CHAPTER 13

THREE FORMS OF NATURALISM

PENELOPE MADDY

MANY philosophers—from Hume[1] to the pre-Fregean German materialists,[2] from Reichenbach to Arthur Fine[3]—have been classified as "naturalists," in some sense or other of that elastic term, but the version most influential in contemporary philosophy of logic and mathematics undoubtedly comes to us from Quine. For him, naturalism is characterized as "the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described" (1981a, p. 21). Agreeing wholeheartedly with this sentiment and the spirit behind it, some post-Quinean naturalists, including John Burgess[4] and myself, occasionally find ourselves uncomfortably at odds with particular doctrines Quine

My thanks to John Burgess for all he has taught me over the years, as well as his thoughts on earlier drafts of this chapter. I am also grateful to Patricia Marino for helpful comments and discussions.

[1] Mounce (1999) and Stroud (1977) seem to attribute different forms of "naturalism" to Hume.

[2] See Sluga (1980), pp. 17–34. As Sluga notes (1980, p. 178), Stroud (1977, p. 222) sees these "scientific materialists," rather than the logical positivists, as the true descendants of Hume.

[3] I discuss these last two versions of "naturalism" in (2001a).

[4] As the central texts of Burgess's naturalism, I'll be using Burgess (1983, 1990, 1998) and Burgess and Rosen (1997). With apologies to Professor Rosen for downplaying his contributions, I will treat the "naturalism" of the co-authored work as an elaboration of the position in Burgess's earlier papers.

develops in his pursuit of philosophy naturalized, doctrines that seem to us less than completely true to his admirable naturalistic principles. Yet Burgess and I sometimes disagree on just which doctrines those are and on how to go about correcting the situation!

My plan here is to sketch the outlines of the Quinean point of departure, then to describe how Burgess and I differ from this, and from each other, especially on logic and mathematics. Though my discussion will touch on the work of only these three among the many recent "naturalists," the moral of the story must be that "naturalism," even restricted to its Quinean and post-Quinean incarnations, is a more complex position, with more subtle variants, than is sometimes supposed.[5]

1. Quinean roots

When Quine describes his naturalism as the "abandonment of the goal of a first philosophy" (1975, p. 72), he alludes to Descartes, who viewed his *Meditations on First Philosophy* as the only hope for "establish[ing] anything at all in the sciences that [is] stable and likely to last" (1641, p. 12). His approach, of course, was to doubt everything, including all of science and common sense, in order to uncover prescientific, first philosophical certainties that would then underpin our knowledge.[6] Few would suggest, at this late date, that Descartes succeeded in this, but Quine goes further, rejecting the project itself:

> I am of that large minority or small majority who repudiate the Cartesian dream of a foundation for scientific certainty firmer than scientific method itself. (Quine 1990, p. 19)

The simple idea is that no extrascientific method of justification could be more convincing than the methods of science, the best means we have.

The Quinean naturalist, then, "begins his reasoning within the inherited world theory as a going concern" (Quine 1975, p. 72). Alongside the familiar pursuits of physics, botany, biology, and astronomy, the naturalist asks how it is that human beings, as described by physiology, psychology, linguistics, and the rest, come to reliable knowledge of the world, as described by physics, chemistry,

[5] Obviously, I won't do justice to the details and subtleties of the three positions in this chapter. The interested reader is urged to consult the references for more careful and nuanced discussions.

[6] Broughton (2002) gives a fascinating and wonderfully readable account of how Descartes took his first philosophical method to work.

geology, and so on.[7] This is the task of epistemology naturalized, "the question how we human animals can have managed to arrive at science" (Quine 1975, p. 72). Ontology is also naturalized:

> Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense... the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or the physical part, are not different in kind from the considerations which determine a reasonable construction of the whole. (Quine 1948, pp. 16–17)
>
> Ontological questions... are on a par with questions of natural science. (Quine 1951, p. 45)

Insofar as traditional philosophical questions survive in the naturalistic context, they are undertaken "from the point of view of our own science, which is the only point of view I can offer" (Quine 1981c, p. 181).

One portion of this naturalistic undertaking will be a scientific study of science itself. Obviously, this intrascientific inquiry can deliver no higher degree of certainty than that of science. As Quine remarks, "Repudiation of the Cartesian dream is no minor deviation" (1990, p. 19). "Unlike the old epistemologists, we [naturalists] seek no firmer basis for science than science itself" (Quine 1995, p. 16). The naturalist

> sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. (Quine 1975, 72)

Science, on this picture, is open to neither criticism nor support from the outside.

But this leaves ample room for both vigorous criticism and rigorous support of particular scientific methods: "A normative domain within epistemology survives the conversion to naturalism, contrary to widespread belief..." (Quine 1995, p. 49).[8] What's changed is that the normative scrutiny comes not from an extrascientific perspective, but from within science: "Our speculations about the world remain subject to norms and caveats, but these issue from science itself as we acquire it" (Quine 1981c, p. 181). Here Quine returns to a favorite image:

> Neurath has likened science to a boat which, if we are to rebuild it, we must rebuild plank by plank while staying afloat in it. The philosopher and the scientist are in the same boat. (Quine 1960, p. 3)

[7] I depart here slightly from Quine (1969), where epistemology naturalized is said to take place inside psychology, but he gives more inclusive characterizations later (see II.1 below), so I take this as a friendly amendment.

[8] Cf.: "They are wrong in protesting that the normative element, so characteristic of epistemology, goes by the board" (Quine 1990, p. 19).

> He [the naturalist] tries to improve, clarify and understand the system [science] from within. He is the busy sailor adrift on Neurath's boat. (Quine 1975, p. 72)

This process is familiar: norms of confirmation and theory construction often arise in scientific practice, from simple canons of observation through elaborate guidelines for experimental design to highly developed maxims like mechanism.[9] As science progresses, these are put to the test, sometimes successfully and sometime not, and in this way their claim to a role in shaping future science is correspondingly strengthened or undermined. As Quine remarks, "We were once more chary of action at a distance than we have been since Sir Isaac Newton" (1981c, p. 181).

