CHAPTER 5

QUINE'S ACQUIESCENCE IN SKEPTICISM

Quine's (1969a) arguments for naturalizing epistemology have sparked dissent from the ranks of traditional epistemologists. For example, Kim (1988) complains that by making epistemology a "chapter of psychology," the naturalist robs epistemology of its normative force and thereby its status of genuine epistemology. I agree with the spirit of Kim's objection, but it is far from clear that Quine or other naturalists would agree that naturalized epistemology must be merely descriptive rather than normative. Quine (1986a; 1992) denies it, and proposals for naturalizing normativity abound. Other critics like Stroud (1981; 1984) sympathize with the idea that Quine has simply changed the subject, if not by dropping normativity, then for other reasons.

Yet as early as "Epistemology Naturalized," Quine insists that naturalism still yields epistemology "or something like it," on the grounds that the naturalism studies a natural phenomenon which is the subject matter for traditional epistemological questions:

> [E]pistemology still goes on, though in a new setting and a clarified status. Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input—certain patterns of irradiation in assorted frequencies, for instance—and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history. The relation between the meager input and the torrential output is a *relation that we are prompted to study for somewhat the same reasons that always prompted epistemology*; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence. (1969a, 82–3, emphasis mine)

Even though Quine thinks naturalized epistemology is still very much like traditional epistemology, he does stress its "new setting" and "clarified status." In chapter 2, I explained what Quine meant by each of these. The "new setting" is psychology, which implies, for example, that important questions about the relationship between evidence and theory are no longer to be settled by relating evidence and "awareness," but instead by relating evidence to causal proximity to sensory stimulation. The "clarified status" of epistemology is that it is no longer to be concerned with discovering or with

deriving on its own any first principles, given that theory "transcends any available evidence" (a statement of Quine's underdetermination thesis).

But this understanding of Quine's naturalized epistemology is largely negative. It does not concern awareness and it does not concern first principles. So what, then, does Quine want to achieve through such a study? What kind of relationship between evidence and theory does he mean to examine? He admits that it would be circular to try to try to validate the grounds of empirical science by using science (Quine 1969a, 75–6). But he also claims that we should not *want* to try, since philosophers stopped dreaming of a deductivist foundationalism for science long ago (76). If the evidence-theory relationship to be studied is not the traditional relationship of epistemic justification, what then is it?

The critics suggest that whatever the project of the naturalized epistemologist, if it is to count as epistemology, it must at least confront the problem of skepticism in some manner. Critics might say that if Quine doesn't intend to establish the foundations of science, then he should at least show how to "dissolve" the problem of skepticism, perhaps by offering a "therapeutic" diagnosis of skepticism in the manner of Wittgenstein (1969), by asking "whether it can make sense to doubt" what the skeptic asks us to doubt (2e). Yet Quine is ambiguous about his estimate of the Wittgensteinian strategy, pejoratively characterizing it as offering philosophers a mere "residual philosophical vocation." He urges that after the death of foundationalism, contrary to this strategy, "epistemology still goes on" (Quine 1969a, 82).

Quine does appear to offer a naturalistic strategy for dealing with the skeptic, but the nature of this strategy is somewhat unclear. In "The Nature of Natural Knowledge" (2004d), he argues that skeptical doubt is indeed what prompts epistemology—but that skeptical doubt is itself a product of science.[36] He notes, for example, that illusions can only be identified as such in relation to the existence of "genuine bodies with which to contrast them;" likewise the attempt to account for

[36] See also Quine (1960, 2) and Quine (1974, 1-4).

awareness of a third dimension based on two-dimensional images on the retina could only be initiated against the backdrop of the investigation of three-dimensional physiology. But in observing this, Quine does *not* appear to be raising therapeutic points designed to show that the skeptic's doubts make no sense. He does not wish to say that the skeptic presupposes knowledge of the external world, and therefore is engaging in a self-defeating argument. Indeed, he notes:

> [The skeptic] is quite within his rights in assuming science in order to refute science; this, if carried out, would be a straightforward argument by *reductio ad absurdum*. I am only making the point that skeptical doubts are scientific doubts. (2004d, 288)

If Quine thinks that the skeptic is within his logical rights to assume science to refute science, this implies at least that there is nothing straightforwardly incoherent in such assumptions. There may, of course, be something incoherent drawn out of the assumption—which is why it would be a *reductio ad absurdum*—but this incoherency would be grounds for skepticism, not a refutation of it.

What then is the significance Quine ascribes to the fact that skeptical doubts are scientific ones, if it is not therapeutic, and how exactly is it supposed to permit him to answer the skeptic in such a way as to retain some remnant of the subject matter of traditional epistemology? Most importantly, can this strategy succeed? Critics think that Quine either has no logical right to the free use of science to answer the skeptic's challenge (on the grounds that such use would beg the question) or that on Quine's own terms, such use can only lead to skepticism itself. Defenders think Quine's strategy does not beg the question, and that it can succeed against the skeptic, in effect by dissolving skeptical worries after diagnosing their source.

In this chapter, I will examine the major criticisms leveled at Quine's strategy, the attempt of Quine and his defenders to reply to these criticisms, and then evaluate the replies. I will argue that in the end, many interpreters of Quine—including both critics and defenders—are confused about what Quine is trying to do in response to the skeptic. Critics assume his views lead to a skepticism he does not desire; his defenders assume his views can help refute the skeptic. Both parties, I will argue, are incorrect. Quine's views do lead to a version of skepticism, at least by the standards of traditional

epistemology. But since Quine is not a traditional epistemologist, this will not worry him. By his own *pragmatic* standards, he is not a skeptic. The question is whether his own pragmatic standards are consistent with his naturalism, whether a pragmatic account of justification can privilege scientific discourse over all other kinds. I will give reasons to doubt this. If we are upset, then, about Quine's complete abandonment of the traditional goals of epistemology, and challenged by the difficulties of his pragmatism, we should then wonder if we need to be naturalists after all. In my final section, I will explore the roots in Quine's skepticism (traditionally conceived), and show how his various negative theses (including both the indeterminacy of translation and the inscrutability of reference) derive from an argument also used by his underdetermination thesis.

Quinean skepticism via underdetermination and inscrutability?

Barry Stroud was well aware of Quine's strategy to dissolve skeptical problems by making free use of science; still he found this strategy unpersuasive. In "The Significance of Naturalized Epistemology" (1981), Stroud worries that *what* is revealed by a naturalistic examination of the evidence-theory relationship is not conducive to answering the skeptic. For example, given the "meager input" of the sensory surfaces, Quine tells us that the output of belief in physical objects is a "posit," or as he had put it in "Two Dogmas of Empiricism," "comparable, epistemologically, to the gods of Homer" (1953b, 44). Quine of course believes in physical objects and not the gods of Homer, and says everyone ought to agree with him. But the origin of the physical object hypothesis is "shrouded in prehistory" (Quine 1960, 22), and although it has no doubt proved successful, Stroud wonders why we should take it for granted in the face of the skeptic.

Stroud notes that Quine, like G.E. Moore, is willing to assert that there are physical objects. The question is whether this assertion should be taken as contradicting the skeptic in any way. Of course Stroud thinks that Quine wants to explain *how* we know about physical objects from within the scope of science, by explaining the route from meager input to torrential output. But Stroud reminds us that to *explain* the origin of some subject's knowledge, two conditions must hold: we, the explainer,

must know that the subject's belief is true; and we must be able to show that it is not an accident, that the subject's posit turns out to be true because of some connection to the truth. Yet Quine's naturalistic investigation is also supposed to reveal that our subject's sensory inputs are "meager" in comparison to his outputs. If our position is similar to the subject's—a point Quine emphasizes—then there is a serious question about whether we, the investigators, are therefore even in a position to fulfill the two conditions of explaining the origin of knowledge, not only for our subject, but for ourselves. Even our understanding of sensory inputs as meager—a scientific discovery—would end up being a posit of its own, one rivaled by alternate hypotheses, as well.

Stroud acknowledges Quine's (1974; 1975b) points about how skeptical doubts arise in a scientific context. But Stroud is then at a loss to see the advantage behind naming the scientific nature of skeptical doubts. It might be thought that by pointing out how an understanding of "illusion" depends on a prior scientific grasp of "reality," Quine intends to show that the skeptic is asking an incoherent question when asking how we know our perception is not merely an illusion. But Stroud points out that Quine's acceptance of the legitimacy of the skeptic's use of science for *reductio* discounts this possibility. If skeptical questions were incoherent, then scientific assumptions could not be relied upon for the sake of *reductio*. If Quine concedes this, Stroud observes that it is hard to make sense of what *further* use science could be put in answering this *reductio*. If, by assuming certain facts about sensory input (for example), we are led to some general skeptical conclusion casting all of our knowledge into doubt, we have already reached the conclusion of our *reductio*, and at that point it would seem the epistemologist is no longer within his rights to make free use of science.

Stroud acknowledges that perhaps Quine means to do something else to answer the skeptic. Quine's (1981a, 475) answer to Stroud offers a glimmer of an alternative proposal. His direct answer to Stroud's query about *how* he intends to answer the skeptic is presented as follows, by reframing his attitude towards the skeptic's *reductio*:

> Thus, in keeping with my naturalism, I am reasoning within the overall scientific system rather than somehow above or beyond it. The same applies to my statement,

> quoted by Stroud, that "I am not accusing the sceptic of begging the question; he is quite within his rights in assuming science in order to refute science." The skeptic repudiates science because it is vulnerable to illusion on its own showing; and my only criticism of the skeptic is that he is overreacting.

There is a point to this response, because there is an important difference between relying on science *after* a *reductio* of science has gone through (which would be unjustified), and appealing to science *within the scope* of the alleged *reductio*, in order to show that it simply does not go through to begin with. If, for example, the naturalist examines more science than the skeptic does, and concludes that because of facts about illusions unappreciated by the skeptic, their existence casts no doubt on our knowledge at all, this would be an apparently naturalistic means of blocking the conclusion of the skeptic's *reductio*.

Another critic of Quine, Michael Williams (1996), appreciates this point. Williams points out that by pointing out the overreaction of the skeptic, Quine could simply be making a traditional refutation of the argument from error, by noting, for instance, that just because our senses sometimes "deceive" us does not imply that we should never trust them (a point made by Descartes himself). But Williams is concerned that if this is all Quine means by pointing out that the skeptic is "overreacting," then it does not accomplish much. Arguments from illusion and error are not thought to be serious grounds for *radical* skepticism, anyway, so it does the naturalist little good to diagnose the fallacies behind them. If, therefore, there are other scientific grounds for doubt that lead to radical skepticism, these grounds must be something other than the argument from illusion.

Williams suggests that what Quine must have in mind is the additional point, mentioned after the discussion of illusions in "The Nature of Natural Knowledge" (2004d, 288) that "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," which is supposed to make us wonder how we arrive at torrential scientific output given only such meager input. But here Williams appears to side with Stroud: if *this* is the scientific assumption leading to skeptical doubt, then there is no way for the naturalist to block its skeptical consequences. Williams thinks that this is part of Quine's basis for

accepting the underdetermination thesis—a thesis Quine *affirms*, rather than dissolving by claiming that it is an "overreaction." If this thesis suggests that our theories are not justified, then this leads to radical skepticism. Indeed not only does Quine hold to the underdetermination thesis, but making sense of it even appears to be one of the *purposes* of naturalizing epistemology: Quine says we examine the relationship between evidence and theory *"in order* to see how evidence relates to theory, and *in what ways one's theory of nature transcends any available evidence*" (1969a, 83, emphasis mine).

