there is no such thing as privileged representation: our vocabularies, while they may have justificatory connections with *one another*, cannot be privileged in the sense that they would represent "reality". The anti-representationalist may add that this sort of representationalist weight that is usually attached to the notion of "truth", and that it is in this sense that he suggests we stop talking about truth.

But then another problem arises: how is this view supposed to be *true*? Even if we drop out the notions of truth and representation altogether and do not ask how anti-representationalism is true or represents reality, the question still remains in a simpler form, namely: why prefer anti-representationalism to its alternatives, such as representationalism? What sort of a privilege does the view itself have?

One way of responding to this question is simply to write it off as misguided. Rorty's purpose is to make a vocabulary shift in favour of liberalism, for which, by his own admission, there is no philosophical justification. Instead, the issue is one in philosophy turned cultural politics. However, this is not quite the whole story: often Rorty attempts to make a distinctly "philosophical" point in favour of anti-representationalism. As we already saw, unlike Quine, Rorty thought that from the fact that the language game of science is a language game or vocabulary among others, it follows that the scientific game enjoys no sort of privilege among the games we may play. But quite like Quine, Rorty retains the key *naturalist* idea that we are connected to the world only causally. There is no bridge from the domain of language games or vocabularies – the realm of conversation, justification and "rationality" – to the world of cause and effect. Naturalism underwrites Rorty's *historicism*: there is nothing supra-historical to be said, or non-historically "rational," about the change from one vocabulary to another. As Rorty at one point put his conclusion on Quine: epistemology and ontology never meet (Rorty, 1979, p. 202). Normative connections are internal to vocabularies or between vocabularies; but none of these connections are with the world, so to speak. Rorty often labels his naturalist position "Darwinism", by which he means

[...] a story about humans as animals with special organs and abilities, [...]. According to this story, these organs and abilities have a lot to do with who we are and what we want, but have no more of a *representational* relation

to an intrinsic nature of things than does the anteater's snout or the bowerbird's skill at weaving. (Rorty, 1991, pp. 47-48)⁶

This is the view that underlies anti-representationalism. But paradoxically this threatens to turn the anti-representationalist position, as developed by Rorty, into just another sort of “first philosophy” or foundationalism.⁷

In effect, Rorty’s naturalism is a reversal of Putnam’s internal realist view. The latter took the fact that we are causally related to the world as internal to the successful language game of science. The former understands the world as simply that of causal relations, and normativity, in its turn, as a matter of the relations between items in vocabularies. This difference could be roughly put as follows: starting with epistemology, Putnam never arrives at ontology (a view of what is there independent of our theories and descriptions); starting with ontology, Rorty never arrives at epistemology (a view of how theories and descriptions can be justified by reference to what there is). Denying that there is a privileged language game but retaining a naturalist ontology will lead to anti-representationalism; starting with a privileged naturalist position but denying that this position represents the true structure of the world will lead to internal realism.

Subject naturalism

So far, my discussion has centred on the question of how science might justify naturalism – its own position as a privileged perspective on the world. Quine’s view that (“normative”) epistemology becomes a chapter of science led to Putnam’s internal realism about the language game of science. On the other hand, Rorty’s anti-representationalism, which held that science is just one language game among many, started out with a scientific or physicalist ontology. This interplay of epistemological, semantic and ontological issues leads to wonder whether it would be possible to drop the idea that science would need to be judged against its possibility of referring to an “independent” reality at all by assuming an anti-representationalist position, but without the plain assumption of an ontological naturalism of the sort that Rorty’s view is ultimately based upon. Such an attempt can, I think, be detected in one of the most important contributions to recent discussions on naturalism, the *global expressivism* Huw Price has advanced in a number of recent writings (2004; 2010; this volume).

6 Typically, Rorty continues by the remark that he has adopted this position because of its practical benefits: “[...] I am suggesting, in the spirit of Deweyan experimentalism, that it behooves us to give the self-image Darwin suggested to us a whirl, in the hope of having fewer philosophical problems on our hands” (Rorty 1991, p. 48).