When Quine begins his naturalistic scientific study of science, he is struck by a simple but important observation of Duhem:

> The physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed. (Duhem 1906, p. 187)

This phenomenon undermines the picture of a single scientific claim enjoying "empirical content" by itself, and leads Quine to holism and his famous "web of belief":

> Our statements about the external world face the tribunal of sense experience not individually but only as a corporate body.... The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics... is a man-made fabric which impinges on experience only along the edges.... (Quine 1951, pp. 41–42)

Somewhat later, Quine tempers this holism to something more "moderate":

> It is an uninteresting legalism... to think of our scientific system of the world as involved *en bloc* in every prediction. More modest chunks suffice... (Quine 1975, p. 71)

But the moral—that particular scientific theories are tested and confirmed as wholes—remains intact.

Faced with a failed prediction, then, Quine notes that, strictly speaking:

> Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system.... Conversely, by the same token, no statement is immune to revision. (Quine 1951, p. 43)

[9] I discuss the rise and fall of mechanism in (1997, pp. 111–116). This normative element is also present, for example, when a physiological, psychological theory of perception indicates why perceptual beliefs are largely reliable, and therefore reasonable, under certain conditions and largely unreliable, and therefore unreasonable, under others.

Practically speaking, we are guided by the "maxim of minimum mutilation" (Quine 1990, p. 14), "our natural tendency to disturb the total system as little as possible" (Quine 1951, p. 44), so we quite properly prefer to alter simple statements about observable physical objects—deciding that the swami only seems to levitate—rather than highly general laws (e.g., the law of gravity) if this is at all possible. In the image of the web, altering a statement closer to the experiential edges causes less widespread disturbance than revising a centrally located generality.

Granting that confirmation accrues holistically to scientific theories, on what sort of evidence is this confirmation based? On what grounds, for example, do we adopt atomic theory? Quine addresses this question as he continues his pursuit of a "scientific understanding of the scientific enterprise" (1955, p. 253).

> The benefits... credited to the molecular doctrine may be divided into five. One is simplicity.... Another is familiarity of principle.... A third is scope.... A fourth is fecundity.... The fifth goes without saying: such testable consequences of the theory as have been tested have turned out well, aside from such sparse exceptions as may in good conscience be chalked up to unexplained interferences. (Quine 1955, p. 247)

In another place ((1970), ch. V), Quine and Ullian give a slightly different list of theoretical virtues—conservatism, generality, simplicity, refutability, modesty, plus conformity with observation—and elsewhere (1990, p. 95), Quine lists economy and naturalness as examples, but the general flavor is the same throughout. Finally, as Quine notes, the various virtues can conflict; they must be balanced off against one another in particular cases.

Quine acknowledges that such a defense of atomic theory is indirect, and he considers the possibility that

> the benefits conferred by the molecular doctrine give the physicist good reason to prize it, but afford no evidence of its truth.... Might the molecular doctrine not be ever so useful in organizing and extending our knowledge of the behavior of observable things, and yet be factually false? (Quine 1955, p. 248)

Quine begins his response by pushing this skeptical line of thought even further, calling into question the tendency to "belittle molecules... leaving common-sense bodies supreme":

> What are given in sensation are variformed and varicolored visual patches, varitextured and varitemperatured tactual feels, and an assortment of tones, tastes, smells and other odds and ends; desks [and other common-sense bodies] are no more to be found among these data than molecules. (Quine 1955, p. 250)

This line of thought tempts us to conclude that

> In whatever sense the molecules in my desk are unreal and a figment of the imagination of the scientist, in that sense the desk itself is unreal and a figment of the imagination of the race. (Quine 1955, p. 250)

The upshot would be that only sense data are real, but this conclusion

> is a perverse one, for it ascribes full reality only to a domain of objects for which there is no autonomous system of discourse at all. . . . Not only is the conclusion bizarre; it vitiates the very considerations that lead to it. (Quine 1955, pp. 254, 251)

We can hardly see ourselves as positing objects to explain our pure sense data when those sense data can't even be described without reference to objects.

All this, Quine counts as a *reductio*: "Something went wrong with our standard of reality" (1955, p. 251). To correct the situation, he urges that we turn this tendency of thought on its head:

> We became doubtful of the reality of molecules because the physicist's statement that there are molecules took on the aspect of a mere technical convenience in smoothing the laws of physics. Next we noted that common-sense bodies are epistemically much on a par with the molecules, and inferred the unreality of common-sense objects themselves. (Quine 1955, p. 251)

But surely "the familiar objects around us" are real if anything is; "it smacks of a contradiction in terms to conclude otherwise." So,

> Having noted that man has no evidence for the existence of bodies beyond the fact that their assumption helps him organize experience, we should have done well, instead of disclaiming the evidence for the existence of bodies to conclude: such, then, at bottom, is what evidence is, both for ordinary bodies and for molecules. (Quine 1955, p. 251)

This, then, is Quine's conclusion: the enjoyment of the theoretical virtues is, at bottom, what supports all our knowledge of the world.

