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CHAPTER 12

QUINE AND THE WEB OF BELIEF

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1. Introduction

When W. V. Quine began his philosophical career, logical positivism and logicism both flourished. The positivists distinguished sharply between truths known empirically through sense experience and truths known a priori or independently of sense experience. But they resolutely rejected a priori intuition, be it Platonic or Kantian, as a source of mathematical knowledge, and they believed that the inadequacy of Mill's empiricist philosophy of mathematics shows that sense perception is no source, too.[1] Fortunately, logicism's new and richer conception of logic and its reduction of mathematics to logic provided the positivists with a ready-made basis for mathematical knowledge: they took it to be a priori knowledge grounded in our conventions for using logical (and mathematical) symbols. And this is just what Quine called into question. Neither their a priori-empirical distinction nor their doctrine of truth by convention survived his criticisms unscathed. Even the thesis that mathematics is logic came to be seen in a different light as a result of Quine's theorizing about logic. In view of this, it is ironic that significant themes from both logical positivism and logicism still ran through Quine's own work.

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[1] For a fuller discussion of Mill's views, see chapter 3 in this volume.

Although his negative work brought Quine into philosophical prominence by the 1950s, he had already developed a positive philosophical vision, which he expanded and refined during the next forty years. Both the power of Quine's criticisms and the depth and scope of his positive views combined to make Quine an influential—perhaps the most influential—philosopher of mathematics today. He set the agenda for many current discussions: the role of convention in logic and mathematics, the nature of the a priori, criteria of ontological commitment, the indispensability of mathematics in science, the reducibility of mathematics to logic, the nature of logic, and the value of ontological parsimony.

Here is a very brief overview of Quine's philosophy of mathematics. Its fundamental feature is a combination of a staunch empiricism with holism. These are the ideas that the ultimate evidence for our beliefs is sensory evidence and that such evidence bears upon our entire system of beliefs rather than its individual elements (whence the phrase "the web of belief"). This means that our evidence for the existence of objects must be indirect, and extracted from the evidence for our system of beliefs. Thus it was essential that Quine develop a criterion for determining which objects our system commits us to (a criterion of ontological commitment). Seeing science as the fullest and best development of an empirically grounded system of beliefs, Quine heralded it as the ultimate arbiter of existence and truth. Mathematics appears to be an indispensable part of science, so Quine concluded that we must accept as true not only science but also those mathematical claims that science requires. According to his criterion of ontological commitment, this also requires us to acknowledge the existence of those mathematical objects presupposed by those claims. Finally, we usually take ourselves to be talking about a definite system of objects. However, there is enough slack in the connection between our talk of objects and the evidence for it that one can uniformly reinterpret us as referring to another system of objects while holding the evidence for the original system fixed. Thus we have ontological relativity: only relative to a fixed interpretation of our beliefs is there a fact as to our ontology.

This chapter will focus on Quine's positive views and their bearing on the philosophy of mathematics. It will begin with his views concerning the relationship between scientific theories and experiential evidence (his holism), and relate these to his views on the evidence for the existence of objects (his criterion of ontological commitment, his naturalism, and his indispensability arguments). This will set the stage for discussing his theories concerning the genesis of our beliefs about objects (his postulationalism) and the nature of reference to objects (his ontological relativity). Quine's writings usually concerned theories and their objects generally, but they contain a powerful and systematic philosophy of mathematics, and the chapter will aim to bring this into focus. Although it will occasionally mention the historical context and evolution of Quine's philosophy, it

2. Holism and the Web of Belief

2.1. Holism: The Basic Idea

When I speak of holism here, I shall intend epistemic or confirmational holism. This is the doctrine that no claim of theoretical science can be confirmed or refuted in isolation, but only as part of a system of hypotheses. This is different from another view frequently attributed to Quine, namely, meaning holism, which is roughly the thesis that an expression depends upon the entire language containing it for its meaning.

Quine used a number of metaphors to expound his holism. The following passage from "Two Dogmas of Empiricism," which is an early and particularly strong formulation of his view, uses the metaphors of a fabric and a field force:

> 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 or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or to change the figure, total science is like a field of force whose boundary conditions are experience. (Quine 1951, 42)

In a later book with Joseph Ullian, the operative metaphor was one of a web, reflected in the book's title, *The Web of Belief* (1970).