7 I will presently return to this view and why it threatens paradox or incoherence.

Price's starting point is pretty much the converse of our problem so far: it is the contemporary naturalist's attempts at fitting all of our different vocabularies or language games – such as those of morality, modality, etc. – with the “truth-makers” of the scientifically conceived world. For our purposes here, it is important to note that these *placement problems* already mentioned above are not due to the fact that we somehow know what reality is like independently of these different language games. Instead, they are due to the fact that one type of statement, or one language game, is taken as privileged – the language game of science. The picture we start with is, then, what Price calls the *object naturalist* picture: the world is what science tells us what it is, and the rest, Quine's “other good language games”, have to somehow be dealt with.⁸

Price's preferred means of dealing with the remainder is to advance an expressivist view according to which the statements of some particular domain (e.g. evaluative judgments) do not “represent” but are truth-apt in a minimalist vein. This effectively reduces the number of statements that are supposed to “refer,” or “represent” reality. However, in a radical move, Price “globalizes” expressivism to cover all of our statements, and not just statements of some particular domain. Expressivism, Price maintains, has somewhat artificially been maintained locally, resulting in a bifurcation problem of drawing a line between the cognitive and non-cognitive (or expressive) uses of language. This problem has usually been taken to benefit the cognitivist side of the debate. But Price reads the logic of this situation differently. For him, the failure of non-cognitivists to contain their view in one linguistic practice and prevent it from

⁸ Price (2010) lists the three evident alternatives. The first is to say that the different language games actually all refer, for example in the reductionist manner of equating the apparently “excess” statements with, ultimately, shorthand for talk about scientifically admissible objects and properties. The other two options concern the amount of things on either side of the statement-world divide. The second is to expand on the ontological side of what there is to include more “truthmakers” than initially thought. As Price rightly remarks, it is a question of labels whether this would amount to allowing “non-natural” truthmakers or to widening our conception of science. The third is to reduce the number of statements that require “truthmakers” on the linguistic or semantic side of things; Price lists eliminativism, fictionalism and expressivism as variants of this last project. It perhaps deserves to be noted that the way Price understands eliminativism as a form of reducing the left-hand side is not an obvious reading. If eliminativism works analogously to the error theory of moral judgments (cf. Mackie, 1977), which pretty much equals anti-realism about morality, it does not “eliminate” anything. Instead, both sides stay just like they are, but we note that some of the things on the left cannot be fitted with things on the right – it is for this reason that we can be said to have erred. Error theory in morality does not mean dropping moral language altogether. Like anti-realist views in general, it only points out that a piece of language, which we may merrily keep using in all practical contexts, is in error because it fails to refer.

“spilling” over to other regions of language points towards the possibility of turning expressivism into a global – we might say *default* – position.⁹

As expressivism generally holds, our language – especially our usage of standard semantic notions such as reference and truth – may have misled us into thinking that there is a representing relation between statements and the world. But in an expressivist framework, such representationalist and realist intuitions can be explained in the minimalist manner of Blackburn’s quasi-realism – in Price’s case, a global variety of quasi-realism. Instead of beginning with the view that reality is as science describes it (object naturalism), his view offers a scientific – anthropological or “genealogical” – account of human language and its function, a *subject naturalism*.¹⁰

Like Jonathan Knowles (2010; this volume) I find that the obviously difficult question for the subject naturalist is that concerning the position of his own claim (or the *replacement* problem as it already appeared in connection with Rorty above): in what sense is the global expressivist view itself *true*?¹¹ Compare this predicament to that of the local expressivist tenets. As Price points out, the local expressivist’s position does not depend on a distinction between statements that refer and statements that “express.” However, perhaps Price still understates the importance of at least *something* like representation for the local expressivists. While that domain of language that the local expressivist’s view concerns is not “representing”, the expressivist theory *itself* is: it represents the way things are, or so it would seem natural to take it. This is exactly where the local nature of expressivism becomes important. To borrow the Wittgensteinian image, Price has used the ladder of local expressivisms to climb up to a global expressivism – but then wishes to throw that ladder away.