With this quick summary of Quine's views on science and the scientific study of science as backdrop, we can turn to his naturalist's position on logic and mathematics. He sees our knowledge of both as part of our web of belief, part of our best scientific theorizing about the world, confirmed with the rest by cooperative enjoyment of the theoretical virtues:

> A self-contained theory which we can check with experience includes, in point of fact, not only its various theoretical hypotheses of so-called natural science but also such portions of logic and mathematics as it makes use of. (Quine 1954, p. 121)

Confining our attention to logic for the moment, this means, for example, that the pursuit of the theoretical virtues might one day lead us to revise one or another of our current laws:

> Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (Quine 1951, p. 43)

The great weight of the maxim of minimum mutilation would stand against such a move, and Quine remarks, skeptically, that "the price is perhaps not quite prohibitive, but the returns had better be good" (1970, p. 86; 1986, p. 86).[10]

Readers familiar with this classical Quinean position on the revisability of logic are sometimes puzzled by later remarks to the effect that a deviant logician cannot disagree with the classical logician because his embrace of different logical laws shows that he actually means something different by the logical connectives. As Quine puts it: "Here, evidently, is the deviant logician's predicament: when he tries to deny the doctrine he only changes the subject" (1970, p. 81; 1986, p. 81). This is less jarring than it might seem, given that a change to new connectives can apparently be motivated by the same scientific reasons that were first imagined as motivating a change of logical laws:

> By the reasoning of a couple of pages back, [the deviant logician] changes the subject. This is not to say that he is wrong in doing so.... he may have his reasons. (Quine 1970, p.83; 1986, p. 83)

It is undoubtedly odd to hear Quine distinguishing change of meaning from change of theory,[11] but the central thesis of the revisability of logic on empirical grounds remains untouched.

However, a real departure on the revisability of logic can be found in a late discussion of the holism. Here Quine returns to the scientist facing a falsified prediction:

> We have before us some set *S* of purported truths that was found jointly to imply the false [prediction].... Now some one or more of the sentences in *S* are going to have to be rescinded. We exempt some members of *S* from this threat by determining that the fateful implication still holds without their help. Any purely logical truth is thus exempted.... (Quine 1990, p. 14)

This qualification of the revisability doctrine to rule out revision of logic is perhaps not unwelcome: one common objection to the original Quinean web has been that some laws of logic are needed for the simple manipulations of web maintenance[12] (the law of noncontradiction, for example, is what tells us we have

[10] In his (1981b), Quine discusses the costs of retaining the law of the excluded middle, but he reports, "My inclination is to adhere to it for the simplicity of theory it affords" (p. 32). Other proposed deviations from classical logic meet with even less enthusiasm.

[11] Cf. Quine (1951, pp. 36–37): "It is obvious that truth in general depends on both language and extra-linguistic fact.... Thus... it... seems reasonable that in some statements the factual component should be null; and these are the analytic statements. But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith."

[12] For example, see Wright (1986) or Shapiro (2000).

to change something when we reach a falsified prediction[13]), so it is hard to see how these laws could be revised without crippling the scientific enterprise. But we are left with no replacement for the holistic justification of the assumption that our logic (with our meanings), as opposed to some deviant logic (with deviant meanings), is more suitable for our scientific theorizing about the world.

Finally, mathematics. First, we observe that our scientists typically make use of mathematics in their theorizing. This might be a mere manner of speaking—like saying "patience is a virtue" to mean that a patient person is to that extent virtuous—but strenuous efforts to reconstrue science in a mathematics-free idiom, from Quine and Goodman to Field, have all failed.[14] Second, as naturalized metaphysicians, we take science to be our best guide to what there is and how it operates. Third, as holists, we take a scientific theory to be confirmed as a whole, the mathematical along with the physical hypotheses.[15] To conclude, we apply Quine's criterion of ontological commitment (Quine 1948), which takes science to establish the existence of precisely those things that appear in its existential claims. Thus we arrive at Quine's mathematical realism by means of his "indispensability argument." Notice that the evidence for mathematical objects is the same as for molecules and for common-sense objects—participation in a theory with the theoretical virtues—and recall that "such ... at bottom ... is what evidence is" (Quine 1955, p. 251).

This famous argument only supports the existence of those mathematical entities that appear in our best scientific theory. But there is more to mathematics than this, as Quine recognizes:

> A word finally about the higher reaches of set theory itself and kindred domains where there is no thought or hope of applying in natural science. When

[13] Perhaps Quine has this case in mind when he writes: "On learning 'not' and 'and,' the child already internalizes a bit of logic; for to affirm a compound of the form 'p and not p' is just to have mislearned one or both particles" (1995, p. 23). But, of course, there are those who defend dialetheism (see Priest and Tanaka 2002).

[14] See Goodman and Quine (1947), Field (1980, 1989). This claim remains debatable, of course.

[15] In the late discussion of holism that "exempts" logic, mathematics seems to retain its original status: the maxim of minimum mutilation "constrains us ... to safeguard any purely mathematical truth; for mathematics infiltrates all branches of our system of the world, and its disruption would reverberate intolerably.... Simplicity of the resulting theory is another guiding consideration, however, and if the scientist sees his way to a big gain in simplicity he is even prepared to rock the boat very considerably for the sake of it" (Quine 1990, p. 15). And in (1995), he stresses "the difference between logic, narrowly construed, and the rest of mathematics.... However, I am inclined to lighten somewhat the emphatic contrast usually drawn between mathematics and natural science. I already equated the roles of mathematical laws and laws of nature in implying [empirical predictions]" (pp. 52–53).

> I likened mathematical truths to empirical ones ... I was disregarding these mathematical flights.... how should we view them? (Quine 1995, p. 56)

At one point, they're dismissed as meaningless:

> So much of mathematics as is wanted for use in empirical science is for me on a par with the rest of science. Transfinite ramifications are on the same footing insofar as they come of a simplificatory rounding out, but anything further is on a par rather with uninterpreted systems. (Quine 1984, p. 788)

Later on, Quine grudgingly relents

> What of the higher reaches of set theory? We see them as meaningful because they are couched in the same grammar and vocabulary that generate the applied parts of mathematics. We are just sparing ourselves the unnatural gerrymandering of grammar that would be needed to exclude them. (Quine 1990, p. 94)

Having allowed them meaning, he also allows truth-value, but given their complete isolation from the data of experience, no evidence is available either way.

In the absence of holistic confirmation or disconfirmation from experience, Quine proposes we proceed simply by applying the remaining theoretical virtues. In particular, he suggests, simplicity or economy supports Gödel's axiom of constructibility, $V = L$, and opposes large cardinal axioms. This choice "inactivates the more gratuitous flights of higher set theory" (Quine 1990, p. 95), Quine remarks with approval. He insists this approach is "no threat to the starry-eyed set theorist for whom the sky is the limit," because the set theorist's statements are still meaningful and his theory of large cardinals "still makes proof-theoretic sense" (Quine 1995, p. 56). But this concession can't mask the fact that Quine's preference for $V = L$ contradicts the near-unanimous opinion of practicing set theorists.