Whatever the metaphor, holism is based upon an observation about science and a simple point of logic. The observation is that the statements of any branch of theoretical science rarely imply observational claims when taken by themselves, but do so only in conjunction with certain other statements, the "auxiliary" hypotheses. For example, taken in isolation, the statement that water and oil do not mix does not imply that when I combine samples of each I will soon observe them separate. For the implication to go through, we must assume that the container contains no chemical that allows them to homogenize, that it is sufficiently transparent for me to observe the fluids, that my eyes are working, and so on. Hence—and this is the point of logic that grounds holism—if a hypothesis H implies an observational claim O only when conjoined with auxiliary assumptions A, then we cannot deductively infer the falsity of H from that of O, but only that of the conjunction of H and A, H & A. Furthermore, insofar as observations confirm theories, the truth of O does not confirm H but rather H & A. Strictly speaking, it is systems of hypotheses or beliefs rather than individual claims to which the usual, deductively characterized notions of empirical content, confirmation, and falsification should be applied.

Pierre Duhem expounded these ideas at the beginning of the twentieth century, and defended the law of inertia and similar physical hypotheses against the charges that they have no empirical content and are unfalsifiable. One way of

putting the law of inertia, you will recall, is to say that a body remains in a state of uniform motion unless an external force is imposed upon it. Since we can determine whether something is moving uniformly only by positing some observable reference system, this law, taken by itself, implies no observational claims. Furthermore, by appropriately changing reference systems we can guarantee that a body moving uniformly relative to our present system is not relative to the new one, and thereby protect the law against falsifying instances. All this troubled the law's critics, because they believed that it should have an empirical content and be falsifiable. Duhem responded to their worry by observing that the law readily produces empirical consequences when conjoined with auxiliary hypotheses fixing an inertial system; and that in needing auxiliaries to produce empirical consequences, it was no different from many other theoretical principles of science, whose empirical content everyone readily acknowledged. Thus the law's critics could not have it both ways: to the extent that their critique challenged the empirical status of the law of inertia, it also challenged that of most other theoretical hypotheses. (Duhem 1954).

Using logic to extract observational consequences from the law of inertia also depends upon including mathematical principles among the auxiliary hypotheses. Duhem drew no conclusions from this about mathematics. But Quine subsequently did, as the quote above indicates. Using the very strategy Duhem used in defending the law of inertia, he argued that even mathematical principles, which by most accounts are just as unfalsifiable and devoid of empirical content as the law of inertia, share in the empirical content of systems of hypotheses containing them (Quine 1990, 14–16).

In his later writings Quine toned down his holism. In speaking of "the totality of our . . . beliefs," the passage quoted above gives the impression that each of our beliefs and observations is connected logically to every other belief and observation. In *Word and Object*, Quine notes this and qualifies his holism:

> . . . this structure of interconnected sentences is a single connected fabric including all sentences, and indeed everything we ever say about the world; for the logical truths at least, and no doubt many more commonplace sentences too, are germane to all topics and thus provide connections. However, some middle-sized scrap of theory will embody all the connections that are likely to affect our adjudication of a given sentence. (Quine 1960, 12–13)

(Note also the footnote to the first sentence of this passage:

> This point has been lost sight of, I think, by some who have objected to an excessive holism espoused in occasional brief passages of mine. Even so, I think their objections largely warranted. (p.13)

So long as these "middle-sized scraps" of theory contain bits of mathematics, Quine's points about its falsifiability and empirical content will continue to hold.

2.2. Important Consequences of Quine's Holism

Let us note some important consequences of Quine's version of holism. First, and foremost, it entails rejecting the distinction between empirical and a priori truths, where the a priori truths are those that are known independently of experience and immune to revision in the light of it. This is because, for Quine, experience bears upon bodies of beliefs and, insofar as it may be said to bear upon individual beliefs of a system, it bears upon each of them to some extent. Quine rejects other means for distinguishing between the a priori and the empirical, such as the use of a priori intuition, self-evidencing truths, or sentences true by convention or by virtue of the meanings of their component terms. Thus no belief is immune to revision in the face of contrary experience. As Quine famously put it in "Two Dogmas":

> ... it becomes folly to seek a boundary between synthetic statements, which hold contingently upon experience, and analytic statements, which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery [of experience] can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. (Quine 1951, 43)

Second, although Quine acknowledged abstract objects and their perceptual inaccessability, and even spoke of some of our beliefs as arising from observation and others as arising through the exercise of reason, this provided him with no epistemological distinction for privileging statements about abstract mathematical objects. The difference here is simply one of degree rather than of kind.