There are at least two major ways in which Quine's naturalism highlights sensory inputs as "meager" in comparison to our theoretical outputs, and seems to imply a more radical form of skepticism, as a result. The first is its embrace of the inscrutability of reference thesis; the second its embrace of the underdetermination of theory by evidence. Although Quine discusses mainly the inscrutability thesis in his "Reply to Stroud" (1981a), Stroud actually invokes a hybrid of the two in attempting to show that Quine cannot respond successfully to the skeptic on his premises (Stroud 1981, 465).[37] In the remainder of this section, I will briefly outline these two theses, and explain why

[37] The distinction between underdetermination and inscrutability is, in fact, sometimes ambiguous. When Quine discusses underdetermination in his (1953b), he slides almost imperceptibly into saying that beliefs in physical objects are also underdetermined by the evidence (this is his infamous comparison between physical objects and the gods of Homer). But of course the sentences in which we register our belief in physical objects are typically observation sentences. Observation sentences are the medium in which all observational evidence is expressed, and thus there is a puzzle about what it could mean to say that belief in physical objects could be underdetermined by evidence, when rival theories said to be underdetermined are said to be such in relation to evidence that is held constant—evidence held in the form of observation sentences. This puzzle has led some, such as Williams (1996) to declare that Quine's views here are simply incoherent. I think, however, that the puzzle can be reconciled with a fuller understanding of Quine's views about inscrutability. It is true that observation sentences are held constant in order to say that non-observation sentences are underdetermined. But from that perspective observation sentences are taken holophrastically. Quine later concedes that when observation sentences are not treated holophrastically, they are theory-laden (Quine 1992, 7-9). It is true that few of us outside of philosophy ever form theories about the existence of physical objects. This is why Quine thinks that a belief in physical objects is buried deep in history. When children learn the individuating apparatus of reference, they are in effect *inheriting* this theory from history. So it is true that observation sentences are held fixed in relation to underdetermined theory, but as such they are merely holophrastic and do not represent an actual belief in physical objects. Once one approaches them from an adult perspective, from the perspective of dividing the reference of onetime holophrastic sentences, objects "go theoretical," and are subject to similar underdetermination concerns. There is, therefore, a way in which the underdetermination thesis is *fundamental* to the inscrutability thesis. If the latter has skeptical implications, it is probably in virtue of the former.

they each seem to imply a form of skepticism. Once this is complete, we will be in a position to evaluate Quine's full reply to skepticism in the next section.

The inscrutability of reference thesis is actually discussed explicitly in an exchange between Stroud and Quine. Stroud is worried that on Quine's view, there is a "possibility that the world is completely different in general from the way our sensory impacts and our internal makeup lead us to think of it." In discussion with Stroud, Quine proposes to understand this point in terms of "proxy functions and displaced ontologies" (1981a). "Proxy functions" are particular logical devices Quine has exploited to bolster the argument for his inscrutability of reference thesis, the thesis that the reference of individual terms within sentences are indeterminate, i.e. unsettled by the totality of evidence or sensory stimulation—the only relevant "facts of the matter"—available to language users (Quine 1969b). Unlike the indeterminacy of translation thesis, Quine takes inscrutability of reference to apply not only to terms in theoretical sentences, but those in observation sentences, as well. Famously, "rabbit" might refer to "rabbit," but also to rivals such as "rabbit stage," or "undetached rabbit part," or "Rabbithood" (Quine 1960). Quine uses proxy functions to show how similar rivals may be constructed for any term, simply by systematically mapping each predicate in a language to a unique predicate in a rival language.

Inscrutability *seems* to have inescapable skeptical consequences. If it is true that our terms may refer to any of Quine's proxies, then we don't know what the objects of our theoretical or even our observational sentences are, and we would seem to be cut off from the world. The reader may be concerned that inscrutability of reference by itself is not sufficient to generate serious skepticism. So what if we don't know *what* we're talking about?: perhaps all that counts is what we know about *whatever* it is we're talking about. But questions of reference and questions of epistemic justification might not be isolated so easily. It is likely that, at least in traditional epistemology, self-consciously successful reference is critical in avoiding Gettier problems. One way that a justified true belief can be only accidentally true is if it does not refer to the fact in question that makes the belief true. Suppose,

for example, that Jones sees a look-alike of Smith in the room, and claims that Smith is in the room, so fails to know this even though Smith is in the room (he's hiding). Here we can diagnose the failure as resulting from the fact that the person to whom Jones is referring is not actually Smith.

Just in case there is still doubt as to the role of inscrutability of reference in generating skeptical doubts, there is of course another issue in Quine's philosophy that appears to have similar skeptical implications: the underdetermination thesis. This thesis claims, roughly, that there are very different scientific theories which are supported equally well by all available empirical evidence. As mentioned earlier, the underdetermination thesis is, of course, one of Quine's motivations for naturalizing epistemology in the first place.

Underdetermination's apparent skeptical implications have been explained best by Lars Bergstrom (1993, 344–5). Assuming that knowledge is justified true belief, Bergstrom argues that the existence of theories that are rival to, possibly incompatible with, but equally well-supported by the evidence as one's own theory, undermine one's justification in believing one's own theory—and thus, one's knowledge. This is particularly clear in a case in which there are two clearly theories, T_0 and T_r, which are equivalent in evidence and theoretical virtues, but known to be incompatible.[38] Knowing that T_r rivals our home theory T_0, Bergstrom says the only rational option is to suspend judgment between the two. We cannot justifiably pick one or the other, and so we do know which one is true.[39]

Bergstrom (2004, 105) has pointed out, however, that the skeptical implications of underdetermination are clearest only when the rival empirically equivalent theories are taken to be incompatible, i.e., not possibly both true. Earlier (1993, 343, 345), he does suggest that if T_0 and T_r and not incompatible but merely different, there might still be something irrational about accepting one rather than the other when both account equally well for the evidence. But it is worth pausing on this point. If T_0 and T_r which are empirically equivalent but not logically incompatible, it is trickier to

[38] The incompatibility enters, presumably, because of a difference in theoretical claims, e.g. about unobservables. After all, the underdetermination thesis is the underdetermination of *theory* by observational evidence.)
[39] Bergstrom thinks the same is true even if we don't know the nature of T_r, but simply know it exists (if, for example, we accept Quine's underdetermination thesis).

show how skepticism would arise, because it is then possible to take what Quine has called an "ecumenical" (rather than a "sectarian") line, and say that both of these theories could be true (Quine 1986b). If both can be said to be true, then it seems there is no question to be agnostic about, no reason to think one's present theory is threatened. In fact Quine believes that many cases of apparently incompatible rivals can be reduced to compatible ones, if incompatibilities arising from theoretical terms are eliminated by spelling the relevant theoretical terms differently in each theory (e.g., "the universe expands" vs. "the youniverse does not expand") rendering claims predicated with them compatible. Bergstrom gives a variety of reasons (related to simplicity and economy) for thinking it strange that one could be warranted in believing the conjunction of T_0 and T_r to be true (1993, 347), and argues that the spelling expedient would not *eliminate* the existence of incompatible theories—if there are such—but only allow us to deal with their compatible counterparts (350–1). In any case, Quine later (1986; 1992, 99–101) distances himself from the ecumenical position, though he leaves open the possibility that a sectarian might oscillate back and forth between compatible rivals, without believing both at the same time.

There are some philosophers, particularly those enamored of verificationism, who think there could never be such things as empirically equivalent but incompatible rivals (Dummett 1973, 617; Davidson 1990b, 306). The issues involved in deciding whether they are correct are difficult and beyond the scope of this essay.[40] I do think that if empirically equivalent rivals are never incompatible, this would make the skeptical consequences of underdetermination less obvious. And Roger Gibson has pointed out that empirically equivalent but compatible theories might still count as instances of underdetermination, according to Quine, because Quine (1988) also stresses that underdetermined theories are such that they cannot be rendered logically *equivalent* (120). Even if empirically equivalent rivals are not incompatible, this does not mean they can be rendered equivalent (as through the spelling expedient). If Gibson is right, then the underdetermination thesis would not be

[40] See Bergstrom (2000, 101-4) for a summary of the debate.

contradicted by the compatibility of empirically equivalent rivals (though note that this leaves untouched whatever genuinely incompatible theories there may be, prior to the spelling expedient).

At the same time, however, I think the version of the underdetermination thesis that does not assume the incompatibility of rival theses becomes trivial or at least uninteresting from an epistemological perspective. Here it is important to remember *why* the underdetermination thesis is so important in Quine's philosophy in the first place. As early as "Two Dogmas of Empiricism" (1953b), Quine invokes the metaphor of a field of force (theory) underdetermined by its boundary conditions (experience), in order to show that there is no such thing as a belief that may be held true come what may, or a belief that is immune from revision. In an earlier chapter, I have argued that the underdetermination thesis is also crucial to solidifying Quine's critique of traditional epistemology. It is an important element of his critique of the "doctrinal project" in epistemology, the attempt to show how one's knowledge can be justified by experience, insofar as it eliminates as options any number of foundationalist proposals, including merely probabilist (as opposed to Cartesian) candidates. If underdetermined (but compatible) theories are all equally true, Quine has many fewer ways to argue that the doctrinal project of traditional epistemology has failed. Likewise, as I shall argue later, underdetermination, or at least underdetermination-style theses, are also crucial in establishing the indeterminacy of translation, and thereby undermining the "conceptual project" in epistemology", which I have also argued is another central motivation for naturalizing epistemology. If underdetermined theories are all equally true—as the ecumenical position suggests—then there is no reason to say there is no fact of the matter involved in choosing a translation manual. If all translations are equally right, then there are many facts of the matter on which they are passed, and translation is not indeterminate. So, if underdetermination is watered down to not require the incompatibility of empirically equivalent rivals, it is true that its skeptical implications become less clear, but by that same token it also loses its significance as a motivation for naturalizing epistemology. We could be satisfied with undermining underdetermination this early in the game, but since I think there is much

more of interest to explore, I will assume a version of the underdetermination thesis that motivates naturalized epistemology, we should examine the version that assumes that empirically equivalent rivals are incompatible. Indeed as late as "The Empirically Equivalent Systems of the World" (1975a) Quine does stress the *possibility* of incompatible rivals, even if not *all* empirically equivalent rivals are incompatible.

Between the inscrutability thesis and the underdetermination thesis, there does seem to be some support for Stroud's and William's contentions that Quine's resort to the scientific nature of skeptical doubt will do little to erase the skeptical implications of his own basic philosophic commitments. In the next section, however, I will explore responses offered on behalf of Quine himself and his supporters, which appear to undermine much of the force of the skepticism discussed above.

Quinean responses to skeptical challenges

Before endorsing Stroud or Williams, then, it is worth examining the fuller context of Quine's reply to Stroud, which might give us a better understanding of what Quine means when he says that the scientific nature of skeptical doubt shows that the skeptic is overreacting. Roger Gibson's (1988) defense of Quine against Stroud could shed some light here. Gibson maintains that Stroud's (1984) criticism of Quine (a recapitulation of much in Stroud's earlier article (1981)), fails because it neglects a central aspect of Quine's naturalism, the "reciprocal containment" of ontology (natural science) in epistemology and epistemology in ontology (natural science) (Quine 1969a, 83). The first containment is the view shared by both naturalized and traditional epistemology: the idea that we formulate our ontologies based on our accepted methods of acquiring knowledge. Just as the traditional epistemologist sought to construct science out of sense data, confining himself to its ontological deliverance, so the naturalized epistemologist acquiesces in the deliverances of the best

science, because he accepts that knowledge only arises from the senses. But the second containment, of epistemology in ontology, is distinctive to the naturalist.