II. Two Post-Quineans

Given this sketch of Quine's naturalism and its consequences, as he sees them, for logic and mathematics, let's turn to the views of our two post-Quinean naturalists, to illuminate both their departures from Quine himself and their disagreements with each other. To bring some order to this three-ringed circus, I'll break down this exercise in compare-and-contrast under a series of headings. Let's begin by considering the "science" in which "reality is to be identified and described."

II.1. Science

Quine plainly acknowledges that "I use 'science' broadly," including not only the "hard sciences" but also "softer sciences, from psychology and economics through sociology to history" (1995, p. 49). All these presumably display the markers of "observation and the hypothetico-deductive method" (1975, p. 72) and some attention to the theoretical virtues. And, conversely, any undertaking that shares these markers is likewise science, regardless of whether or not it falls squarely within some established branch. For Quine, the scientific study of science, a part of naturalized epistemology, is of this sort:

> The inquiry proceeds in disregard of disciplinary boundaries but with respect for the disciplines themselves and appetite for their input. (Quine 1995, p. 16)

We theorize about science, using the results and methods of science itself.

Burgess's "science" seems in some ways narrower than Quine's; for example, he speaks of the "scientific community... whether understood narrowly, as including only specialist professionals, or broadly, as including also informed laypeople" (Burgess 1990, p. 5). Even the broad sense here seems limited to the established scientific disciplines, each a specialty unto itself, which may leave us wondering where the naturalist's scientific study of science is to find a home. And the semblance of a serious departure from Quine is encouraged by Burgess's insistence that the naturalist's project must be purely descriptive: "It seems that prescriptive methodology could not be a branch of science, though descriptive methodology is" (Burgess 1990, p. 6).[16]

In tone, at least, this tune diverges from Quine's. "The busy sailor adrift on Neurath's boat" is out to "improve, clarify and understand the system from within," not simply to describe the behaviors of the tribe of credentialed scientists. For Quine, the naturalistic study of science examines the methods of science with an eye to understanding how and why they are effective. Shoulder to shoulder with scientists, the naturalist strives to appreciate the reasons behind their design of experiments, their evaluation of evidence, and their preference for one theoretical elaboration over another; the ideal practicing scientist should be prepared to explain these things in terms of the general canons of scientific inquiry they both share. If the naturalist is a member of the scientific community, she should have the same grounds for scientific ratification and critique of scientific methods as are available to her fellow scientists. Science is a self-corrective process in which the naturalist participates.

At least on the score of normativity, I suspect that the semblance of disagreement here is largely illusory. Burgess (writing in collaboration with Gideon

[16] See also Burgess and Rosen (1997, pp. 208–209).

Rosen) clearly holds that the naturalist is a "citizen of the scientific community" (Burgess and Rosen (1997), p. 33).[17] And there is this important passage:

> science is not a closed guild with rigid criteria of membership. Philosophers professing naturalism often do contribute to debates in semantical theory or cognitive studies or other topics in the domain of linguistics or psychology, even though they are not officially affiliated with a university department in either of those fields. In principle nothing would bar such philosophers from participating in discussions on topics in the domain of chemistry or geology, though in practice they seldom do. The naturalists' commitment is at most to the comparatively modest proposition that when science speaks with a firm and unified voice, the philosopher is either obliged to accept its conclusions or *to offer what are recognizably scientific reasons for resisting them.* (Burgess and Rosen 1997, p. 65; added)

This leaves room for Quine's naturalistic justification and critique of scientific methods, for normative, prescriptive stands, since his "busy sailor" was never tempted to offer anything other than "recognizably scientific" grounds. Thus, it seems that Burgess's insistence that the naturalist's task is purely descriptive doesn't contradict Quine's insistence that normativity survives the move to naturalism because Quine is endorsing evaluations internal to science and Burgess is rejecting evaluations external to science.

But perhaps one more subtle difference remains. In Burgess's phrasing, the philosopher "should become a citizen of the scientific community... should become naturalized" (Burgess 1990, p. 5). In contrast, Quine's "busy sailor" would seem to be a native, not someone in need of conversion. Perhaps this is merely stylistic, but for my part, I much prefer the latter formulation. My naturalist "begins his reasoning within the inherited world theory as a going concern" (Quine 1975, p. 72). Such a naturalist, asked why she believes in, say, atoms, will react as an ordinary scientist, citing the usual scientific evidence (more on this below). Another sort of naturalist, the sort who started as a first philosopher and subsequently became a naturalized citizen of science, might be tempted to reply to the same question by citing the fact that her fellow scientists so believe and that she now believes as her fellow citizens do (i.e., "science says there are atoms and I, the naturalist, believe the utterances of science"). I have no reason to think that the Quinean or Burgessite naturalist would give the second answer, but my naturalist would certainly give the first.[18]

Where our three naturalists' views of "science" clearly diverge is on the status of mathematics. As we've seen, Quine includes mathematics as scientific, but only insofar as it takes part in empirical science, plus a bit more for "simplificatory roundings out" and a bit more again to avoid "unnatural gerrymandering of

[17] Burgess and Rosen are speaking of Quine here, but they voice no dissent of their own. See also Burgess (1990, p. 5).

[18] See Maddy (2001a) or (2002), I.

grammar." But evidence, for Quine, is always empirical evidence; proper methods are always those of natural science. Even in the grudgingly admitted higher reaches of set theory, it is the familiar theoretical virtues common to empirical theories that carry the day.