Mathematics does not yield a priori knowledge, though it seems to proceed largely through the exercise of reason. What, then, of philosophy? Since, for Quine, there is no a priori or conceptual knowledge, any knowledge that philosophy can impart about science must be a piece of science. This is part of what Quine calls naturalism. Here are two passages characterizing it:

> ... naturalism: abandonment of the goal of a first philosophy. It 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 1981b, 72)

> ... naturalism: the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described. (Quine 1981a, 21)

As we will see below, Quine's naturalism is a key component of his argument for mathematical realism (see chapters 13 and 14).

Several interesting questions now arise concerning Quine's own philosophical theory: What is its status? Is it to be a contribution to knowledge? And if it is knowledge, what is its source? Now philosophers commonly distinguish between normative and descriptive epistemology. The former assesses our ways of knowing and systems of beliefs with an eye toward improving upon them; the latter merely describes them. Many of Quine's more recent pronouncements concerning epistemology indicate quite clearly that he takes himself to be pursuing it descriptively. For example, in "Epistemology Naturalized" we read:

> 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. The human subject is a 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 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 have always prompted epistemology; namely, in order to see how evidence relates to theory, in what ways one's theory of nature transcends any available evidence. (Quine 1969b, 82–83)

Can we interpret Quine's doctrine of holism as a piece of descriptive epistemology? As I noted earlier, holism arises in part from observing scientific practice and noting that scientists usually make numerous auxiliary assumptions when designing experiments for testing their hypotheses. That they do so is a straightforward descriptive claim that in turn can be scrutinized scientifically. But this is not enough, since this claim will not yield the conclusion that no statement of science is immune to revision.

Curiously, I don't think that Quine intended his claim that no statement is immune to revision as a description of what scientists have done or as prediction of what they will do. He would be the first to emphasize how radical it would be to revise mathematics in order to save a scientific theory. Perhaps he meant the conclusion as remark concerning the methodological code to which scientists subscribe. This remark could be counted as descriptive epistemology and again be subjected to scientific scrutiny, although, due to its imprecision, the results are likely to be inconclusive. However, I am inclined to read Quine as claiming that not only do scientists use auxiliary assumptions, they must do so to deduce testable conclusions from their hypotheses. If scientists must use auxiliary hypotheses, then it would follow that a negative test result would only call into question the conjunction of the auxiliaries and the main hypothesis rather than the main hypothesis alone. So, absent further specification, revising any component of this conjunction would violate no law of logic. Moreover, given that scientists freely draw our auxiliary assumptions from the entire body of science, we can arrive at the more general conclusion that circumstances could arise in which logic would permit revising any one of our (nonlogical) beliefs.

On the reading of Quine that I am offering, the claim that none of our beliefs is immune to revision amounts to the thesis that from a logical point of view, none of our beliefs is immune to revision. Now, in speaking of what logic permits, I have been using normative terms. Thus one might wonder whether Quine's holism is a piece of normative epistemology after all. I think not. In the case at hand, apparent normative talk of what logic permits is only a metaphorical substitute for descriptive speculation about how various arguments would fare when subjected to standard logical tests. Nor in applying logic do we involve ourselves in the a priori—provided, of course, that with Quine we reject the distinction between a priori and empirical knowledge.

2.3. Holism and Logic

But what of logic itself? Doesn't its role in forging the connections between theory and experience give a kind of a priori status, some immunity to empirical falsification? While we know that in "Two Dogmas" Quine counted the laws of logic as part of the fabric confronting experience, the next passage seems to reflect a change in his view. Here he is discussing the options we have when revising a set of sentences *S* in the face of failed prediction (a "fateful implication"):

> 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 on determining that the fateful implication still holds without their help. Any purely logical truth is thus exempted, since it adds nothing to what *S* would logically imply anyway.... (Quine 1990, 14)

Quine's point here is that giving up a logical truth to repair *S* is idle. For let *L* be a logical truth and *W* any sentence or conjunction thereof. Then the conjunction of *W* and *L* implies a sentence *O* only if *W* alone implies *O*. To deactivate the fateful implication, we must revamp logic at least to the extent of refusing to recognize that *S* has the implication in question.