However, *pace* Knowles, I don’t think the global expressivist has much trouble saying that his view is, scientifically speaking, true. Indeed, he has an easy enough response. Of course, the global expressivist is not required to give

9 Indeed, it seems expressivism about evaluative language or judgments has a tendency to spill over. If questions about how evaluative judgments “represent” are misguided, we cannot assume normative privilege to a certain group of statements (e.g. those yielding object naturalism). One diagnosis of Price’s view is to say that it works out the full implications of this view: expressivism must be extended to cover all of science.

10 How we should understand the relationship between Price’s view and expressivism depends on which aspect of the former we emphasize. In a sense, Price is, like local expressivists, reducing the cardinality of statements that are intended to “represent” (in his case, to zero) by explaining these away by a scientific theory. But another way of looking at the matter is that Price simply abandons the whole picture of language representing reality for an anti-representationalist and quietist view. This difference is one that makes a difference, as I’ll presently try to show.

11 To be precise, Knowles asks the more particular question of how subject naturalism can be true by its own lights. My question here is wider: in what sense is the subject naturalist account true?

an account of how things are expressivist in a *non*-expressivist (or representing) sense. Staying on an expressivist level, he will simply state that his theory is true in the same way that anything is true (by his view), in a minimalist, non-representationalist way. In effect, to talk about how the theory is true is just to talk about the theory itself.

This easy response, however, hides what I think is the crucial issue here. It remains to be understood how Price's global expressivist view of language is the *preferred* "subject naturalist" theory about language. This is, after all, a question that is supposed to be decided by empirical, naturalist means, and not *a priori*. The global expressivist will presumably just go ahead and argue that the view proposed will be found successful on an anthropological understanding of language. Moreover, he can explain the genesis of his own position in the same way he explains the genesis of all others: as an instance of his account of human language-use. As Knowles (this volume) points out, Price's account as such pretty much remains on a rudimentary and philosophical – as opposed to the "scientific" – level. As with Quine, this condition can be read as indicative of the need of further advances in science – in Price's case, anthropology and "genealogy." It is the choice of the explaining special science, so to speak, that differentiates Price's naturalism from that of Quine. Again, none of this forces the global expressivist to talk about how the theory is "true" in some sense other than the minimalist sense of the theory itself. His responses are simply more *use* of that theory.

The analogy to Quine brings to the fore a central complication. Namely, perhaps there are other, competing "subject naturalist" *theories* – theories that are intended as accounts of human *language* but do not equal Price's global expressivism. For example the causal theory of reference, or Quine's view of language as responses to stimuli, might amount to exactly such accounts, at least suitably adjusted and developed.¹² How are we to choose among these differing theories of language? The crucial problem here is that all of them will similarly be able to explain themselves and the sense in which they are "true": such an explanation is just more "use" of that theory – just more theory. It is in a sense *too* easy for the theories to account for themselves, or be "true" by their own lights.

Of course, this point of view requires that we take a step back from different possible subject naturalist theories of language and ask why any of these would

12 In addition, competing subject naturalist accounts might have clear "representationalist" implications. As we will shortly see, Price has advanced an interesting argument against empirical theories of reference or representation. However, this argument is directed against such views as positions in *object* naturalism, and (it seems to me) has no direct bearing on whether representationalist views could be viable theories in *subject* naturalism. As I will presently argue, however, similar problems will arise with subject naturalist theories of language – including Price's own global expressivism.

be preferred. As Price (2004) has argued, such a move is a real change of subject-matter, or a *genuine* move from “use” to “mention”. However, it seems such a move is unavoidable if only to make sense of how Price’s theory has an advantage over other possible *subject* naturalist accounts.¹³ Obviously, on this level, Price cannot say that his global expressivist theory *represents* the way things are. Indeed, Price takes seriously Paul Boghossian’s (1990) argument that we cannot coherently formulate an irrealist view of semantic terms: such a position would, it seems, amount to arguing that reference doesn’t refer to anything. It is for this reason that the global expressivist remains *quietist* about reference – the question simply does not come up, not to mention being answered in the negative. Price takes care not to overstep his subject naturalist position and court incoherence: instead of saying that our statements do not represent and terms do not refer, he emphasizes that the whole question does not appear in the subject naturalist framework as he conceives of it.