Gibson argues that Stroud is neglecting the significance of this second containment, which implies that naturalized epistemology presupposes the existence of the external world, including the sensory inputs which it judges to be meager (59). Why does it matter that epistemology presupposes these claims? Gibson elaborates:

> The relevant point about the containment (of epistemology by ontology) is that transcendental epistemology is incoherent. The skeptic may indeed use a portion of science to bring doubt to bear upon science, but only by presupposing the truth of other portions of science. For example, the skeptic might show that some scientific posits are epistemologically unwarranted, but his epistemological deliverances presuppose his *interim* acceptance of other scientific posits, namely, those presupposed by his own theory of evidence (59–60, emphasis mine).

So, Stroud and others may be worried that the inscrutability thesis leads one to raise skeptical doubts about the ontology (by way of the reference) of one's beliefs. But Quine's response is that even in the act of doubting our ontology of rabbits, given the possibility of a "rabbit stage" ontology, we are still presupposing as fixed the ontology of nerve endings, etc., which leads us to see a disparity between meager input and torrential output in the first place. Quine makes precisely this point in "Things and Their Place in Theories" (1981b, 21) an essay which appears to have developed out of his original critique of Stroud:

> Epistemology, for me, or what comes nearest to it, is the study of how we animals can have contrived that very science, given just that sketchy neural input. It is this study that reveals that displacements of our ontology through proxy functions would have measured up to that neural input no less faithfully. To recognize this is not to repudiate the ontology in terms of which the recognition took place.

So on this view, even if we can doubt some things, we can't doubt everything all at once. Therefore even if inscrutability and underdetermination lead us to be skeptical about some things, *radical* skepticism is, indeed, an overreaction.

But, we might object, how stable is the science that is "presupposed" by skeptical doubts? At minimum, to "presuppose" means that we *used* to believe it, up until the point that it came to a general

conclusion about the reference of all terms, *including* the scientific ones used to formulate the original argument. This kind of presupposition still works perfectly well as a premise in a *reductio ad absurdum*, and yet it is a premise we might eventually come to reject as a result of that *reduction.* Note, after all, that Gibson says that what is presupposed by naturalized epistemology is *interim* acceptance of scientific theory. Quine himself (1960, 4) admits this much in a passage Gibson quotes immediately after making his point about interim acceptance:

> [O]ur questioning of objects can coherently begin only in relation to a system of theory which is itself predicated on our interim acceptances of objects. We are limited in how we can start even if not in where we may end up.

If "where we may end up" is not limited in the way that we start, that would seem to include "ending up" abandoning the objects we originally accept. Indeed, the wider context of this quotation from *Word and Object* suggests that this is a possibility Quine had in mind. He says that while we all start, like Dr. Johnson, affirming the existence of physical objects, we may come to find that best account of the world does not affirm this. Immediately after the sentences quoted by Gibson, Quine tells us: "To vary Neurath's figure with Wittgensteins' we may kick away the ladder only after we have climbed it" (1960, 4). This is clearly allowing for the possibility of kicking that ladder away.

Now Quine might have something other than skepticism in mind here, perhaps instead the possibility of coming to see the world composed of particles instead of commonsense objects. But in what follows he explains more about what he thinks could account for kicking the ladder away. Two paragraphs later, Quine explains that by beginning with physical object talk, we are merely assimilating a "cultural fare," without distinguishing between actual stimuli and what is posited additionally over and above them. He concludes (1960, 5):

> Retrospectively we may distinguish the components of theory-building, as we distinguish the proteins and carbohydrates while subsisting on them. We cannot strip away the conceptual trappings sentence by sentence and leave a description of the object world; but we can investigate the world, and man as a part of it, and thus find out what cues he could have of what goes on around him. Subtracting his cues from his world view, we get man's net contribution as the difference. This difference marks the extent of man's conceptual sovereignty—the domain within which he can revise theory while saving the data.

Quine's reference here to "the domain within which he can revise theory while saving the data" is yet another reference to his underdetermination thesis, or to his inscrutability of reference thesis. The continuing relevance of this point to Quine suggests that the containment of epistemology by ontology has little force to prevent the kind of *reductio* based on the underdetermination thesis, which Stroud envisions. Thus it seems we should concur with Stroud, along with Davidson (1990a, 74) and Koppelberg (1998, 266–7) who urge that the containment of epistemology in ontology is no panacea for the naturalistic response to skepticism. If our ontology contains scientific facts that suggest that we might abandon existence claims about physical objects—including the object presupposed by that scientific theory—then it seems we can, in fact, "kick away the ladder." Inscrutability and underdetermination do presuppose interim acceptance of scientific theory, but every *reduction ad absurdum* presupposes interim acceptance of whatever is to be reduced to absurdity.

Of course Quine has insisted he is not trying to challenge the coherence of the skeptic's doubts, so it is still unclear what he thinks the overall import of this scientific presupposition is supposed to be.[41] So perhaps we should find some further interpretation of his claim to block the "overreaction" of the skeptic. Indeed it is odd that both Stroud and Gibson focus their respective interpretations on only the final three paragraphs of "Reply to Stroud," neglecting to discuss the main body of the essay. There is much, in fact, to suggest that even though Quine knows that we are free to "kick away the ladder," he does not think this implies any skeptical threat. Immediately after saying that accepting the possibility of proxy functions does not imply that we must repudiate our ontology, he also says:

> We *can* repudiate it. We are free to switch, without doing violence to any evidence….But it is a confusion to suppose that we can stand aloof and recognize all the alternative ontologies as true in their several ways, all the envisaged worlds as

[41] I myself am sympathetic to the idea that the skeptic's doubts are incoherent, and that this by itself is sufficient to diagnose the skeptical illness. But I also agree with Stroud (1984, 227) that Quine's "view of language and his rejection of the philosophical use of synonymy or analyticity leave him in no position to appeal to what is or is not included in the meaning of a particular term," and that arguments from coherence do tend to presuppose specific theories of meaning, whether analytic or otherwise.

> real. It is a confusion of truth with evidential support. Truth is immanent, and there is no higher. We must speak from within a theory, albeit any of various. (1981a, 21)

Earlier in the essay, Quine makes a point that he also made directly in response to Stroud. He tells us that even when observation sentences are no longer treated holophrastically, but instead as composed of referring terms—even after we become adult philosophers and catch a glimmer of the possibility of replacing our terms with proxies—there is a way in which we are insulated from the effects of inscrutability:

> The point is not that we ourselves are casting about in vain for mooring. Staying aboard our own language and not rocking the boat, we are borne smoothly along on it and all is well; 'rabbit' denotes rabbits, and there is no sense in asking 'Rabbits in what sense of "rabbit"?' Reference goes inscrutable if, rocking the boat, we contemplate a permutational mapping of our language onto itself, or if we undertake translation. (1981a, 20)

Here Quine appears to be pulling back from a brink reached in "Ontological Relativity" (1969b), in which the notion of reference—one originally thought to be respectable and objective in "Notes on a Theory of Reference" (1953a)—appears to lose all such respectability and drop to the status enjoyed by murkier notions such as meaning. But Quine had hinted at this retreat even in "Ontological Relativity" when, arguing that questions of the reference of terms are answered only by translating them into other language, the resulting regress of translations could be halted only by "acquiescing in our mother tongue and taking its words at face value" (1969c, 49). Apparently what we do to refrain from rocking the boat with concerns over inscrutability is simply to acquiesce in this manner. Once we do this, we are able to maintain a "robust realism" about the reference of our terms, and affirm an "unswerving belief in external things—people, nerve endings, sticks, stones" (1981b, 245). This Quine sees as a reflection of his naturalism, the idea that truth is "immanent" to theory, that "it is within science itself, and not in some prior philosophy, that reality is to be identifies and described" (1981b, 246). So even if we can permute our preferred reference scheme with proxy functions, Quine's point is that we need not do what we can. By acquiescing in our mother tongue, we in effect accept a scheme of reference, and there is simply no question about other possible schemes.

Presumably a similar story can be told about our acceptance of theories, *mutatis mutandis*, that would obviate worries concerning underdetermination.

But does this strategy of acquiescence, based on the difference between what we can do and what we in fact do, provide a response to the skeptic that would satisfy the traditional epistemology? Retracing the steps by which Quine first formulated his inscrutability thesis suggest it is not. Consider one of his earliest formulations in *Word and Object* (1960, 51–2):

> For, consider "gavagai". Who knows but what the objects to which this term applies are not rabbits after all, but mere stages, or brief temporal segments, of rabbits? In either event the stimulus situations that prompt assent to "Gavagai" would be the same as for "Rabbit". Or perhaps the objects to which "gavagai" applies are all and sundry undetached parts of rabbits; again the stimulus meaning would register no difference.

The argument here is roughly parallel to Quine's argument for the indeterminacy of translation of whole sentences (except of course that Quine thinks inscrutability applies to terms even in observation sentences, while indeterminacy does *not* apply to observation sentences taken as wholes): he implies that we do *not* know the reference of the term "gavagai" (and later, "rabbit" itself) because the term could be equally true of rabbits, rabbit stages, etc., *given the same stimuli.* In other words, Quine wants to say there is no "fact of the matter" to determine reference, given that the only naturalistically respectable facts to consider are sensory stimuli and dispositions to assent, and these stimulations are logically compatible with any number of possible reference schemes.[42] This is parallel to his argument

[42] Now it might be objected that the argument for inscrutability listed above, regarding the compatibility of different reference schemes with identical stimuli and speech dispositions, is not Quine's only argument; other arguments may not carry with them this kind of significance for what we can do as opposed to what we do in fact do. Here one might appeal to the remarks of one commentator (Ben-Menahem 2005, 266) who says that Quine has two separate arguments for inscrutability: one the "informal" argument from an inability to extract "individuation schemes" from stimuli and speech dispositions, the other a "formal" argument exploiting certain logical properties of expressions. In the presentation above, I have not systematically separated these types of arguments. Arguments concerning rabbit vs. rabbit stage, and resulting questions like "Is this the same gavagai as that?" are examples of the first kind, which is more concerned with the situation of radical translation. Arguments concerning "proxy functions," are examples of the second, which are more concerned with inscrutability in the home language and even with our own utterances.

However I do not believe these arguments are fundamentally different: both turn on what we *can do* given the naturalistic facts of the matter in the same way. A review of the "formal" argument shows why. This argument is supposed to be independent of the "informal" argument, because even if we could settle on a determinate individuation scheme for some terms, it would at best settle questions of reference arising from *direct ostension*: from reference made through pointing to objects, etc. Even these determinate individuation

for indeterminacy of translation, where he likewise argues that any number of translation manuals for theoretical sentences are equally acceptable given identical stimuli and speech dispositions: since these stimuli and dispositions are the only relevant facts of the matter, and they are compatible with multiple translation manuals, there is no fact of the matter to decide between competing translation manuals.