My naturalist sees things differently.[19] She begins, as Quine's does, within empirical science, and eventually turns, as Quine's does, to the scientific study of that science. She is struck by two phenomena: first, most of her best theories involve at least some mathematics, and many of her most prized and effective theories can only be stated in highly mathematical language; second, mathematics, as a practice, uses methods different from those she's turned up in her study of empirical science. She could, like the Quinean, ignore those distinctive methods and hold mathematics to the same standards as natural science, but this seems to her misguided. The methods responsible for the existence of the mathematics she now sees before her are distinctively mathematical methods; she feels her responsibility is to examine, understand, and evaluate those methods on their own terms; to investigate how the resulting mathematics does (and doesn't) work in its empirical applications; and to understand how and why it is that a body of statements generated in this way can (and can't) be applied as they are.

Burgess (with Rosen) goes further in his disagreement with Quine, sharply criticizing any naturalism according to which

> the mathematical sciences—so often considered by non-philosophers the very model of a progressive and brilliantly successful cognitive endeavor—must somehow be expelled from the circle of "sciences." (Burgess and Rosen 1997, p. 211)

To do this is to "mak[e] invidious distinctions... marginaliz[e] some sciences (the mathematical) and privileg[e] others (the empirical)" (Burgess and Rosen 1997, p. 211). Here it must be admitted that both Quine and I are guilty of so privileging natural science and of giving special attention to mathematics only because of its role in science.

But I do not follow Quine in the final move decried by Burgess (with Rosen), the one taken by those who "simply discard whatever of pure mathematics has not yet found application in the empirical sciences" (Burgess and Rosen 1997, p. 211). My naturalist, unlike Quine's, does not hold that those parts of mathematics that have been used in applications should be treated differently from the rest. She notes that branches of mathematics once thought to be far removed from applications have gone on to enjoy central roles in science[20] and, perhaps more important, that the methods that have led to the impressive practice she now observes, the practice so liberally applied in our current science, are the actual methods of mathematics, not the methods of natural science (as the Quinean naturalist would have it) nor

[19] See Maddy (1997, pp. 183–184).
[20] See Maddy (2001b).

some artificially gerrymandered subset of mathematical methods (as exclusive attention to the methods of applied mathematics, as distinct from pure mathematics, would require). She concludes that the entire practice of mathematics should be taken seriously, a practice including both applied and unapplied portions of contemporary pure mathematics, intricately intertwined.[21]

One more question arises for naturalists like Burgess and myself who venture beyond Quinean naturalism. Addressed to my version of naturalism, it takes this form: If the naturalist, engaged in her scientific study of science, discovers that one practice of human beings (namely, mathematics) is carried out using methods different from those of her natural science, why should she view this mathematical practice as different in kind from other practices with methods of their own, like astrology or theology? Addressed to Burgess, this becomes: Why should mathematics, but not astrology or theology, figure in the list of sciences?

My answer to this question, suggested above, has been that mathematics is used in science, so the naturalist's scientific study of science must include an account of how its methods work and how the theories so generated manage to contribute as they do to scientific knowledge. Astrology and theology are not used in science—indeed, in some versions they contradict science—so the naturalist needs only to approach them sociologically or psychologically.[22] Perhaps Burgess intends a similar answer to the analogous question for his view, for he writes (with Rosen):

> You cannot simply dismiss mathematics as if it were mythology on a par with the teachings of Mme Blavatsky or Dr. Velikovsky. A geologist interested in earthquake prediction or oil prospecting had better steer clear of Blavatsky's tales about the sinking of lost continents and Velikovsky's lore about the deposition of hydrocarbons by passing comets; but no philosopher will urge that the geologist should also renounce plate tectonics, on the grounds that it involves mythological entities like numbers and functions. (Burgess and Rosen 1997, p. 5)

Presumably, Burgess would also join my naturalist in holding that mathematics as a whole, not just its applied portion, is so-separated from Blavatsky tales and Velikowsky lore, perhaps for reasons not unlike those my naturalist gave a moment ago.[23]

[21] It seems to me that the two can't be separated without serious distortion. Tappenden (2001, p. 497) quite reasonably proposes that this claim be tested by a detailed study of the actual interactions between the various branches of mathematics.

[22] See Maddy (1997, pp. 203–205). Tappenden (2001, pp. 496–497) gives a fair outline of the debate on this score between my naturalist and her critics (in particular, Hale, Dieterle, Rosen, and Tennant; see Tappenden for references).

[23] Burgess rightly emphasizes that the "scientific" uses of mathematics include those of common sense and everyday belief, as in "stock indices, precipitation probabilities and batting averages" (personal communication, quoted with permission). See Burgess (1983, p. 94) on the slippery slope.

In sum, then, "science" for Quine and for me is natural science, while for Burgess it is a variety of natural and mathematical sciences,[24] but Quine and I differ on the status mathematics earns for itself in the course of our scientific study of natural science. I'll examine the differing results of this study for our three naturalists in more detail below, but first a brief look at logic.

II.2. Logic

The status of logic is not often the focus of Burgess's discussions, but passing comments (with Rosen) suggest that he would adopt something like the second Quinean position sketched above: "logical and analytic knowledge... is ultimately knowledge of language" (Burgess and Rosen 1997, p. 42). It isn't clear whether he would then follow the Quine who continues to count logic as empirically revisable or the Quine who exempts logic from holistic jeopardy (or takes some other stance entirely). As indicated above, I think both Quinean moves have their downsides.

My own rather speculative suggestion (see Maddy 2002) has been that humans are so constructed as to conceptualize the world in terms of some simple fundamental categories (e.g., as comprised of individual objects standing in various relations); that the world, to a large extent, is properly described as so structured (up to the point of quantum mechanics, at least); and that a rudimentary logic is implicit in these shared structures (e.g., fair versions of "or," "and," cruder counterparts to "not" and "if/then," and simple quantifications). This much logic, then, is obvious to us, as part of our most basic conceptual machinery, true of the world (for the most part), and, furthermore, can be known by us when we verify that the simple structures required for its support are present in the situations to which logic is applied. Beyond this rudimentary basis, we add idealizations of various sorts—bivalence, the truth-functional conditional, assumptions about our domains of quantification—whose wisdom must be judged as the wisdom of any scientific idealization is judged: by their appropriateness in particular contexts. Most cases for deviant logics can be seen largely as arguments that the relevant idealization is not advisable for one reason or another.