But if this is how we are to understand Price’s subject naturalist account, his global expressivist theory clearly approaches a version of internal realism as discussed above. Again, science explains itself: the global expressivist position explains the sense in which it is “true”. But this is “just more theory”. Stepping back from that theory – or from the level of “use” to the level of “mention” – we can note that there is potentially a large variety of such equally self-supporting theories of language. Moreover, on this level, quietism must prevail when it comes to “representation”: whether the theory represents the “world” as it is independently of our vocabularies is not a question to be considered. This is analogous to Putnam’s insistence that the theories themselves only give an “internal” realism. The question of how they are preferable to competing, equally self-supporting accounts cannot be answered – although both Putnam and Price do mention this question *on* the level of “mention”¹⁴.

There are two important reasons why Price would and should, I think, insist that his view is not an internal realist one. Firstly, perhaps the most central of Price’s own arguments against representationalism is that there are multiple *compatible* theories of reference: using Price’s notation, by theory R*, “Refer-

13 This also points towards a more general issue with all minimalist accounts of truth, which I will only mention here. Minimalist accounts have often been defended as successfully explaining our use of the truth concept or predicate (and sometimes related semantic notions). However, when we ask how a particular account of our use of the truth predicate is preferable to another, or moreover why a minimalist account is preferable to a more robust theory of truth (in which the concept does explanatory work), we seem to be asking how these minimalist accounts are preferable (or true) in a sense *not* (necessarily) entailed by these accounts themselves. The implication is that ultimately the question is one of justification and not just explanation.

14 Of course, “representing” is perhaps not the only way in which a theory might be so privileged; however, internal realism as conceived of above should not be considered as wedded to a representationalist picture.

ence” stands in the relation R^* to the relation R^* ; by theory R^{**} , “Reference” stands in the relation R^{**} to the relation R^{**} , etc. (Price, 2004). This shows that the object naturalist’s project of devising a singular theory of reference (or representation) is somewhat misguided, as formulating that theory presupposes the notion itself. In subject naturalism, it may turn out that there is no such relation to be scrutinized or explained (which is the case in Price’s global expressivism). However, as we already saw, something analogous still happens to different subject naturalist accounts of *language* (including the concept of truth): a variety of such accounts may be similarly compatible as all of them can be “true” by their own lights. What this seems to show is that the object naturalist’s problem will return to haunt the *subject* naturalist. Our choice between different subject naturalist views of language should presumably be an empirical matter, but it appears that there are several compatible subject naturalisms (at least as long as the choice of subject naturalist theory is supposed to be naturalist and not *a priori*¹⁵). If this is right, subject naturalism will lose a key point of advantage against object naturalism.

Secondly, as Price’s subject naturalism resorts to quietism about representation, it is in danger of losing much of its *anti*-representationalist bite. Global expressivism was never supposed to show the representationalist view *mistaken*. However, this places anti-representationalist tendencies of global expressivism “inside” a second-order quietism. As internal realism is not realism “all the way down”, Price’s global expressivism is not anti-representationalist “all the way down”. While internal realism *is* realist, and global expressivism *is* anti-representationalist, they both come with a limitation of domain (or instructions of application) that will leave us wondering whether something could actually be said for realism or anti-representationalism deep down.

Price might resist such likening of internal realism and subject naturalism by pointing out that in his view, the world underlying our language-use is that of science. This notion gives us a way of pointing out what our statements do *not* represent. The problem with this gesture is that it begins to look as if Price’s expressivism – like Rorty’s anti-representationalism – would then be based on the point of view of that independent reality, the underlying scientific picture of the world. Expressivism, then, would itself be based on the underlying scientific position (like local expressivism was above described as a view that *itself* “represents”): it would be an *object* rather than subject naturalist view. But such an account would threaten exactly the sort of incoherence which Price’s quietism was designed to avoid.