Now one might wonder how any revision of logic could even be an option for us. For without logic a failed prediction would be neither connected to a theory nor contrary to it. But this kind of worry can be set aside. Of course, without some logical framework, hypothesis testing could not take place, but that does not mean that the framework and the hypotheses tested cannot both be provisional. Obviously, revisions in the framework must come very gradually, since after changing it, we will need to determine whether previously tested hypotheses still pass muster. Thus, instead of denying all instances of, say, the law for distributing conjunction over alternation, we might reject certain applications of it to quantum phenomena. In this way there would be no danger of lapsing into total incoherence. Nor need we abandon the norms surrounding deduction. While we may change, for example, what counts as an implication or a contrary, we need not abandon norms that commit us to what our theories imply or that prohibit us from simultaneously maintaining two contraries. Logic is revisable, so long as major changes result from the accumulation of minor ones.

2.4. Objections to Holism

Several philosophers have been critical of both the theory of confirmation that seems implicit in holism generally and the account of mathematical evidence that seems implicit in Quine's version of it. Science takes mathematics and logic as fixed points for determining the limits of what we can entertain as serious possibilities (to borrow a phrase from Isaac Levi (1980)). In allowing for experientially motivated revisions of mathematics and logic, Quine appears to be riding roughshod over this feature of scientific practice. Charles Parsons, who has voiced objections of this sort, also points out that Quine seems to provide no place for specific kinds of mathematical evidence, such the intuitive obviousness of elementary arithmetic (Parsons 1979–1980, 1986). In a different vein, Charles Chihara (1990) has observed that in deciding whether to add a new axiom to set theory, no set theorist is going to investigate its benefits for the rest of science. Yet on Quine's approach the ultimate justification of the axiom will rest upon the acceptability of the total system of science to which it belongs. One might add that in developing his own axiomatic set theories, even Quine narrowed his focus to their ability to smoothly reproduce the standard set-theoretic foundations for mathematics while skirting contradictions.

In addition to this, some philosophers continue to hold, contrary to both Quine and Duhem, that observational evidence can be seen to bear upon specific hypotheses instead of whole systems. Elliott Sober, for example, claims that scientific testing consists in deriving incompatible predictions from competing hypotheses. Because these tests share the same auxiliary assumptions, they put specific hypotheses at risk and, consequently, the data they produce reflect upon just these hypotheses and not upon the broader systems to which they belong. Sober also notes that scientific tests never, or hardly ever, put mathematical claims at the risk of being falsified. Because of this, he argues, mathematics cannot share in the confirmation afforded to those hypotheses that do pass such tests. In particular, the mathematical theory of sets, in contrast to, say, the atomic hypothesis, cannot claim empirical support (Sober 1993, 2000).

Now Chihara, Parsons, and Sober are certainly right when it comes to scientific and mathematical practice. Mathematicians tend to keep their focus narrowly mathematical, and their notion of mathematical evidence has much more room for citing obviousness and self-evidence than it does for citing experimental data. But does this refute Quine? Let us take a closer look at these objections. As I see it, they reduce to two points. The first is that Quine is wrong about the revisability of mathematics and logic, and they are a priori, or at least Quine has not shown that they are not. The second is that holism cannot be correct, since it excludes important forms of reasoning used in science and mathematics. Thus Quine's argument, based as it is upon holism, fails to show that mathematics and logic are not a priori.

Sober seems to be urging the first point when he points out that there are very few mathematical statements that we know how to test empirically in any reasonable sense of empirical testing.[2] Now this would tend to favor the apriority of mathematics if we knew how to test empirically almost all statements of theoretical science. But in fact we don't know how to apply the sorts of specific tests that Sober has in mind to the framework principles of the various branches of science. They function as the background principles that we hold fixed when testing lower-level hypotheses. For example, we don't know how to test the hypothesis that space–time is a continuous manifold, and, given quantum mechanics, this may be untestable in principle. (To put my point in Kuhnian terms, the framework principles are part of the paradigms held fixed while we do the testing that is part of ordinary science.) If we take Sober's idea to heart, we will count much more as a priori than fans of the a priori want. Furthermore, we might find ways of testing many more mathematical statements if we tried. At best we have a notion of the a priori that is relative to our current ability to design empirical tests.