So, is the fact that there is a difference between whether we *can* and whether we *do* permute our terms into proxies relevant to stopping the infiltration of inscrutability? From the above argument for inscrutability, I do not see why the difference is relevant to addressing the concerns of the traditional epistemologist. The entire argument for inscrutability derives not from what we *do* in fact do, but merely from what we *can*: we *can* use a variety of proxies in the same manner as our original terms, without doing violence to our stimuli and speech dispositions. This becomes clearer when, after

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schemes, however, would not settle questions of so-called "deferred ostension" (Quine 1969c, 40-41), reference made to objects other than objects pointed to, either through causal mediation or the relationship of instantiating a universal. In particular Quine discusses the attempt to use deferred ostension with abstract singular terms to refer to abstract objects. (I myself do not share Quine's (1960, 269) view that we have an ontological commitment to real abstract objects, and I find his arguments for this (Quine 1947) to be unpersuasive. This is, however, an issue beyond the scope of the present paper. Suffice it to say that on any account of universals, platonist or otherwise, it is clear that the reference of abstract terms cannot be settled through *mere* direct ostension. So the problem here is not unique to Quine's idiosyncratic views about ontological commitment.) Considering the example of a "thoughtful protosyntactician" who wishes to refer to the sentence types involved in his proof theory, Quine argues that to make reference to these abstract sentence types, he could map expressions onto sequences, which can in turn be mapped onto numbers (like Gödel numbers) (1969c, 41-2). Quine then considers the arithmetician, who could map numbers onto various set-theoretic constructs, any of which could be consistent with laws of arithmetic (43-44). The idea is that neither these laws, nor any amount of direct ostension towards the protosyntacticians expressions will settle the reference of the abstract *types* to which he wishes to refer. So: even if the "informal" argument does not succeed in rendering the reference of ostensive terms inscrutable, the "formal" argument is supposed to render inscrutable the reference of non-ostensive terms.

I think the above "formal" argument is no different in principle than the earlier argument about individuation. In fact it is not a uniquely formal *argument*, but simply an argument that is *about* formal properties (of mathematics and set theory). Whereas Quine's "informal" argument held that the reference of observation terms could not be settled by ostension, his "formal" argument simply says that even if direct ostension could settle anything, it couldn't settle the reference of abstract singular terms: do they refer to sequences, or numbers, or sets of sets? Like the earlier argument this argument simply identifies a range of reference schemes one *can* adopt given a fixed set of allegedly naturalistic facts: the only difference is that in the second argument the set of naturalistic facts has been charitably extended to include those related to direct ostension (a charity Quine is, in the end, reluctant to extend). Facts related to direct ostension do not settle the reference of abstract singular terms, nor do any related to laws of arithmetic. And in case it is objected that the arithmetic constraints are somehow independent of naturalistic concerns, it must be recalled that for Quine, mathematics, like logic, is of a piece with natural science, and continually tested through repeated applications in science (Quine 1995)

showing how reference can only be specified by bringing in (equally inscrutable) questions about identity and diversity ("Is this the same gavagai as that?," etc.), Quine observes the following:

> Two pointings may be pointings to a numerically identical rabbit, to numerically distinct rabbit parts, and to numerically distinct rabbit stages; the inscrutability lies not in resemblance, but in the anatomy of sentences. We *could* equate a native expression with any of the disparate English terms 'rabbit', 'rabbit stage', 'undetached rabbit part', etc and still, by compensatorily juggling the translation of numerical identity and associated particles, preserve conformity to stimulus meanings and occasion sentences. (1960, 53–4, emphasis mine)

Quine's argument for inscrutability, I conclude, turns on what we can do, not what we do in fact do. That is because it is an argument about whether there is a *fact of the matter* constraining what we do. The fact that we can permute our terms into any number of proxies reflects the fact that doing so is not inconsistent with our sensory stimuli and speech dispositions, i.e., it reflects the only facts that are facts of the matter. Thus there *is no* fact of the matter picking out one reference scheme rather than another. That is what the inscrutability thesis means: that there is no such fact. We are free to pick whichever reference scheme we like, including one that does not, perhaps, *remind us* of this thesis (the one according to which "rabbit" refers to rabbits). Whether or not we choose to remind ourselves of our ignorance does not change the fact of our ignorance. Simply saying that "rabbits" refers to rabbits does not create a *fact* about reference.

Given the above, it appears that every argument Quine advances for the inscrutability of reference really does diminish the significance of the mother-tongue acquiescence strategy, at least from the perspective of *obviating the worries of the skeptic, to the satisfaction of the traditional epistemologist*. But of course Quine's worries are not necessarily those of the traditional epistemologist. Let me suggest that each of Quine's responses to the skeptic can make a limited amount of sense, provided that we stop trying to understand him as pursuing the goals of traditional epistemology. Of course insofar as naturalized epistemology is understood in contrast with traditional epistemology in the first place, it might seem that we should never have thought of him as pursuing these goals in the first place. But it would seem that way only if we neglect that Quine has long been

claiming that there is some subject matter that is shared in common by traditional and naturalized epistemology. Indeed the naturalists I called "optimistic" thought that epistemology could pursue traditional goals (for example, explaining how our beliefs are justified) simply by adopting unconventional, naturalistic means. Chapters 2 and 3 called much of this into question, of course, and we are now seeing the full nature of the break between the optimist and the pessimist. The pessimistic naturalistic epistemology demurs even of achieving (many) traditional epistemological goals: of showing how beliefs in our ontologies can be justified, i.e., logically justified (deductively or inductively). What it shares as a common subject matter with traditional epistemology is not the goal of the logical justification of beliefs, but simply *some explanation or other* of our beliefs. As it happens, the kind of explanation Quinean naturalists seem to have in mind is also a form of justification, only not *logical* justification, but *pragmatic* justification. In the next section, I will explain how each of the elements of Quine's approach to the skeptic is imbued with this pragmatism.

Pragmatism and naturalism

First, let us consider the reciprocal containment point, in conjunction with Quine's contention that skeptical doubts presuppose the acceptance of ordinary scientific ontology. What exactly does Quine mean by "presuppose" here? Clearly in order to get to the point of accepting the inscrutability and underdetermination thesis, one needs to have *once* accepted various putative truths of science. My point in the section above is that this does not guarantee that one accepts them any more. (It is possible to "kick away the ladder.") I also mention that inscrutability and underdetermination each have a perfectly *general* scope, i.e., each concerns every scientific term or theory, including the ones used to describe meager sensory input. Because of this, it seems that it might even be incoherent to accept *both* the scientific ontology *and* the results of the inscrutability and underdetermination theses—and that this incoherence might serve as a *reductio* of the acceptance of scientific ontology. But as I have

continually emphasized, Quine claims that there is no such incoherence. How might he explain the incoherence away?

I think that the answer is that Quine's "presupposition" thesis can only make sense in combination with his "acquiescence" thesis, his idea that we face no problem of inscrutability when we acquiesce in our mother tongue. The second thesis helps us to see that there is, in effect, a use/mention confusion in the suggestion that there is an incompatibility between acceptance of inscrutability and acceptance of scientific ontology. When a naturalized epistemologist argues for inscrutability of reference, he *uses* some scientific terms in order to come to a conclusion in which he *mentions* that "rabbit" and "rabbit stage" are reference schemes compatible with the meager sensory input. When the naturalized epistemologist uses those scientific terms, he is *himself* acquiescing in his mother scientific tongue. If he should then turn to zoology and begin to reason about *rabbits*, he once again acquiesces in his mother tongue. And this acquiescence is fully compatible with his embrace of the inscrutability of reference, for that embrace merely mentions "rabbit" and its rivals; it does not use them.

The natural question to raise at this point is: what if the scientist, recognizing the inscrutability of reference, suddenly decides to abandon the disquotational reference scheme, and affirm that "rabbit" refers to rabbit-stages (mapping his own language onto itself)? Quine would have to allow this possibility. Of course it does not immediately threaten the scientific ontology used to generate the inscrutability thesis, since that ontology presumably contained nerve endings, etc., rather than rabbits. Even so, Quine would have to allow that decisions to abandon disquotation are possible given the recognition of inscrutability. Why, then, does he seem unconcerned? Here, I think, is where his pragmatism enters. Why pick "rabbit" rather than "rabbit stages"? Because a disquotational reference scheme is simply easier to apply. Perhaps there will be cases in which we avoid disquotation—perhaps we commit malapropisms—but these will be exceptional cases, and will make sense only against the background of lots of other disquotationally-generated reference. Accepting this reference scheme

because it is easier does not imply, of course, that we now have some logical justification in terms of reference-facts; it only means we have pragmatic justification.

There is another dimension of pragmatism to the reference scheme we accept. When deciding which terms to *use*, our decision is pragmatic in the sense that disquotation is easy. But when the naturalized epistemologist *mentions* the terms we use on the meta-level, and discusses what it is we talk about when we use them, he may be inclined to say the following:

> To say what objects someone is talking about is to *say no more* than how we propose to translate his terms into ours; we are free to vary the decision with a proxy function…. Structure is what matters to a theory, and not the choice of its objects. F.P. Ramsey urged this point fifty years ago, arguing along other lines, and in a vague way it had been a persistent theme also in Russells' *Analysis of Matter*. But Ramsey and Russell were talking only of what they called theoretical objects, as opposed to observable objects. I extend the doctrine to objects generally, for I see all objects as theoretical….The objects, or values of variables, serve merely as indices along the way, and we may permute or supplant them as we please as long as the sentence-to-sentence structure is preserved. The scientific system, ontology and all, is a conceptual bridge of our own making, linking sensory stimulation to sensory stimulation. (Quine 1981b, 20, emphasis mine)

Or he may even say this:

> What then does our overall scientific theory really *claim* regarding the world? *Only* that it is somehow so structured as to assure the sequences of stimulation that our theory gives us [sic] to expect. More concrete demands are indifferent to our scientific theory itself, what with the freedom of proxy functions. (Quine 1981a, 474, emphasis mine)[43]

When speaking as a naturalized epistemologist on the meta-level, then, Quine seems to describe an *almost* fully instrumentalist or pragmatist semantics. I say "almost" because he does allow that our theoretical terms might at least refer to how the world is "structured so as to assure sequences of stimulation that our theory gives us to expect." Mentioning that structuring might be taken to mean

[43] It has been brought to my attention that if Quine accepts this as a serious statement about the content of scientific theories, it may have the effect of truly trivializing his statement of the underdetermination thesis. It would imply that empirically equivalent theories are also logically equivalent, and therefore certainly logically compatible. Of course, this is only if the statement is interpreted in a purely phenomenalist manner, not in the structural realist manner. As I've said, there's a fine line between the two. In any case, I think that Quine did have a tendency to entertain more and more trivial versions of underdetermination as the years went by. All I can say is that the more trivial they become, the less motivated naturalized epistemology becomes. The trouble is that while Quine lost confidence in underdetermination, he kept confidence in the project of naturalizing epistemology. This is trouble because without the first, there may have been little motivation for the second.

that we refer to underlying essences which somehow order our sensations, as in a two-factor theory of reference. But what is important for Quine, who disavows the naturalistic respectability of natural kinds, is that any number of possible reference schemes can exhibit the same structure. There is a fine line, then, between the possibility of the world's exhibiting the same structure through many different ontologies, and *our experience* having the same structure, regardless of the world's ontology. When speaking on the meta-level, it is hard to see whether the naturalized epistemologist is committed to structural realism or simply to phenomenalism. In either case, the pragmatic element is all that matters: what matters to speaking of objects is the role the play in permitting us to explain and predict our "sequences of stimulation." Even if the content of our statements about objects is not exhausted by their pragmatic role, the inferential *significance* of our statements about objects is.