The issues at hand are obviously intricate and difficult. But if what I have said here is along the right lines, there is, I think, an underlying reason why

15 To put this point differently, subject naturalism cannot simply *equal* global expressivism or minimalism. This would be an *a priori* account. Again, the choice between different subject naturalist theories must be an empirical matter.

Price's choice turns out to be the one of Putnam vs. Rorty, indicating a kind of dialectical tension in the global expressivist position. The question posed above was about the sense in which global expressivism is the preferable subject naturalist account of language. My suggestion here is that in answering this question the global expressivist either turns towards quietism (where nothing can be said in favour of the global expressivist view against competing *subject* naturalist views) or lapses back to object naturalism. This problem, however, is not peculiar to Price's views – indeed, it seems to be shared by all naturalist accounts. If we do not draw from either a naturalist epistemology or a naturalist ontology, we have no idea of how a distinctly *naturalist* view can get off the ground. But by either alternative we end up in trouble making sense of the privilege of these "naturalist" positions. What is required is an account of how the scientific project or naturalist ontology might *genuinely* be privileged. For this purpose, I will finally turn to normative science.

Normative science

As Quine held, philosophy has been for a long time occupied with the foundationalist project of laying groundwork for the certainty and objectivity of science. There are of course a number of historical examples of exactly such attempts: Descartes's argument that a particular kind of idea – those that are clear and distinct – have epistemological privilege, which would in turn lead to their functioning as the indubitable foundation for further (scientific) study; similarly, Kant's project of "deducing" the categories of experience is an attempt to justify basic claims in metaphysical philosophy, which would in turn lay the ground for the objectivity of science. The naturalist's key move is to deny the feasibility of such "first philosophy." But as we have seen, such attempts threaten to lapse into either more "first philosophy" or a "mere" internal realism.

To navigate between these two alternatives, I will here (albeit briefly) return to the idea left open above, that of science itself studying normative questions, as well as the question left open above, that of what science is. Although many other things have been called pragmatism in more recent discussions, taking the possibility of a scientific study of normative issues seriously is perfectly pragmatist at least in the classical sense of the word (cf. Pihlström, 2005, p. 95). William James positioned pragmatism as standing between two tempers in philosophy, namely, the "tough-minded," turning towards hard science for answers, and the "tender-minded," whose wish to take moral issues seriously leads to metaphysical and religious speculation. Here James not only managed to point at a major dilemma in his contemporary thought but anticipated central developments in subsequent philosophy. While advancing an essentially scien-

tific world-view, then, classical pragmatist philosophers attempted to allow room for genuinely normative and ethical questions, although differently. James's pluralism sometimes took a more explicitly epistemological and normative form, suggesting that there is an irredeemable plurality of epistemic goals (cf. Rydenfelt, 2009). John Dewey, in turn, emphasized that philosophical as well as (other) scientific theories were meant to solve practical, tangible problems that arise from our situations, including our "ends in view," but in his view, there is no overarching end to these ends, or a quest for certainty for its own sake. It is no coincidence that Quine derived his naturalism originally from Dewey (cf. Sinclair, this volume) – and that Dewey was also Rorty's anti-representationalist champion.

It is in Charles S. Peirce's writings that I think we can find the most interesting and fruitful perspective on the possibilities of such pragmatist views. In his early and probably most read paper, "The Fixation of Belief" (1877), Peirce gives an account of inquiry and his notion of the scientific method. Beginning with the idea that inquiry is the passage from the restless condition of doubt to that of belief, Peirce then considers four different ways in which such an inquiry might proceed, different methods of fixing belief – in effect different accounts of truth. The first is the method of tenacity, the clinging to one's opinion despite the criticism of others and the disappointments of experience. However, under what Peirce calls the "social impulse," this method is bound to fail. The disagreement of others begins to matter, and the question becomes how to fix beliefs so that they are fixed in a community and not only for oneself. The next two methods are those of authority, which draws from an authoritarian source, and its refinement, the *a priori* method, which fixes belief in accordance with what human beings can agree upon by way of free deliberation. But ultimately both methods are deemed unsatisfactory, as they make the opinion of all depend on the arbitrary views of the authority, or accidental matters of taste. Instead, our beliefs must be fixed in a way that would make them independent of our subjective opinions and tastes altogether – by a reality "independent of what anybody may think them to be" (Peirce, 1878, p. 137). This is the scientific method. It assumes that there is an independent reality, of which we moreover already have some true beliefs and with which we are in touch in experience, widely conceived. The success of science gives this hypothesis credence, although not infallible authority.¹⁶