On the other hand, one might argue against Quine that as a matter of fact, we have never revised an established branch of mathematics in the face of empirical findings, and thus have little grounds for thinking that it is revisable.[3] Of course, *never revised* does not entail *not revisable*, and even Sober admits that when an observation falsifies a prediction, there is a choice of revising the main hypothesis or the auxiliary assumptions. (Sober 2000, 267). The problem with this response is that no well-formulated methodology recommends taking the choice of revising the mathematics contained in the auxiliary assumptions. Quine's own suggestions (e.g., that we revise so as to obtain the simplest overall theory and try to save as much of our current theory as we can) are too vague, and fail to lead to a unique outcome. Furthermore, we have no reason to believe that revising mathematics or logic will ever lead us to a theory that would even count as optimal in comparison

[2] Many philosophers would argue that no mathematical statement can be tested empirically. However, Sober cites a mathematical conjecture that he takes to have been tested empirically (Sober 2000, 268–269). I have also argued for the empirical testability of certain mathematical statements (Resnik 1997).

[3] Let us set the case of Euclidean geometry to the side, since there is much controversy as to whether in using non-Euclidean geometry in general relativity theory, Einstein falsified a mathematical theory of space.

with its competitors.[4] But, perhaps it is enough that revising logic or mathematics could lead us to a theory that is at least *acceptable*, if not optimal. Ruling this out would appear to beg the question by assuming that any theory arising from revising mathematics or logic is unacceptable. We may have arrived at a standoff here. Quine and his fans see revising mathematics and logic as a live option to be used only when we must take extreme measures. His opponents fail to see how it could ever be appropriate to exercise this option.

We still have to consider the second point: that holism cannot be correct, since it excludes important forms of reasoning used in science and mathematics. The strategy I will use here is to try to show that even within the framework of empiricist holism, one can make sense of the scientific and mathematical practice to which Sober, Chihara, and Parsons have called our attention.

Duhem noted that scientists often leave their auxiliary assumptions unquestioned in the course of testing hypotheses, and thereby take the evidence they obtain as bearing upon the main hypotheses. Duhem said that ordinarily this was just using "good sense," but he added, "These reasons of good sense [for favoring certain hypotheses] do not impose themselves with same implacable rigor that the prescriptions of logic do" (Duhem 1954, 217–218). Holists may readily admit that it is rational for scientists to fix certain hypotheses (as auxiliaries) while testing others, and thus also rational (in the practical sense) for them to act as if the evidence they obtain bears upon the specific hypotheses being tested. Holists can thereby accommodate the type of hypothesis testing that Sober applauds. They will simply deny that, independently of our holding the "auxiliaries" fixed, a logical (or a priori) evidential relationship obtains between the hypotheses tested and the evidence.

Let's develop this point further. In practice the various branches of science take large blocks of theory for granted. Molecular biology, for example, is developed within a framework that draws upon principles of more general theories, such as chemistry, physics, and mathematics. We also find a division of labor in the sciences: mathematicians normally do not meddle in physics nor physicists in mathematics, and biologists and chemists are normally not competent to suggest changes in mathematics or physics even when they might want to see it changed. As a result, when something goes awry in a relatively local science (say, biology), it is not likely that practitioners of more global sciences (say, physics or mathematics) will hear of it, much less be moved to seek a solution through modifying their own more global theories. Nor is it likely that the specialists in a local theory will tinker with global background theories to resolve local anomalies.

This is not just a matter of sociology; it is good sense, too. Practical rationality counsels specialists to attempt to modify more global theories only as a last resort; they probably do not and cannot know enough to tackle the task, and modifying a

[4] Field (1998, 13–14) makes this point.

more global theory is likely to send reverberations into currently quiescent areas of science. Quine has expressed the point by saying that in revising their theories, scientists should minimize mutilation.

Specialization has also fostered local methodologies and standards of evidence. These provisionally override more global and holistic perspectives and declare data, obtained via local methods, to bear on this or that local hypothesis. These will tell us, for example, that we have more reason to be confident of the existence of electrons than of gluons or of the existence of prime numbers than of inaccessible cardinals. Holists will urge, however, that local conceptions of evidence, in particular those that lead us to take data as confirming specific hypotheses, are ultimately justified pragmatically via their ability to promote science as a whole, and not via some a priori basis. Hence the divisions we find in the practice and scope of the various sciences should not be taken as refuting holism or as indicating hard-and-fast epistemic divisions between mathematics and the so-called empirical sciences. Nor do they show that it is invariably irrational to modify some global principle to fix a more local problem.