This brief sketch may well be too quick to be decipherable; I commend the interested reader to the longer sketch (Maddy 2002), and (I hope) to more detailed future elaborations. For present purposes, one point is worth noting: simple arithmetical claims like "$2 + 2 = 4$" can be expected to correspond to logical truths of the most rudimentary sort, and thus to robust truths about the world.

[24] Burgess writes, "I believe in the community, but not the unity, of science" (personal communication, quoted with permission).

II.3. The Scientific Study of Science

We've seen that Burgess disagrees with Quine on the scope of "science" and that I disagree on what the scientific study of science tells us about mathematics, but both Burgess and I also depart from Quine on fundamental aspects of the scientific analysis of natural scientific method. This in turn impinges on how we understand the notion of "best current scientific theory," the central notion of our naturalized metaphysics and epistemology.

Recall that Quine's analysis of the method of natural science comes down to holism and the theoretical virtues. Burgess's critique focuses on the virtue of economy, in particular the preference for theories that posit fewer things. According to Quine, this virtue implies that if we could do science without mathematical objects (and without seriously compromising other theoretical virtues), we should do so, for this would rid our theory of a vast ontology of abstracta, yielding a more economical, and thus better, scientific theory. Thus it is only the failure of his attempt to reconstruct science without abstracta that leads Quine to his indispensability argument for the existence of mathematical entities.

In contrast, Burgess doubts that scientific standards actually include a preference for theories with smaller ontologies of abstracta.[25] Most often, such economy is "a matter to which most working scientists attach no importance whatsoever" (Burgess 1983, p. 98). In most cases, "proposed changes in the mathematical apparatus of physics that have received ultimate acceptance have increased its power and freedom" (Burgess 1990, p. 11). On occasions when scientists have hesitated over adding new mathematical ontology (e.g., the introduction of analytic methods into geometry or of infinitesimals in the calculus), he argues that "decrease in rigor and/or a danger of inconsistency," not the new ontology, was the cause of concern, and points out that "rigor and consistency are already usually conceded... to be weighty scientific standards" (1990, pp. 11–12). Thus Burgess (with Rosen) proposes a modification of the list of theoretical virtues to reflect, among other things, a more limited version of economy (Burgess and Rosen 1997, p. 209).

On the main point here I completely agree: scientists feel free to adopt any mathematical apparatus that is convenient and effective, without concern for its abstract ontology.[26] But my own discomfort with the Quinean picture goes beyond the detail of the theoretical virtues, to the holistic model of confirmation itself. Based on a look at the historical case of atomic theory, I suggest that the theory enjoyed the Quinean virtues in abundance by 1860, when its successes in chemistry were crowned by the computation of stable atomic numbers, and even more so by 1900, after the rise of kinetic theory in physics. But scientists were

[25] See Burgess (1983, 1990), and Burgess and Rosen (1997, pp. 214–219).
[26] See Maddy (1997, pp. 154–157).

not satisfied until the direct detection of atoms by Perrin, verifying the crucial predictions of Einstein's 1905 calculations. This, I claim, means that the theoretic virtues are not enough; that enjoyment thereof is not what evidence is; that our best scientific theory is not confirmed as a whole; that some of its posits are properly regarded as fictional until further, more specific testing is possible.[27]

This anti-holistic point of view makes room for the wider range of things that scientists want to say—for example (in 1900), that atomic theory is one of our most highly accepted theories, but we don't yet know whether or not atoms exist, or (right now) that general relativity is one of our most highly accepted theories, but we don't know for sure if space-time is a continuous manifold.[28] The actual attitudes of practitioners toward their best theories are complex and nuanced, as when scientists worry over whether or not some aspect of their best theory is or isn't an artifact of their mathematical modeling, or when Einstein admits to using the continuum to formulate general relativity because "I have been unable to think of anything organic to take its place" ((1949), p. 686). Burgess (with Rosen) expresses some sympathy for this line of thought: it "might be that science itself makes invidious distinctions"; to show this would require "presenting studies of the distinctions and divisions observed within the community of working scientists" (Burgess and Rosen 1997, p. 213). This is what I've tried to do.

Finally, we post-Quineans owe one more item in our scientific study of science, namely, an account of mathematical methods. Burgess's general observations on this line have already been noted: rigor and consistency are important mathematical standards; economy of ontology is not. He also clearly regards the methods of mathematics as distinct from those of the natural sciences:

> Among sciences, mathematics is, owing to its distinctive methodology of deductive proof, a special case (Burgess 1992a, p. 437)
>
> ... Rigorous proof is clearly distinguishable from systematic observation or controlled experiment. (Burgess 1992b, p. 10)

Beyond this, however, he sets aside the problem of axiom selection and, it seems, the related dynamics of concept formation, which have been my focus. Let me take a quick look at how this goes.

One moral of metaphysics naturalized is that natural science itself tells us a considerable amount about ontology. For example, medium-sized physical objects

[27] See Maddy (1997, pp. 135–143). Oddly enough, Burgess's naturalist might disagree with mine about the current status of atoms; he writes (with Rosen): "The naturalized epistemologist may largely accept the sceptic's description according to which our method of positing a physical system with parts we do not perceive is just the only effective way for us, with such cognitive capacities as we have, to cope with what we do perceive" (Burgess and Rosen 1997, p. 33; see also p. 212).

[28] See Maddy (1997, pp. 143–151).

exist in space and time, interact with one another causally, are as they are largely independently of our thought and knowledge of them; unobservable atoms also so exist and compose these more familiar objects; and so on. As my naturalist begins her study of the methods, justificatory procedures, and conclusions of pure mathematics, she might well ask an analogous question: What does mathematics tells us about its ontology? Some answers come quickly—there are numbers, sets, spaces, and so on—but very little is forthcoming about the nature of this existence. Further claims—that mathematical things exist in a non-spatiotemporal, acausal world; that mathematical things are mental constructions of an idealized mathematician; that mathematical things exist only as fictions—can be found in the literature, but a look at their role, especially in resolution of various historical debates, suggests that they are not integral parts of mathematical method, but extramathematical philosophizing.[29] If this is right, the upshot is that mathematics, in contrast to natural science, tells us nothing about the metaphysical nature of its objects beyond the bare claim that they exist.