To many, the idea that claims about objects are not primarily claims about external, mind-independent objects is already to concede everything to the skeptic. (Lars Bergstrom (1993, 255) argues something along these lines.) But skepticism is just the position that our beliefs (about the external world, or anything else) are not *justified*. So the same epistemology may look skeptical to those who hold one standard of justification, but non-skeptical to others. Because of his inscrutability and underdetermination theses, Quine cannot accept that there is full *logical* (deductive or inductive) justification for belief in our preferred theories or ontology. So he accepts what looks like skepticism from the perspective of the traditional epistemologist (who demands logical justification for our beliefs to avoid skepticism). But what he accepts is not skepticism by reference to a pragmatic standard of justification.

Quine's commitment to pragmatism is especially evident in his discussion of theory choice. While he thinks that theory is *logically* underdetermined by evidence, he thinks choices among empirically equivalent rivals are ultimately based on pragmatic concerns. In his later works, he enumerates a list of "theoretical virtues" possessed by our preferred scientific theories: conservatism,

generality, simplicity, refutability, and modesty (1992, 20). Elsewhere Quine (1992, 15) explicitly links theoretical virtues like simplicity and "minimum mutilation" with predictive power:

> [T]he ultimate objective is so to choose the revision as to maximize future success in prediction: future coverage of true observation categoricals. There is no recipe for this, but maximization of simplicity and minimization of mutilation are maxims by which science strives for vindication in future predictions.

How, then, is Quine responding to the skeptic if he concedes that our beliefs lack full logical justification—even if they are supplemented by pragmatic justification? The difference between his mention of the skeptic's "overreaction" in "Reply to Stroud" and "Things and their Place in Theories" is revealing. In the first, he simply says that his "only criticism of the skeptic is that he is overreacting" (1981a, 475). In the second, he says the skeptic is merely "overreacting when he repudiates science across the board" (1981b, 22). The first excerpt doesn't specify the respect in which Quine thinks the skeptic is overreacting. This has led some (including critics and defenders) to conclude that Quine thinks the skeptic's embrace of *skepticism* is the overreaction. The second excerpt clarifies that it is simply the skeptic's repudiation of science that is the problem. If Quine is a pragmatist, however, he can embrace both the skeptic's thesis that our beliefs are not fully logically justified *and* embrace the pragmatic power of science that a skeptic might be inclined to reject on this ground. He can embrace the latter because even if science does not deliver full logical justification of our beliefs, in a way that circumvents inscrutability and underdetermination, it does deliver all the pragmatic justification we need to predict and control our experiences.

Accepting Quine as both a pragmatist and skeptic (understood in the traditional sense) helps to explain a number of other interpretative quandaries about Quine's response to skepticism.

One way in which Quine responds to Stroud's worries about the skeptical implications of inscrutability is to find solace in the fact that truth of observation sentences is prior to reference on this thesis. Inscrutability allows that given our stock of observations, we may still assert a constant set of observation sentences, which we have no option but to regard as true. Even if we cannot divide the reference of our sentence in a determinate way, taken *holophrastically* they can still be asserted to be

true. Truth here is understood in a deflationary manner: to assert a sentence as true is simply to assert it. We do not need correspondence relations to understand truth.

But at first this response seems unresponsive to Stroud's claim. Stroud has claimed that this is a straightforward endeavor to study the relationship between sensory input and theoretical output, provided that we are in a position to observe important facts about the subject's environment and its relationship to him. If we see that what a subject claims is true, and if his claim is a reliable one, we have no problem explaining the origin of his knowledge. Stroud observes, however, that the mere truth of the subject's beliefs is not sufficient to explain this knowledge, although it is necessary (1981, 461). If the subject claims there are bodies, and we see there are none, we know the subject does not know there are bodies. But even if the subject's output is true, the subject does not necessarily know if it is only accidentally true. If, for example, the subject claims there are bodies in front of him, and there are—but they are behind a screen past which he cannot see—then his output is only accidentally true and he does not know. Stroud goes even further, and claims that if we have no access to truth or reliability of the subject's beliefs, we cannot decide whether they know. But if we glean from our naturalistic study of the subject's meager inputs that theory is underdetermined by evidence, we will come to see our own theories as likewise underdetermined, and we cannot understand how *we* can ever come to know anything, including the claims needed for evaluating whether or not the subject knows (462–3).

In response to this, for Quine simply to assert that one might assert true observation sentences in response to some evidence *seems* to be missing the point. So what if the observation sentences are true—even if they come along with some sensory evidence? Gettier cases are plentiful, and show that in traditional views of knowledge, justified true belief is not sufficient for knowledge. Presumably this is what Stroud is emphasizing when he claims that it is not enough that a subject's belief be accidentally true. But Quine is only missing the point here if Stroud is not. Stroud appears to believe that Quine is trying to offer a *refutation* of the skeptic, some positive naturalistic case for how we can

show that both we and the subject can have logically justified beliefs. But if Quine is not even trying to leverage naturalism to answer traditional epistemological questions, then Stroud is missing the point, and Quine is not. Quine (1981a, 474) seems to suggest as much in the following:

> Stroud finds difficulty in reconciling my naturalistic stance with my concern with how we gain our knowledge of the world. We may stimulate a psychological subject and compare his resulting beliefs with the facts as we know them; this much Stroud grants, but he demurs at our projecting ourselves into the subject's place, since we no longer have the independent facts to compare with. My answer is that this projection must be seen not transcendentally but as a routine matter of analogies and causal hypotheses. True, we must hedge the perhaps too stringent connotations of the verb "know"; but such is fallibilism.

This is the paragraph that directly precedes the paragraph in which Quine laments the skeptic's "overreaction." Seen in this context, it should be especially clear that whatever Quine's response is to the skeptic, it has little to do with traditional epistemological responses to skepticism. Quine's last line, about not being too stringent about the verb "know" is particularly revealing. It shows that he is not interested in holding onto an epistemology that explains the origin of knowledge in a manner faithful to the "traditional" concept of knowledge (some kind of logically justified true belief). Quine is, after all, not much interested in "traditional" concepts of anything. He is not interested in conceptual analysis as a method in philosophy. He is interesting only in *explication*: the process of taking some pre-existing concept and modifying it to make it useful for theoretical purposes. Quine would probably say that the traditional "justified true belief" concept of "know" serves no important purposes, hence it is safe to discard.

Another interpretive quandary resolved by treating Quine as a pragmatist is the paragraph that immediately follows his paragraph about the skeptic's overreaction:

> Experience might, tomorrow, take a turn that would justify the skeptic's doubts about external objects. Our success in predicting observations might fall of sharply, and concomitantly with this we might begin to be somewhat successful in basing predictions upon dreams or reveries. At that point we might reasonably doubt our theory of nature in even its broadest outlines. But our doubts would still be immanent, and of a piece with the scientific endeavor. (1981a, 475)

Commenting on this passage, Stroud (1984) considers that it may have something to do with Quine's lament of the skeptic's overreaction. Indeed it does, but not in the way that Stroud proposes. Stroud suggests that Quine thinks that only if the predictive power of science wanes should we take the skeptic seriously. Since its predictive power has not waned, we should therefore reject skepticism. On this view, skepticism would be a doctrine that is itself subject to confirmation. Stroud notes that this is not what the skeptic says: the skeptic does not take a position about some rival source of our beliefs (e.g., dreams rather than science): he only says none of our knowledge, whatever its content, is logically justified. As a result, Stroud thinks that Quine's alleged answer to skepticism is knocking down a straw man, and ineffective.

But in light of my reading of Quine as a pragmatist who is willing to concede that our beliefs are not fully logically justified, we can interpret him differently here. Roger Gibson, a defender of Quine, castigates Stroud for alleging that Quine takes skepticism to be subject to confirmation (1988, 59). And surely skepticism understood in the traditional way, as thesis that our beliefs are not fully logically justified, is not a thesis that we would treat as subject to confirmation. Gibson goes on to object that Stroud misses the importance of reciprocal containment, which we have already discussed. Curiously, however, Gibson offers no alternative explanation for what Quine actually means in the passage about the possibility that our predictions might some day lose their power. Now I think we can offer an explanation. Even if the traditional thesis of skepticism is not subject to confirmation, a pragmatic version of the thesis might be. That is to say, if skepticism is the idea that our beliefs are not justified, then if we admit pragmatic justification as one species of justification, whether or not skepticism is true will depend on whether or not our beliefs are pragmatically justified. And *that* is a thesis that we can imagine being subject to confirmation. Whether or not our beliefs are pragmatically justified depends on whether or not they have significant explanatory and predictive power. In the passage quoted above, Quine is saying that we trust scientific beliefs because they *do* allow us to predict observations. If someday they stop allowing this, then we would no longer regard them as

(pragmatically) justified. If dreams instead turned out to yield the best predictions, we would regard them as justified instead.

It should come as no surprise that Quine is willing to embrace the thesis that our beliefs are not logically justified—even if he thinks they are pragmatically justified. Quine (1969a, 72), after all, famously announced in "Epistemology Naturalized" that the doctrinal and conceptual projects of epistemology had failed, and that "the Humean predicament is the human predicament." Furthermore, there is the tantalizing line at the end of his description of making epistemology a "chapter of psychology," in which he mentions that one of the *purposes* of naturalized epistemology is to discover how "one's theory of nature transcends any available evidence." This seems to build the problem of underdetermination—and any associated traditional skeptical theses—into the naturalist project from the beginning.

After all, if we accept that we can never understand our beliefs to be fully logically justified, there is a problem: if our beliefs have not been determined by evidence, what then *are* they determined by? What makes it possible for us to have scientific output that is "torrential" in comparison to our "meager" perceptual input? Quine's answer is contained in *Word and Object*, *The Roots of Reference*, and his other works in which he describes the variety of accidental, analogical, and otherwise non-logical devices by which such theory is formed. It is not intended to show how our knowledge is justified, but how our "knowledge" arises in a world where justification in the traditional sense is not an option.[44] It is not even intended to show us how our sensory evidence connects us cognitively to independent facts. As Quine wrote in "Epistemology Naturalized," "Awareness ceased to be demanded when we gave up trying to justify our knowledge of the external world by rational reconstruction" (1969a, 84). The variety of accidental, analogical, and other non-logical devices that

[44] Note that this does not imply that Quine has necessarily abandoned epistemology as a normative project. As I have already suggested, he still has the option of naturalizing normativity, of showing how these various theoretical developments have served some adaptive function for us. So they would count as "good" beliefs from the perspective of natural selection, perhaps. Even so, they would only be normative in this new sense, not in the traditional epistemological sense of justifying our beliefs.

do explain the origin of our theory are *pragmatic* devices. As Quine (1953b, 46) remarks in "Two Dogmas of Empiricism":

> In repudiating [the boundary between the analytic and the synthetic], I espouse a more through pragmatism. Each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in warping his scientific heritage to fit his continuing sensory promptings are, where rational, pragmatic.

Stroud, Williams and others have noted the statements about the Humean predicament, and have considered that Quine might not intend to *refute* the skeptic. But they are usually perplexed by Quine's statements, in *The Roots of Reference* and elsewhere, which analyze skeptical doubts as arising from science. These in combination with Quine's claims that the skeptic is overreacting make critics think Quine is simply inconsistent: sometimes he concedes the full force of skepticism, other times he wishes to answer it. But if my reading is correct, Quine is not inconsistent. His remarks about the scientific source of (local) skeptical doubts are simply aspects of his attempt to show that even if we accept that our beliefs are not fully logically justified, this is no cause for concern. The skeptic overreacts by ignoring the possibility of pragmatic justification, which in turn explains why we are able to acquiesce in our mother tongue and mother scientific theory. This is satisfactory if you share Quine's affinity to pragmatism, but not otherwise. Since his acquiescence strategy does nothing to calm traditional skeptical doubts or show that really our beliefs have logical justification after all, in a way it is really an acquiescence in skepticism itself. Speaking in the language of the traditional epistemology, Quine is arguing that we should learn to live with skepticism, by taking pragmatism as a source of solace.