These reflections apply to our ordinary conception of mathematical evidence as well. Empirical success no more confirms individual mathematical claims than it does individual theoretical hypotheses. However, it does provide a pragmatic justification for positing mathematical objects, truths about them, and principles for applying mathematical laws to experience. It encourages mathematicians to develop their own standards of evidence, so long as the result does not harm science as a whole. Because mathematics is our most global science, we should expect that many mathematical methods and principles would be justified by means of considerations neutral between the special sciences, and thus often pertaining to mathematics alone. In this way we can reconcile holism with the features of mathematical practice that Chihara, Parsons, and Sober have emphasized.

Considering the place of proof in mathematics will illustrate this. Early mathematicians probably took their experience with counting, bookkeeping, carpentry, and surveying as evidence for the rules and principles of arithmetic and geometry that they eventually took as unquestionably true. They began to put more emphasis on deduction after they became aware of the difficulties in deciding certain mathematical questions by appealing to concrete models—which, for example, are notoriously unreliable in deciding geometric questions. By the time of the Greeks, the goal of mathematics was to prove its results. Moreover, proof wins out from the perspective of science as a whole. For requiring mathematicians to give proofs increases the reliability of their theorems, and decreases their susceptibility to experimental refutation.

The development of non-Euclidean geometry and abstract algebra further promoted the purely deductive methodology of the axiomatic method through showing mathematicians how to make sense of structures that might not be realized physically. It also promoted a shift from viewing mathematical sentences as unqualifiedly true or false to regarding them as true or false of structures of various types. These two developments have further insulated mathematics against empirical refutation. To see how, consider the case of Euclidean geometry. General relativity did refute it in its original role as a theory of physical space, but it still has important mathematical models, and survives through being reinterpreted as a theory of Euclidean spaces. A similar move is available when a scientific model incorporating a bit of mathematics proves inadequate to a physical application. It is usually far simpler to save the mathematics from refutation and conclude that the physical situation to which it was being applied failed to exhibit a suitable structure. We can use this technique to rescue any consistent theory—even a so-called empirical theory—by reinterpreting it. However, mathematical theories need no reinterpretation, since they do not assert that the structures they describe are realized in this world.

Of course, more needs to be done to answer fully the objections to the holist account of mathematical evidence. The foregoing paragraphs are offered as an indication of a way of responding to those objections.

3. Ontological Commitment: Recognizing Objects

3.1. Quine's Criterion of Ontological Commitment

One's first thoughts on the recognition of objects might be that we first acknowledge an object and then learn things about it. For example, we first see the tiger as it emerges from the brush, and later realize that it is about to attack us. But this would be contrary to both more sophisticated commonsense and holism. For to see the tiger we must see it as a tiger, and to do that, we must hold many beliefs about it. For example, we probably will believe that it is a large, animate object that looks like other objects we have identified as tigers. According to holism, recognizing the tiger is a matter of modifying our system of beliefs in certain ways; thus our evidence for the existence of the tiger ultimately traces to our evidence for a system of beliefs concerning it. What goes for tigers goes for objects generally: our evidence for them depends upon our evidence for our beliefs countenancing them. For this reason it is essential that we have a means for determining which objects we commit ourselves to in holding a system of beliefs. Quine's criterion of ontological commitment serves this purpose.

Quine's criterion applies directly to sentences and sets thereof (theories), and only indirectly to beliefs. (The transition from beliefs to sentences is based upon assuming we can determine people's beliefs by seeing what sentences they are prepared to affirm.) Then the roughest form of the criterion may be put quite simply: a set of sentences is committed to those entities that must exist in order for the members of the set to be true. But this happens when the sentences in question affirm the existence of things through the use of quantifiers. Thus we may put the criterion more precisely as *Sentences are committed to those entities over which their bound variables must range in order for them to be true.* Here is how Quine puts it in "On What There Is":

> ... We can very easily involve ourselves in ontological commitments by saying, for example, that *there is something* (bound variable) which red houses and sunsets have in common; or that *there is something* which is a prime number larger than a million. But this is essentially the *only* way we can involve ourselves in ontological commitments: by our use of bound variables. (Quine 1948, 12)
>
> ... The variables of quantification, "something," "nothing," "everything," range over our whole ontology, whatever it may be; and we are convicted of a particular ontological presupposition if, and only if, the alleged presuppositum has to be reckoned among the entities over which our variables range in order to render one of our affirmations true. (Quine 1948, 13)

Quine came to emphasize the triviality of what's going on here.