In her analysis of mathematical methods, then, the naturalist should ignore such extramathematical metaphysical debates and attend to the explicit or implicit intramathematical reasons being offered for one course of action or another. The hope is that an understanding of the goals of a particular mathematical undertaking can be reached, and that alternative methods can then be evaluated in terms of their effectiveness as means toward those goals.[30] In (2001b), I sketch such analyses of the development of the concepts "group" and "topological space." In (1997, pp. 206–232), I argue on such grounds that the set theorist's rejection of $V = L$ is rational, given the goals of set-theoretic practice. This highlights the contrast between Quine's naturalist and mine: the Quinean endorses $V = L$, applying the methods of natural science, while my naturalist rejects it, applying the methods of the relevant branch of mathematics, that is, the methods of set theory.

Finally, let me emphasize that a purely internal, methodological study of mathematics is not the only investigation of the subject that my naturalist can undertake. Mathematics is a form of human activity, a distinctive linguistic practice, and as such it can be studied like any other such practice: by linguistics, psychology, and so on, as well as various natural scientific studies spanning the standard disciplines. Here the naturalist will face questions about the similarities and dissimilarities between mathematical and natural scientific language, and questions about how this practice manages to function so effectively in natural science. These inquires will raise naturalized versions of traditional philosophical questions about the ontology and epistemology of mathematics. This naturalized

[29] This is a difficult distinction to draw. See Maddy (1997, pp. 185–193).

[30] See Maddy (1997, pp. 193–200). Notice that this is the sort of intramathematical ıstification and critique that ought to be acceptable to Burgess.

philosophy of mathematics[31] is distinct from the naturalized methodology of mathematics discussed above but, given that we want a philosophical analysis of mathematics as it is, our naturalized philosopher must respect the practice; justification and criticism are internal to the practice, the province of internal methodology, not philosophy, even naturalized philosophy.

II.4. The Indispensability Argument

We can now assess the effects of these post-Quinean departures on the crucial indispensability argument. On Burgess's analysis, economy of abstract ontology is not among the theoretical virtues, so our "best scientific theory" will include mathematical entities regardless of the success or failure of strenuous nominalistic efforts to remove them.[32] There is still room here for a naturalistic argument to the existence of mathematical entities via holism and the criterion of ontological commitment,[33] but this is not the line Burgess takes. There are, no doubt, various more subtle reasons for this, but one straightforward motivation is clear:

> A thorough-going naturalist would take the fact that abstracta are customary and convenient for the mathematical (as well as other) science to be sufficient to warrant acquiescing in their existence. (Burgess and Rosen 1997, p. 212)

For Burgess (writing with Rosen), mathematics is a science in its own right, and fully capable of justifying its own existence claims.[34] Thus, Burgess embraces an ontology of mathematical entities, but on grounds quite different from those of the Quinean naturalist.

Since I share Quine's starting point in natural science, my own engagement with his indispensability argument focuses on our disagreement over holism. If cases like atomic theory show that proper scientific method does not regard the

[31] Critics of Maddy (1997) have sometimes complained that the position leaves no room for philosophy (as opposed to methodology) of mathematics, but naturalized philosophy of mathematics, as understood here, is described and endorsed on pp. 200–205.

[32] Burgess (1998) calls this "unconditional anti-nominalism." Of course, if the nominalists were to produce math-free scientific theories that improved on our current ones on other theoretical virtues, these would be preferred, but Burgess argues in some detail that they have not done this.

[33] Burgess (with Rosen) expresses some doubts about Quine's criterion (Burgess and Rosen 1997, pp. 225–232), but in the end, he seems happy to conclude that current science is committed to mathematical entities.

[34] He writes, "Numbers, even if real, aren't physical, and so the physicist can pass the buck to the mathematician... mathematical existence questions are questions for mathematicians and not for... empirical scientists" (personal communication, quoted with permission).

existence of all posits of our best scientific theory as confirmed, then we have to look more carefully at the status of its mathematical posits in particular. Obviously much mathematics occurs in explicitly idealized situations, when physical situations are mathematized in terms of simple geometrical structures, when large finite collections are treated as infinite, when discrete situations are treated as continuous, and so on; surely no simple ontological morals should be drawn from these appearances of mathematics.[35] In more fundamental theories, the most convenient and effective mathematics is used, seemingly without qualm, as Burgess suggests. But it is also true that the appearance of, say, a continuous manifold in our best description of space-time does not seem to be regarded as establishing the continuity of space-time; the microstructure of space-time remains an open question.[36]

These observations suggest, first, that holism is incorrect, that our best scientific theory is not simultaneously confirmed in all its parts, that at least in cases like atomic theory, some variety of "direct detection" is required. Recall Quine's claim that the theoretical virtues served to establish the existence of atoms *and* of medium-sized physical objects—that this is, in the end, what evidence is; applying this conclusion to mathematical objects yields the indispensability argument. But if the evidence for atoms is something more direct—and surely medium-sized physical objects are "directly detectable" if anything is—it follows that the "evidence" for mathematical objects is not the same as for the others. And, second, when we look more closely at the considerations that actually move scientists to include the mathematical posits they do, we find the likes of convenience and effectiveness. Perhaps unsurprisingly, when structures are posited on such a basis, the success of the overall theory in which they appear is not regarded as confirming their existence. On this analysis, the indispensability argument fails because ordinary scientific standards do not confirm all parts of our best theory, and because the mathematical posits are not among the posits that are confirmed.