Does pragmatism support naturalism?

Perhaps pragmatism is a source of solace, but is it the right kind? Quine wants to be pragmatist about justification, but at the same time wants to privilege the pragmatic power of science. Every element of his naturalized epistemology has been motivated by the commitment to scientific

theory as the highest form of human discourse. Any form of discourse—intensional or modal, etc.—which does not fit into the working vocabulary of rigorous natural science has been disqualified as inadmissible for philosophic purposes.

But if Quine is not attempting to offer a traditional, logical justification of scientific theory, and is merely pursuing a pragmatic justification, what manner of pragmatic justification privileges natural science over the many other modes of human discourse? Richard Rorty (1979, 171) raises a similar question:

> Quine, after arguing that there is no line between science and philosophy, tends to assume that he has thereby shown that science can replace philosophy. But it is not clear why natural science, rather than the arts, or politics, or religion, should take over the area left vacant.

Later he continues:

> The conviction that science differed from softer discourse in having "objective reference" to things "out there" was bolstered in pre-Quinean days by the thought that…there certainly were points of contact with the world in the presentations of sense. This contact…seemed to give science what was lacking in religion and politics—the ability to use contact with the real as the touchstone of truth. The horror which greeted Quine's overthrow of the dogmas…was a result of the fear that there might be no such touchstone. For if we once admitted that Newton was better than Aristotle not because his words better corresponded to reality but simply because Newton made us better able to cope, there would be nothing to distinguish science from religion or politics (269).

Rorty, of course, thinks that there *can* be no principled distinction between the pragmatic value of science and that of arts-politics-religion. He thinks that Quine (and others) have shown us, inadvertently, that some doctrine is acceptable just in case it is consistent with the standards—scientific or otherwise—of our cultural peers. And Rorty is not the only one to have seen an affinity between his views and Quine's. Note the following from Hilary Putnam (2004, 65):

> [I]f neither criterion has any pretension to providing a sense in which our propositions are capable of mapping the behavior of specific hunks of reality…then valorizing prediction of nerve-stimulations over "coping" broadly construed is (as Rorty tirelessly points out) utterly arbitrary. Quine, it seems to me, gave up realism without noticing that he did, because he thought that as long as he valorized scientific discourse above all other discourse, this *made* him a realist…. "Naturalism" is unstable indeed if it slides so easily into Rortian antirealism.

Quine, of course, would likely disavow the extreme cultural relativism espoused by Rorty. The question, of course, is how he would then propose to privilege the pragmatic authority of *Naturwissenschaften* over *Geistewissenschafen?* This is, of course, a question that would need to be addressed within the confines of science itself—or at least in whatever immanent theory we happen to find ourselves in. Presumably Quine could present a formidable argument showing that only the scientific pragmatic virtues of simplicity, conservatism, and empiricism are conducive to predicting further sequences of sensory stimulation. But what if other pragmatists are not so interested in predicting their sensory sequences? Surely prediction enables control, but what if other pragmatists are interested in coping with life through other means than controlling life's experiences? If Quine wanted to give a definitive answer to this question, he would need to find a way of naturalizing the norms of prediction and control. Given the material I have already discussed in chapter 3 concerning the difficulty of naturalizing normativity, Quine would face an uphill battle.

Of course this criticism might be besides the point. Probably all Quine ever intended by his naturalism was to exalt science as the ultimate source of *truth*, not the ultimate source of coping. Whether and to what extent truth is to be understood as a norm is, perhaps, a secondary question. The primary question is: whatever our reasons for wanting the truth, how do we find it? If Quine could then argue that whatever form of discourse permitted us to predict our sequences of sensory stimulations *also* thereby permitted us to make conclusions about the truth would be the discourse of interest to the philosophical naturalist. We already recognize that wherever questions about unobservables or other theoretical matters are underdetermined by the evidence, we must rely on pragmatic virtues which are not themselves linked to truth. This much pragmatism possibly threatens scientific realism, though not necessarily realism *per se*. But there is still the possibility that at least our observation sentences, the statements which report our sensory stimulations, may be adequately linked with truth. The question is how.

In a separate article, Putnam (1995) presents some serious difficulties for the Quinean attempt to formulate a truth-linked pragmatism, even about observation sentences. In the remainder of this section, I will present some highlights of Putnam's concerns. I will not have occasion to consider objections to Putnam, and for this reason I do not mean to endorse him wholeheartedly.

Putnam begins by considering Quine's reliance on a deflationary notion of truth, as explicated by a Tarski-style true-in-L predicate, according to which "Snow is white" is true-in-L if and only if *snow is white*. This is a notion of truth that we are supposed to be able to rely on in spite of problems of referential inscrutability. Of course the Tarski definition applies directly only when the object language is contained in the metalanguage. If we want to state the true-in-German definition for "Schnee ist weiss," then we need to translate "Schnee ist weiss" from German with its English equivalent, and say "Schnee ist weiss" is true-in-German if and only if snow is white. But then we need to know that our translation is correct, which Quine of course says there is no fact of the matter to determine. Therefore sentences in German are true or false only in relation to a translation manual with English. This quandary even applies to our home language if we view it as just a set of speech dispositions. But as we have already explored, we can take solace in acquiescing in our mother tongue, taking our words as face value. We can assert wholeheartedly that "Snow is white" is true if and only if *snow is white*. So only in acquiescing in our own language do we grasp that our words refer: but where there is no theory, there is no determinate reference. Apart the context of our language, there are no determinate reference relations between our words and the world.

Much of this we have already explored. But Putnam considers the natural question one often encountered after examining the Tarski truth definition: how are we ever to *know* whether or not snow is white? The usual Quinean response is that this is a question for epistemology, not for a theory of truth. Whatever leads us to say *snow is white* is what leads us to accept that "snow is white" is true. But Putnam asks, "how can there be an epistemology in connection with truth if there is no truth to be found?" (339). He notes a shortcoming in the analogy Quine draws between the relativity of truth to a

translation manual and the relativity of position to a coordinate system. We can give an absolute statement of position relative to a coordinate system even if we are not using that coordinate system, but we cannot even give an absolute statement of truth relative to a translation manual if that manual is not one in which we have acquiesced. (He gives an example using a Martian linguist whose meta-language is not necessarily ours.) Noting this, he says:

> The only solution consonant with Quine's general position that I can see would be to abandon the geometrical analogy and to say that in the case of my own language, calling a sentence 'true' is doing no more than reaffirming the sentence. I am not ascribing any property, not even a relative property, to the sentence when I say that it is true. I am just assenting to the sentence. Quine himself puts the matter this way when he says that 'to say that a sentence is true is just to reaffirm the sentence.' On this interpretation, to say, as Quine does, that there is only 'immanent truth' is—as close as makes no difference—to say *il n'y a pas de hors text* [there's nothing outside the text]. (341)

If Putnam is correct, and not even such a thing as an absolute relative truth predicate, it does sound like truth is no predicate at all, just a device used for semantic ascent. Perhaps a device like this is all we need if we are interested in describing our agreement or disagreement with others, and other pragmatic functions often cited by advocates of the deflationary conception of truth. But it would seem that if we want to be able to show why science rather than art-politics-religion is the preferred mode of human discourse, we will need a truth *predicate* that does more work than that. We want to be able to say, quite abstractly, that there are certain modes of discourse which produce *true statements* about the world, in virtue of features of that discourse which are *truth conducive*. Part of the reason we need to be able to state it abstractly is precisely because we do not know which examples are supposed to fall under the extension of "truth" to begin with. If we are approaching a new question to answer, and we have two competing worldviews telling us how to answer it, as epistemologists we want to know which worldview has the *general features* that make it conducive to truth. We want to be able to say that one, rather than the other, derives from the senses, and that the senses are in causal contact with the world. It is not enough just to be able to say that one worldview produces various sentences, and then affirm those sentences. Often competing worldviews agree on a lot. Even Quine's naturalized

epistemology attempts to do more than this. He notes that science makes an important connection between theory and the senses. But why is any special connection needed at all? Why do we even need any method for pursuing truth, if truth is not any distinctive property?

I offer Putnam's concerns above simply as evidence of a difficulty with Quinean pragmatism that *must* be settled if naturalized epistemology is to retain its exclusive reliance on natural science. Because I am not presenting a comprehensive case for Putnam's view, I take it that in this chapter I have *not* proven that Quinean naturalism is inadequate on its own terms. (It seems to me possible that a deflationist could address some of the questions I raise above about how deflationism could characterize the general property of truth-conduciveness needed by epistemology.) I will have shown only two things up until this point: 1) Traditional epistemologists who are interested in justification of the truth of scientific claims beyond those immediately related to sensory stimulations will see Quinean naturalism as a form of skepticism, and 2) Quinean naturalism faces serious difficulties even in formulating an epistemology that links sensory stimulations to truth. Both of these points taken together will give us reason, if not to reject Quinean naturalism outright, then at least to decide if it is well-motivated. If we do not like the idea of giving up traditional epistemology, and if we are not up to the difficult task of solving Putnam's problem, we should decide if we really have to. In the final section of this chapter, I will explore the source of the principles which motivate Quinean naturalism. This will enable us, in our final chapter, to assess the source of these principles, and decide if we really need to abandon traditional epistemology and be Quinean naturalists after all.

Proximate sources of inscrutability and indeterminacy

In this final section, I would like to show how two of the most important sources of Quine's rejection of traditional epistemology—his inscrutability of reference and indeterminacy of translation theses—ultimately reduce to another: his underdetermination thesis, or more broadly the confirmation holism from which underdetermination immediately springs. This will not be an exercise in reduction

for the sake of reduction, because I think that in the end, showing the ultimate sources of Quine's rejection of traditional epistemology (and acquiescence in skepticism, traditionally understood) will provide us with a clue for how to circumvent that position. Furthermore, showing the roots of Quine's theses in underdetermination will help solidify my claim that there is a sense in which the proposal to naturalize epistemology represents an acquiescence in skepticism, traditionally understood.

Let us begin with a brief review of how Quine wields his various negative theses (underdetermination, inscrutability, indeterminacy) as weapons against traditional epistemology. This is important to do if we are to show that Quine's case for naturalism is not just a "pessimistic induction" about the failures of past epistemologies. That is certainly a part of his case, but not the whole. In "Epistemology Naturalized," Quine (1969a, 69–70) divides epistemology into two projects, the "doctrinal" and the "conceptual". The first of these is concerned with justifying our knowledge (for empiricism, in sensory terms), whereas the second is concerned with clarifying the meaning of key concepts (which relates to the doctrinal project insofar as translation of obscure truths into clearer ones can help justify them). Quine notes that the doctrinal project had failed in empiricism at least by the time that Hume had formulated his problem of induction (this is what Quine means by the "Humean predicament") (71–2). The conceptual project, however, was kept alive as long as various empiricists attempted to reduce talk of "bodies" to sensory terms, although again, Quine thinks the most advanced attempt (Carnap's) was a failure. But this is not the whole story.