16 An interesting comparison could be made between Peirce's view and Price's (1998) argument that the concept of truth plays a normative role in our discourse (or practice of assertion) in that disagreement implies that one of the conversation partners must be wrong. Peirce concentrates on the fixation of belief rather than the norms of assertion. But for him, too, the importance of truth begins with the move from tenacity to the more general conceptions of truth. From Peirce's perspective, however, this still leaves open the more interesting question of what way of fixing "mutual" belief can be found satisfactory, or

From this Peircean perspective, contemporary naturalism has resorted to too narrow accounts of science itself in terms of e.g. physicalism, or to relying on our common understanding of the scientific project, and then attempting to give an account of how this project is a privileged one, or “represents” reality. As we have seen, such an attempt leads nowhere. Instead, the Peircean approach begins with the idea that belief-fixation is ultimately satisfactory only when it is responsive to what is independent of us. On an abstract level, this *defines* the scientific method, which is the attempt to fix belief in accordance with such a reality. This conception builds neither on specific scientific results nor on a particular method that is considered the scientific one. In principle, any particular method inside science is open to revision and “scientific revolutions,” although we have much reason to believe that at least our most fundamental ways of reasoning are not completely out of accordance with the way things are. Obviously, this basic conception is to be fleshed out in terms of what specific methods are better and worse in attaining this task. Peirce later conceived of a part of philosophy he called *normative science* as the study of what sort of rules and aims are satisfactory, or possible to adopt (Peirce, 1903). Of the triad of such sciences – aesthetics, ethics and logic – logic is the inquiry into the feasibility of more particular methods of inquiry, or inference. Thus normative science concerns both of the two levels here discussed: the more abstract choice of overall method (such as the scientific one) and the choice of particular methods or modes of inference inside science, giving the overall content of the method.

As Peirce positions his normative science inside the confines of philosophy, it might easily seem that it amounts to pretty much the same as “first philosophy,” just differently formulated. If normative science has the important task of discussing the *justification* of scientific claims – then it might seem like philosophy also retains its traditional status. But in contrast to “first philosophy,” normative science reconceives of philosophy as that part of science which (among other things) is concerned with the justification of the scientific method itself. This leads to two important differences between such a normative science and a first philosophy. Firstly, while the latter places philosophy beyond and above science and imposes no limitations to the content and methods of such inquiry, the former sets boundaries for what would amount to philosophical study in a scientific framework. Normative science, *qua* science, must also follow the general methods of science, including the testing of its theories. Unlike a first philosophy completely detached from a scientific framework, normative science is then methodologically bound to a scientific perspective. Secondly, and consequently, unlike traditional first philosophy, normative science does not attempt at any *a priori* certainty, or laying a foundation for science that is, in Quine’s phrase, “firmer than the scientific method itself.” Instead, normative

how we are to resolve disagreements. As we will see, the Peircean account also attempts to account for *why* it is that we have such norms (of fixing belief or assertoric practice).

science is quite as experimental and fallible as all of science. It successfully resists regress to a dubious, “philosophical” level of certainty.

This brings us directly to the second point, the question of internal realism. As we saw, the problem with theories of reference, language, etc., which might be highly successful in explaining themselves, was that this does not imply that these theories cut the world at its joints. That is “just more theory”. The implication of this is internal realism. If normative science does not resort to foundationalism, how can it avoid being *circular* in this manner? The main problem of first-order scientific theories as justifying naturalism was that success of scientific theories in explaining other bits of science, or their own genesis, does not equal a philosophical justification for privilege for these theories. (In addition, several such theories might turn out to be equally successful.) However, normative science differs from theories of reference and language in this respect. Instead of an explanatory task, it has a *justificatory* position: its conclusions are explicitly normative. Hence the problem of explanation not being justification evaporates.