There are passages where Burgess might be taken to agree with this conclusion:[37]

> An ontology of abstracta may be one feature of [our] current theories that is merely *conventional*. (Burgess 1983, p. 99)

> Our science [is] the way it is in part because the universe is the way it is and in part because we are the way we are... the presence of "Avogadro's number" in the language of science is not *caused* by the presence of Avogadro's number

[35] See Maddy (1997, pp. 143–146).

[36] We should distinguish between the purely mathematical existence assumptions involved in this application of mathematics—the existence of a continuous manifold—and the physical structural assumptions that accompany it—continuous space-time. Both seem to be added for convenience and effectiveness (we know of no other way to represent space-time), and neither seems to be regarded as confirmed. For more, see Maddy (1997, pp. 154–157).

[37] Though, in contrast, the passage quoted in note 27 seems more in sympathy with a holistic view like that of Quine (1955).

> in the universe. The relation between name and object is thus in one crucial respect unlike that between mirror image and object in the scene reflected. (Burgess 1990, p. 13)

And with Rosen:

> The naturalized epistemologist may largely accept the nominalist's description according to which our method of positing mathematical systems as models is just the most efficient way for us, with such cognitive capacities as we have, to cope with physical systems. (Burgess and Rosen 1997, p. 33; see also pp. 238–244)

If we can see, in the course of our scientific study of science, that certain parts of our theory are there by convention, that they don't reflect what's actually present in the physical situation, that we posit them merely because we have no better way of describing things, then it seems reasonable to conclude that these parts of our theory are not, in fact, confirmed by our scientific methods. But we've seen that, for Burgess, this is beside the point; the ground of mathematical ontology lies in mathematics, not natural science.

So, in short, Burgess sees the indispensability argument as flawed—it concedes too much to the nominalist in requiring that mathematics be indispensable—and beside the point—because mathematical science by itself gives us grounds for accepting the existence of mathematical entities. My own position is both more Quinean, in granting that natural science is the final arbiter of existence, and more firmly non-Quinean, in its explicit rejection of holism. The upshot, for my naturalist, is that the role of mathematics in natural science does not seem to support the claim that mathematical things exist.

II.5. Post-Quinean Philosophy of Mathematics

The outlines of Burgess's naturalism should now be clear, but there remains an open question for my version: Does pure mathematics provide support for the existence of mathematical objects? It surely implies that there are numbers, sets, functions, geometric and topological spaces, real numbers, continuous manifolds, Hilbert spaces, algebraic structures, complex numbers, and much more. Burgess would count this as the end of the story: the existence of these items is implied by one among our best sciences. But for my naturalist, natural science is the final arbiter of what there is, and it doesn't seem to support its mathematical ontology, so my story will have to be more complex.

Granting that mathematics itself offers no ontological guidance beyond the minimal "mathematical things exist," naturalized methodology of mathematics is of no further use; we can only turn to naturalized philosophy of mathematics. One chapter of this study is the aforementioned conclusion that the confirmation of natural scientific theories in which mathematics figures does not confirm its ontology, that the empirical confirmation does not transfer holistically to the mathematical existence claims. But perhaps support will come from other quarters of this study—for example, from the investigation of the semantics of mathematical language,[38] or from the analysis of human mathematical experience, or of how pure mathematics comes to be so effective in applications.[39]

My guess is that, in the end, the explanations and accounts of naturalized philosophy of mathematics will not involve a literal appeal to the objects of pure mathematics.[40] But even if this is so, it would not settle the case between, say, "set-theoretic claims are true, and sets just are the kind of things that are referred to and known about by set-theoretic methods" and "set-theoretic claims aren't true and sets don't exist, but it's perfectly rational and proper to say that they are and do while developing set theory in the pursuit of various mathematical goals." In fact, I suspect that a decision on these matters will have more to do with the theory of truth than with the methodological or naturalized philosophical facts about mathematics or natural science.[41]

But I leave this for another time, along with a large number of other worthy naturalistic undertakings: for example, a more complete account of set-theoretic methods, of the distinctive methods of the various other branches of mathematics, and of their interrelations;[42] a full discussion of how mathematics works in natural science, from the sorts of detailed analyses given by applied mathematicians (e.g., an explanation of why it's proper to regard fluids as continuous substances when we do fluid dynamics), to consideration of general questions

[38] As we've seen, Burgess admits that there are significant differences between mathematical and natural scientific terms: "the presence of 'Avogadro's number' in the language of science is not *caused* by the presence of Avogadro's number in the universe" (Burgess 1990, p. 13). I wouldn't have chosen this example—given the suggestion that claims like $2+2=4$ correspond to rudimentary and robust logical truths about the world—but I second the point about most of mathematics. Whether or not this is a fact about reference will depend on one's theory of reference (see below).

[39] Steiner (1998) draws strong "anti-naturalistic" conclusions from his analysis of the way mathematics works in application. He takes "naturalism" to be the view that "the human race is [not] in some way privileged, central to the scheme of things... that the universe is indifferent to the goals and values of humanity" (p. 55). This does seem to be the view of natural science, and hence of all three naturalisms considered here.

[40] That is, any appeal outside the context of some idealized or conventional mathematical modeling.

[41] Burgess (with Rosen) follows Quine in holding that the naturalist must embrace a disquotational theory of truth (Burgess and Rosen 1997, p. 33). I disagree, taking the decision between disquotational and more robust theories to be an open scientific question (see Maddy 2001a, III), but I don't think this disagreement is what's at issue here.

[42] As urged by Tappenden (2001, p. 497).

about the applicability of mathematics (the "miracle" of applied mathematics); a broad and careful study of how science can assess the ontological morals of its own best theories, beginning with a study of how we tell when an aspect of our best scientific theory is an artifact of the mathematical modeling; and so on. My more modest aim here has been to illuminate the outlines of three versions of naturalism, and perhaps to demonstrate that philosophizing naturalistically involves attention to matters of considerable subtlety and detail. My hope is that the reader may be inspired to further investigation of one or another of these, or some improved descendant thereof!

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