Critics such as Kim (1988) have argued that in citing these failures as reasons to abandon traditional epistemology *in toto*, Quine has unjustifiably discounted the possibility of new developments of either the doctrinal or conceptual projects that could fare better than earlier attempts. In chapter 3, however, I have argued that *each* of the new developments proposed by Kim could fall prey to one or another of Quine's negative theses. Kim first focuses on the doctrinal project, listing probabilism, coherentism, and externalism as projects worthy of exploration, under the auspices of traditional epistemology. The first of these is ruled out by underdetermination, which implies that not

even the belief in physical objects is more probable than a belief in the gods of Homer (the sole difference is pragmatic). Traditional coherentism is no option for Quine, because of his rejection of analytic *a prioricity*, which is needed by coherentism to assign initial plausibility to certain beliefs to prevent the justification of coherent fantasies. After having examined Stroud's critiques of Quine, which in effect attributed to Quine an externalist account of justification, we can now see in particular why externalism offers no hope for genuine justification on Quine's view. In addition to this, any of these views which attempt to *analyze* "justification" in terms of probability, coherence, or reliability of belief-formation, are to be repudiated out of hand by Quine, insofar as he rejects "conceptual analysis" as a proper philosophic methodology, because of his rejection of meaning and embrace of indeterminacy. Even Kim's own preferred concept of philosophic methodology, supervenience, is likely to be dismissed on naturalistic grounds, owing to its reliance on suspicious modal concepts.

As I have argued in earlier sections, Quine's negative theses, in particular the inscrutability of reference and the underdetermination of theory by evidence imply skepticisms of their own. It is thus not surprising that so wielded, they should also rule out alternative doctrinal projects as proposed by Kim and others. So what is the source of these negative theses? I shall begin with the indeterminacy of translation, and show how it reduces either directly to an underdetermination thesis or to the inscrutability of reference, which then itself reduces to another underdetermination-style thesis. The ways in which indeterminacy and inscrutability reduce to underdetermination are not precisely reductions to the underdetermination thesis, per se (the one about theory and evidence), because strictly speaking, translation manuals and reference schemes are not theories. Thus Quine and defenders of Quine have long held that there are important differences between indeterminacy and inscrutability on one hand, and *the* underdetermination thesis on the other (Quine 1969, 303; Quine 1970, 80; Quine 1981b, 23; Quine 1987, 10; Quine 1992, 101; Gibson 1986; Peijnenburg and Hünneman 2001). That is true, there is a distinction. But the distinction itself is a product of the

arguments for indeterminacy and inscrutability, which themselves turn on a premise reminiscent of the underdetermination thesis.

First, let's examine the indeterminacy of translation—a device that could be exploited by Quine to reinforce his critique of *anyone's* attempt to define conditions for knowledge or justification, analytically or otherwise. In his "On the Reasons for the Indeterminacy of Translation" (1970), Quine outlines two separate arguments for indeterminacy. The first, which he denotes by "pressing from above," is from the underdetermination of physical theory. The second, "pressing from below," is from the inscrutability of reference. Like Quine, I will focus on "pressing from above" first.

In his first argument, Quine takes it for granted—and expects that others will concede—that physical theory is underdetermined, not only by past observations, but by all possible observations. Turning to the question of translation, Quine argues that it is easy and objective enough to match our observation sentences with those of the foreigner, but that translating theoretical sentences requires projecting analytical hypotheses. It is with analytical hypotheses for translating *theory* that the bulk of indeterminacy enters, because:

> [N]ow the same old empirical slack, the old indeterminacy between physical theories, recurs at second intension. Insofar as the truth of a physical theory is underdetermined by observables, the translation of the foreigner's physical theory is underdetermined by translation of his observation sentences. If our physical theory can vary though all possible observations be fixed, then our translation of his physical theory can vary though our translations of all possible observation reports on his part be fixed. Our translation of his observation sentences no more fixes our translation of his physical theory than our own possible observations fix our own physical theory (1970, 180).

This confirms what one might have suspected from reading *Word and Object*, that the indeterminacy of translation follows from the fact that translation manuals are underdetermined by the relevant naturalistic facts, sensory stimuli and speech dispositions:

> Yet one has only to reflect on the nature of possible data and methods to appreciate the indeterminacy. Sentences translatable outright, translatable by independent evidence of stimulatory occasions, are sparse and must woefully under-determine the analytical hypotheses on which the translation of all further sentences depends…. There can be no doubt that rival systems of analytical hypotheses can fit the totality of speech behavior to perfection, and can fit the totality of dispositions to speech

> behavior as well, and still specify mutually incompatible translations of countless sentences insusceptible of independent control. (1960, 72)

But Quine is quick to reassure us that this does not mean that indeterminacy of translation is an *instance* of the underdetermination of our theory. A translation of another's sentences is not just a theory we have about the meaning of their sentences. This is in part because Quine refuses to accept that there are such *things* as meanings of individual sentences, for the familiar reasons stemming from combining confirmation holism and verificationism (1969a, 80–81).[45] Translation is not a new theory using a new set of theoretical terms. It is simply the matching of one's own home theory with that of the foreigner, or as George Romanos puts it, a "way of reading this theory…into the language he is investigating" (1983, 181). While there is a fact of the matter according to which underdetermined theories are true or false, there is no fact of the matter according to which rival but incompatible manuals of translation are correct or incorrect (Gibson 1986, 151–2).[46] When we engage in translation, we presuppose our theory of physics—call it A—and hold it as true, even though a variety of incompatible theories (A, B, C, etc.) are empirically equivalent to it and equally underdetermined by the evidence. Presumably we have used pragmatic factors (such as simplicity) to choose our theory, but when it comes to reading this theory into the speech disposition of others, the same pragmatic factors do not dictate what we attribute to them. Simplicity may dictate assigning a *false* (by our lights) theory, B, to the foreigner, for example. There are other possibilities (Quine 1970, 180). But

[45] To the extent that the underdetermination thesis *itself* relies on confirmation holism, the indeterminacy of translation thesis is really a *triple* iteration of confirmation holism: one to say theory is underdetermined, another to say translation manuals are underdetermined, and a third to say that because there are no things that are the meanings of individual sentences, there is therefore no fact of the matter for translation manuals to concern themselves with. This makes one think confirmation holism is rather important to Quine's philosophy!

[46] Peijnenburg and Hünneman (2001, 20-4) have argued that Romanos' and Gibson's explanations for the difference between underdetermination and indeterminacy are themselves different. I don't see any grounds for this. On this view, Romanos argues for indeterminacy simply from the fact that there is a "double underdetermination" involved in translation. On its face, though, this explanation gives no reason for thinking that indeterminacy should follow from underdetermination. I think it only follows given the assumption that the second iteration of underdetermination has no fact of the matter to appeal to, i.e., Gibson's understanding. But I think Romanos is assuming this explanation implicitly, when he says that a translator is simply taking his own theory and trying to put it into new notation (184). This implies, I think, that there are no new facts beyond the "notation," and of course in radical translation the "notation" we are dealing with is the set of speech dispositions, etc.

these factors are *only* pragmatic. Because there is no reason to believing in the meanings of individual sentences, there is no reason to believe we are dealing with any new facts of the matter beyond observations and speech dispositions.

But even though indeterminacy is not an instance of the ordinary underdetermination of physical theory by evidence, this does not mean it is not an iteration of the same old underdetermination argument. It is particularly revealing, I think, that Quine suggests above that the candidates for translating our theory into the language of others are simply expressions of the rival theories we ourselves consider as being underdetermined by our evidence. In other words, if physical theory itself is not underdetermined, there would be no way of conceiving of alternate translation manuals, and translation would not be indeterminate. Thus Quine states, "What degree of indeterminacy of translation you must then recognize…will depend on the amount of empirical slack that you are willing to acknowledge in physics" (1970, 181). Quine then considers various degrees to which one might accept underdetermination in physics, with corresponding degrees of indeterminacy in translation. But by this logic, if we were not to recognize any underdetermination in physics, there would likewise be no indeterminacy of translation. This is a point that seems to hold even if we were to fail to specify a "fact of the matter" that translation would concern. Perhaps there would be no such fact, and translation would only be a matter of reading one's only possible theory into another's language. The best one could argue at this point would be a non-cognitivism about translation, not an "indeterminacy," on the grounds of Quine's argument combining verification and confirmation holism (if meaning is the method of confirmation, and only whole theories, not sentences are confirmed, then only whole theories, not individual sentences, have meanings).

So I think it is clear how Quine's "pressing from above" argument for indeterminacy relies on the underdetermination thesis. What of "pressing from below," the argument from the inscrutability of reference? This, of course, is the argument regarding the reference of individual terms, like "gavagai." Does "gavagai" refer to rabbits, or rabbit stages, or further bizarre slices of reality made possible by

proxy functions like "cosmic complements," etc.? Quine thinks that inscrutability could also help establish the indeterminacy of meaning/translation, but he doesn't seem to think it is as definite as "pressing from above." Indeed Quine thinks observation sentences may be translated objectively (or at least close to objectively), even though their terms, when divided, are inscrutable. He says that the point of the "gavagai" example "was aimed not at proof [of the indeterminacy thesis] but at helping the reader to reconcile the indeterminacy of translation imaginatively with the concrete reality of radical translation" (1970, 182), though it is not entirely clear what he means by this. He even suggests that an allegedly undebatable example of inscrutability of certain features of Japanese grammar does not imply any indeterminacy of translation (1970, 182).

Whatever Quine says about the link between inscrutability and indeterminacy, it is difficult to keep the issues of inscrutability and indeterminacy separate. To begin with, if what looks like an individual term is *itself* to be taken as a one-word sentence, then indeterminacy in regards to that single term would map onto indeterminacy in translating it as an entire sentence. Also, the question of whether to regard it as a term or a sentence itself renders the sentence indeterminate. Granted, the cases in which inscrutability does not seem to imply indeterminacy are those cases (the translation of observation sentences) which Quine originally believes to be objectively translatable. But in later days, Quine's commitment to the objective translatability of these sentences becomes less forthright. He begins to suggest that translators must rely on subjective-sounding "empathy" in order to ascribe observation sentences to others, no longer being able to rely on an objective knowledge of their stimulus meanings (Quine 1992; 1996). Observationality becomes a more graded notion, and for this reason the extent of indeterminacy spreads. For all these reasons, it seems at least worth exploring inscrutability as a source of indeterminacy, which is probably why Quine includes it in "On the Reasons for Indeterminacy of Translation," and says "pressing from below" consists of "whatever

arguments for indeterminacy of translation can be based on the inscrutability of terms" (1970, 183).[47] Finally, even if inscrutability does not imply indeterminacy, it is worth discussing here, because as I argued above, inscrutability is still an independent source of skepticism, and we are looking for the roots of Quine's rejection of traditional solutions to skepticism.

One recent commentator, Nimtz (2005, 4), summarizes the argument for inscrutability of reference as follows:

> (a) Semantic properties are exclusively determined by A facts.
> (b) For any language L, the totality of A facts is compatible with indefinitely many radically different interpretations.
> (IR) [Therefore,] [f]or any language L, there are indefinitely many radically different yet equally correct interpretations.

One may, of course, object to Quine's list of A facts as stemming from an impoverished behaviorism. But I think even more crucial to Quine's argument here is not his assumption about what counts as naturalistically acceptable, but his move from "*is compatible with indefinitely many interpretations…"* to "*there are indefinitely many …correct interpretations.*" The slide seems almost imperceptible, but makes sense if we assume that an interpretation's compatibility with the A-facts is sufficient to make the interpretation *correct*.