Admittedly, however, also the normative science’s picture of the scientific enterprise is ultimately circular. Even although it will not face the problem of explanation vs. justification, if normative science is to be a *science*, it must, as was already noted, work within the scientific method. Instead of an *explanatory* circle, normative science moves in a *justificatory* circle: as a science, it too follows the very method it is in a sense designed to investigate and justify. An idea that underlies Peircean normative science is that we can indeed give an explanation of why some goals (or methods) will prevail: there is an irreversible tendency toward affirming certain aims instead of some others. The implication of a normative science is, after all, a normative realism: there must be an independent reality which this science is answerable to. (It is here especially that the Peircean view contests much of contemporary orthodoxy.¹⁷) But obviously, such an explanation is only available for those who already have adopted the framework in question. Peirce’s “Fixation” story of proceeding from one method to another is intended neither as a historical account nor as a “method-neutral” argument in favour of the scientific method. There is no such argument available: for any argument to hold sway, one has already had to adopt the method in ques-

17 In a sense, then, there are “normative facts” that such science considers. Initially, this might be taken to be an attempt to respond to the placement problems discussed above by the simple addition of new “truthmakers.” However, two considerations should make clear how this position is not as *ad hoc*. Firstly, it depends on the account of science already developed rather than *vice versa*: the conception of science is not simply expanded to “naturalize” a number of “non-natural” facts. Secondly, there is a Peircean way of accounting for such facts that makes them non-mysterious as well. T. L. Short (2007, ch. 5) has argued that instances of such irreversible developments can be found in phenomena studied by the special sciences, e.g. in thermodynamics and biological evolution.

tion. This circular view is hardly vicious, however. Rather, it is perfectly in line with the scientific method itself: what hasn't been taught by experience cannot be shown with words.

Conclusion

The Quinean denial of a “first philosophy” – the attempt to give (distinctly) philosophical foundations for science – is an important, anti-foundationalist point of departure for naturalist views. However, it results in the problem of accounting for the related naturalist position that science is our best way of finding out what there is. Obviously, this should be done without resorting to more first philosophy. Moreover, as I have argued, even the denial of this whole project, like Rorty's anti-representationalism, itself courts foundationalism. To avoid any form of foundationalism – representationalist or anti-representationalist – naturalism can be recast as a first-order scientific theory (or anticipation of such a theory), as with Quine's account of how stimulus leads to science. But this move has the drawback of issuing a “mere” internal realism, as we saw in Putnam's case. This problem, I have argued, also haunts Price's subject naturalist position. The remedy suggested here is to take normative questions seriously and maintain openness to a scientific (even if philosophical) inquiry into such questions – even a “normative realism” to make sense of such inquiry. This will result in science having the means for justifying itself, which avoids both a recoil to foundationalism as well as the qualifications of internal realism. Science itself will account for what is good science. The picture is obviously circular, but not viciously so.

What has been said here goes only a very short way towards showing what I think is the plausibility of a scientific inquiry of normativity and the possible bankruptcy of the alternatives presented. But it is a beginning of an account of the pragmatist perspective which enables the development of a sophisticated naturalism of the sort required. For us to *be* naturalists, we must be able to account for the normative issue of the “privilege” of science. By means of normative science we can do so while *remaining* naturalist.¹⁸

University of Helsinki

¹⁸ I am grateful to Sami Pihlström, T. L. Short, and especially Jonathan Knowles for many helpful comments and criticisms of an earlier draft of this paper.