Leaping from mere compatibility with the evidence to equal correctness should remind us of the underdetermination thesis. The underdetermination thesis is also about equal "correctness," equal correctness with respect to evidence. According to this thesis, the evidence does not decide between empirically equivalent theories, because the theories are each compatible with the same evidence. As we shall see in chapter 6, this usually stems from the assumption that to for a theory to have the same logical consequences as another just is to have the same empirical support, to be equally "correct." Of course the inscrutability thesis is not a thesis about theories and their evidence, but about the reference of our terms. Nevertheless, the *argument* for this thesis implicitly relies upon a view about evidence,

[47] For further reasons to see a relation between inscrutability and indeterminacy, see Kirk (2000, 165), and Orenstein (1997). See also Quine (1960, 71-2).

because deciding that an interpretation about reference is "correct" is just the same as deciding if we have evidence for thinking that some reference scheme holds.

Consider, for example, how asking "is this the same rabbit as that?" would settle whether "rabbit" refers to "rabbit" or "rabbit stage." If the subject were able to give a determinate answer "yes" while pointing at a rabbity patch, we would know that his reference scheme extended beyond the immediate moment, referring to an enduring object, rather than just a rabbit stage. But Quine believes that there is no way for the subject to answer with a determinate "yes" or "no" to our question, because the question itself could translate as "is this the same rabbit?" or as "is this the same succession of rabbit stages?" I think the same style of argument is at work in arguments for inscrutability that stem from proxy functions: using these cases, we conclude that we may indirectly ostend many different types of objects, but the suggestion that there is no fact of the matter concerning which we indirectly ostend is still made by reference to the limited facts about what we directly ostend. As Quine puts it, "even the pointing out of an inscription is no final *evidence* that our talk is of expressions and not of Gödel numbers" (1969c, 44, emphasis mine).

So the argument for the inscrutability of reference thesis says that if we have a theory about the reference of another's terms, the evidence we might adduce for such a theory underdetermines any of the possible reference scheme attributions. So, Quine is not saying that to *have* a reference scheme is something that requires evidence, or that one's *possession* of a reference scheme is therefore underdetermined by evidence. The conclusion of his argument is just that there are no facts or evidence that determine such a scheme. But *his argument* for this claim is one that *does* rely on the use of evidence, even though in the end he would say that even the attribution of a reference scheme not a theory of our own.[48] What Quine is saying when he argues that "rabbit," "rabbit stage," etc. are

[48] It is not a theory of our own for probably the same reason that a translation manual is not thought to be a theory. "Reference" is not a theoretical term that refers to entities called "references," or at least he would say we have no evidence that such a term could be used to explain, because a fixed amount of evidence is consistent with a variety of divergent reference schemes.

compatible with facts about direct ostension of *gavagai*, is that *if* we take any of these as the reference scheme, *then* we will ostend identical stimuli and answer identical questions about reference. If we are referring to rabbits, then we will ostend the exact same stimuli and answer the exact same questions, as we would if we are referring to rabbit stages. Likewise when he argues that theory is underdetermined by evidence, his argument is that *if* theory A is true, *then* such and such empirical consequences will obtain, and *if* theory B is true, *then* the same empirical consequences will also obtain, so both theories are equally well-supported by the evidence and therefore underdetermined by it.

Indeterminacy of translation, inscrutability of reference, and underdetermination of theory by evidence each have apparently skeptical consequences when they are raised against traditional epistemology, as I have argued both here and in chapter 2. If my argument in this section is correct, and both indeterminacy and inscrutability reduce to the underdetermination-style theses, this suggests that it is the ultimate source of the skepticism. I don't think it should come as much of a surprise that underdetermination has apparently skeptical consequences, because some of the most prominent skeptical arguments in the history of philosophy have been underdetermination arguments. As Okasha (2003) observes, even Descartes' evil demon argument works by noting that our experience is compatible with a variety of empirically equivalent alternatives: that there really is a fire in front of us, that we are dreaming that there is a fire in front of us, that an evil demon has made us believe that there is a fire in front of us, etc. So, seeing Quine's negative theses as rooted in the underdetermination thesis helps us to see just how much affinity there is between naturalism and skepticism. Quine's pragmatism may allow him to explicitly disavow skepticism, by permitting him to rely on extra-empirical criteria to decide between empirically-underdetermined rivals, but to the traditional epistemologist, this just is a concession to skepticism.

Conclusion: Reciprocal containment revisited

Quine thinks epistemology is prompted by skeptical doubts. This essay was also prompted by a doubt, one concerning how Quine intends to respond to skepticism. Critics of Quine take him to be trying to refute it, but failing; defenders seem to agree he is trying to refute it, and believe he succeeds. I have argued that neither party completely appreciates Quine's total project. Quine is not trying to refute the skepticism that concerns the traditional epistemologist. He takes the skeptical problems generated by traditional epistemology as basically irresolvable on their own terms.

Naturalized epistemology is not a refutation of skepticism, but an accommodation to it. It is the attempt to explain the origin of our beliefs, even the origin of what we take to be true beliefs, under the assumption that these beliefs did not arise from anything like awareness or logical justification. This is the full meaning of Quine's idea that doubt is what prompts epistemology. The language of "prompting" is quite appropriate here, because we epistemologists, for Quine, are just like our behavioristic subjects. We too are prompted by stimuli in our environments. In this case, the stimulus is our own skeptical doubts. Doubt prompts naturalized epistemology, not because we wish to erase this doubt and find secure foundations for our knowledge (which are impossible), but because we take this doubt for granted and want to understand how we were ever able to erect an edifice of theory *in spite* of having no secure foundations. From Quine's perspective, it is an impressive feat our species has achieved—the erection of modern science on the basis of sloppy analogies, wistful symmetries, and various other doctrines "conceived in sin."

There is one last doubt prompting our further inquiry: if underdetermination is what prompts the project of naturalized epistemology, what are we to make of the fact that it is only under the assumption of naturalism that underdetermination can be taken seriously in the first place? Does this make Quine's overall project circular? This might make things easier for the critic of Quine, if the circle is vicious. But as Quine has repeatedly urged, an epistemology that rejects foundationalism does

not need to treat circularity as vicious. How, then, can critics break into the coherent circle of Quine's theory and undermine it, if there is no starting point to attack?

First we need to review the sense in which the underdetermination thesis itself has its origins in naturalism. We have already seen the dominant way: Quine's naturalist looks first to an empiricist psychology, which points to the exclusive informational relevance of stimuli on our sensory surfaces. The naturalist concludes that more than one scientific theory or reference scheme is compatible with the sparse stimuli of the sensory surfaces. This is not the entire story, of course. A scientific story must be told about how sensory stimuli coalesce with social reinforcement in order to give rise to observation sentences. Another scientific story must be told about how observation sentences relate to theory, via Quine's so-called "observation categoricals." This last is Quine's particular version of the hypothetico-deductivist view of confirmation, a view which he takes to be grounded not primarily in the content of science, but in the facts of scientific practice. And as we shall see in the next chapter, it must be taken for granted that hypothetico-deductive connections are the exclusive links between theory and observational evidence.

Each of these naturalistic sources of the underdetermination thesis represents an aspect of the containment of epistemology in natural science. So there is a sense in which the underdetermination thesis already presupposes naturalism. Quine is quite content with this, however, because of his doctrine of "reciprocal containment": he also takes natural science itself to be contained in epistemology. It is our empiricist epistemology that tells us to look to science as our highest source of knowledge in the first place (and it tells us that, of course, because it in turn is contained in a natural science that tells us that our only contact with the world is through our sensory surfaces). So it is Quine's doctrine of reciprocal containment that best represents his anti-foundationalism. Neither science nor epistemology is a "starting point" for the other. Each is viewed as an instance of the other depending upon perspective.

But the fact of reciprocal containment does not imply that naturalized epistemology is enclosed by an impenetrable circle. We can still ask questions about the specific content of the science assumed by the naturalized epistemologist and about the specific commitments of empiricist epistemology. We might find that even if we, as naturalists, take all of our science seriously, none of it implies the underdetermination thesis or requires that we become naturalized *epistemologists*. And we might find that even if our only contact with the world is through our senses, this is fully consistent with a traditional approach to epistemology.

There are several ways to challenge the content of Quine's science, in ways that show that real science does not contain his particular version of naturalized epistemology. We will do much of this in the next chapter, primarily by challenging the hypothetico-deductivist account of confirmation by reference to historical scientific practice. We will also challenge the traditional empiricist accounts of sensation and concept-formation which impoverish theories of confirmation, eliminating the possibility of inductivist foundationalism. Apart from this, there are many other ways in which one could challenge the psychology of language-learning behind Quine's account, by challenging his "holophrastic" account of observation sentences (Bloom 1973), and his social-linguistic behaviorism (Nelson 1988; Bloom 1993; Modee 2000). Going further, one could even challenge Quine's central dogma that extensional language is the exclusive language of science (Hookway 1988).

As we shall see, this angle of attack on Quine's system resembles the strategy of defusing the skeptic's *reductio,* which Stroud and others mistakenly attribute to Quine himself. If arguments for skepticism are of the *reductio ad absurdum* form, then by showing that a wider body of scientific evidence does not contain absurdities like the underdetermination thesis, we acknowledge the importance of science without showing that it implies the need to abandon traditional epistemology. In short, it may well be that science tells us that our only source of information is through the senses, but this does not imply that science obligates us to be *naturalized* empiricist epistemologists. What it *means* for the mind to be in contact with the world by the senses is a matter of some controversy, one

that probably requires some philosophic interpretation. Contact with the world through the senses may not be as impoverished and conducive to skepticism as naturalized epistemologists might think.

So even if there is a sense in which natural science contains epistemology, the points above suggest it may not contain *Quine's* epistemology. We can also qualify the converse containment, of science in epistemology, in a way that calls the total package of naturalized epistemology into question. It may well be that we celebrate the empirical method of science by reference to prior empiricist epistemology (which is, in turn, endorsed by science). But there are also significant differences of opinion over *how* science should be celebrated. It may well be that, if empiricism tells us that it is only through the senses that we acquire information about the world, then we should recognize science as the *highest* form of knowledge. It is, after all, the most systematic evidence-driven discipline. But recognizing science as the highest form of knowledge does not imply recognizing it as the only form, or even as the only *empirical* form. There may also be commonsense empirical knowledge on which science is built. In fact I will argue in the next chapter that to the extent that science undermines the crude empiricism of the naturalized epistemologists, it also makes room for the epistemological relevance of first-person pre-scientific experience. Once again, that leaves room for more traditional approaches to epistemology, which, if not *a priorist* are at least not naturalized.

The last point, about the possibility of pre-scientific knowledge, would call into question that central doctrine of naturalized epistemology, that science was "conceived in sin," and is therefore in need of some non-logical, psychological explanation. To show that this "original sin" approach to knowledge may in fact be a myth, however, it is our obligation to examine the science that demonstrates this. As I have argued, there are several ways to show that science does not contain the kind of epistemology Quine champions. In the next chapter, I will focus on just a few of these issues. To assess the underdetermination thesis on which both inscrutability and indeterminacy rest, I will challenge both the hypothetico-deductivist and anti-inductivist views that many empiricists before

Quine took for granted. If these challenges hold water, we will see that it is Quine's acquiescence in skepticism which is truly the "overreaction."