References

- Boyd, R. (1983) 'On the Current Status of the Issue of Scientific Realism', *Erkenntnis* 19, 45–90.
- Boghossian, P. (1990) 'The Status of Content', *Philosophical Review*, 99, 157–184.
- Goldman, A. (1967) 'A Causal Theory of Knowing', *Journal of Philosophy* 44, 357–372.
- Feyerabend, P. (1975) *Against Method*, London: Verso.
- Knowles, J. (2010) 'Two Kinds of Non-scientific Naturalism' in M. Milkowski & K. Talmont-Kaminski, eds., *Beyond Description: Normativity in Naturalised Philosophy*, London: College Publications.
- Kuhn, T. (1970) *The Structure of Scientific Revolutions*, 2nd edition, Chicago: University of Chicago Press.
- Kripke, S. (1980) *Naming and Necessity*, Cambridge, Mass.: Harvard University Press.
- Mackie, J. L. (1977) *Ethics: Inventing Right and Wrong*, Harmondsworth: Penguin.
- Papineau, D. (2007) 'Naturalism', in Edward N. Zalta, ed., *The Stanford Encyclopedia of Philosophy*, <<http://plato.stanford.edu/archives/spr2009/entries/naturalism/>>.
- Peirce, C. S. (1877) 'The Fixation of Belief', in C. Kloesel & N. Houser, eds., 1992, *The Essential Peirce Vol. 1*, Bloomington: Indiana University Press.
- (1878) 'How to Make Our Ideas Clear', in C. Kloesel & N. Houser, eds., 1992, *The Essential Peirce Vol. 1*, Bloomington: Indiana University Press.
- (1903) 'The Three Normative Sciences', in The Peirce Edition Project, ed., 1998, *The Essential Peirce Vol. 2*, Bloomington: Indiana University Press.
- Pihlström, S. (2005) *Pragmatic Moral Realism: A Transcendental Defense*, Studies in Pragmatism and Values (Value Inquiry Book Series, Vol. 171), Rodopi, Amsterdam and New York.
- Price, H. (1998) 'Three Norms of Assertibility, or How the Moa Became Extinct', *Philosophical Perspectives* 12, 241–254
- (2003) 'Truth as convenient friction' *Journal of Philosophy*, 100, 167–190
- (2004) 'Naturalism without representationalism' in De Caro, M. & Macarthur, D., eds., *Naturalism in Question*, Cambridge, Mass.: Harvard University Press.
- (2010) *Naturalism without Mirrors*, Oxford: Oxford University Press (introductory chapter at <<http://www.usyd.edu.au/time/price/preprints/IntroductionFinal.pdf>>).
- Putnam, H. (1975) *Mathematics, Matter and Method*, Cambridge: Cambridge University Press
- (1978) *Meaning and the Moral Sciences*, London: Routledge and Kegan Paul.
- (1981) *Reason, Truth and History*, Cambridge: Cambridge University Press.
- (1983) *Realism and Reason*, Cambridge: Cambridge University Press.
- (1988) *Representation and Reality*, Cambridge, Mass.: MIT Press.
- (1990) *Realism with a Human Face*, Conant, James F, ed., Cambridge, Mass.: Harvard University Press.
- Quine, W. V. O. (1992) *Pursuit of Truth*, Cambridge, Mass.: Harvard University Press.
- Ramberg, B. (2007) 'Richard Rorty', in Edward N. Zalta, ed., *The Stanford Encyclopedia of Philosophy*, <<http://plato.stanford.edu/archives/spr2009/entries/rorty/>>.
- Rorty, R. (1979) *Philosophy and the Mirror of Nature*, Princeton, NJ: Princeton University Press.
- (1982) *Consequences of Pragmatism*, Minneapolis: University of Minnesota Press.
- (1989) *Contingency, Irony, and Solidarity*, Cambridge: Cambridge University Press.

- (1991) *Objectivity, Relativism, and Truth: Philosophical Papers, Volume 1*, Cambridge, Mass.: Cambridge University Press.
- (1998) *Truth and Progress: Philosophical Papers, Volume 3*, Cambridge: Cambridge University Press.
- Rydenfelt, H. (2009) ‘Pragmatism and the Aims of Inquiry’, in Pihlström, S. & Rydenfelt, H., eds., *Pragmatist Perspectives*. Helsinki: Acta Philosophica Fennica.
- Short, T. L. (2007) *Peirce’s Theory of Signs*, Cambridge: Cambridge University Press.