> The artificial notation "$\exists x$" of existential quantification is explained merely as a symbolic rendering of the words "there is something x such that." So, whatever more one may care to say about being or existence, what there are taken to be are assuredly just what are taken to qualify as values of "x" in quantifications. The point is thus trivial and obvious. (Quine 1990, 26–27)

However, often it is far from clear as to what the ontological commitments of a set of sentences are. This is especially true of day-to-day talk in ordinary language.

> The common man's ontology is vague and untidy in two ways. It takes in many purported objects that are vaguely or inadequately defined. But also, what is more significant, it is vague in its scope; we cannot even tell in general which of those vague things to ascribe to a man's ontology at all, which things to count him as assuming.
>
> ... a fenced ontology is just not implicit in ordinary language. The idea of a boundary between being and nonbeing is a philosophical idea, an idea of technical science in a broad sense....
>
> We can draw explicit ontological lines when desired. We can regiment our notation.... Then it is that we can say the objects assumed are the values of the variables.... Various turns of phrase in ordinary language that seem to invoke novel sorts of objects may disappear under such regimentation. At other points new ontic commitments may emerge. There is room for choice, and one chooses with a view to simplicity in one's overall system of the world. (Quine, 1981a, 9–10)

To illustrate what Quine has in mind, consider this bit of hypothetical dialogue:

> John: I saw a possible car for you.
> Jane: I have many things occupying my time, but for your sake, I will look at it.

John seems to be committed to *possible* cars (he says he saw one). Mary, on the other hand, seems committed to her own time, things that occupy it, and John's sake. But what is a sake? Or a thing occupying a person's time? Or a person's time, for that matter? If we simply paraphrase the dialogue, we can avoid such questions and reduce its apparent ontological commitments.

> John: I saw a car that might do for you.
> Jane: I am very busy now, but I will look at it, since you want me to.

We have done quite a bit to clean up John's and Mary's ontologies! Now we may take John to commit himself to just the car he actually saw instead of a possible one, whatever that might be. Mary, though she is quite busy, need no longer be seen as involved with sakes or things occupying her time. This is just a somewhat humorous illustration of Quine's procedure. For Quine the serious applications are scientific and philosophical theories. By paraphrasing them into the canonical notation of extensional first-order logic, we try to assess and reduce ontological commitments.

> ... But the simplification and clarification of logical theory to which a canonical notation contributes is not only algorithmic; it is also conceptual. Each reduction that we make in the variety of constituent constructions needed in building the sentences of sciences is a simplification. Each elimination of obscure constructions or notions that we manage to achieve, by paraphrase into more lucid elements, is a clarification of the conceptual scheme of science. The same motives that impel scientists to seek ever simpler and clearer theories adequate to the subject matter of their special sciences are motives for simplification and clarification of the broader framework shared by all sciences.... The quest of a simplest, clearest overall pattern of canonical notation is not to be distinguished from a quest of ultimate categories, a limning of the most general traits of reality. (Quine 1960, 161)

For Quine, one of the philosopher's major contributions comes from clarifying the language of science and mathematics in order to assess and reduce the ontological commitments of our theories of the world. Though Quine's criterion may be trivial in itself, the applications one might make of it are far from trivial.

3.2. The Canonical Language and Benefits of Regimentation

As we have seen, before applying Quine's criterion, one must paraphrase the theory to be assessed into "canonical notation." Part of the reason for doing so is to eliminate spurious ontological commitments. For example, even the language of working mathematics contains terms, such as, "1/0" or "(sin x)/x with $x = 0$" that appear to be referential, yet may fail to denote anything. Now we might declare sentences containing such terms to be false just as we might declare pieces of fiction false. But then what do we do with truths such as "There is no such number as 1/0"? In the face of this, some philosophers have responded that every name denotes something—even "the largest natural number." But for Quine there is a simpler and more economical solution: we avoid the offending expressions by paraphrasing away names and functional terms altogether. This can be done quite straightforwardly using Russell's theory of descriptions. Quine employs it, or an equivalent device, quite frequently, and in his canonical notation the only singular terms are variables.

Quine sees logic as ending with first-order logic,[5] and his canonical notation also bans other notable adjuncts to first-order logic, such as modal operators and substitutional quantifiers. The modal operator "it is possible that" allows us to formulate the claim "It is possible that there are numbers but there are (in fact) no numbers"—a claim to which some nominalists subscribe. Such talk seems to recognize a kind of existence intermediate between being and nonbeing. Quine has never been able to make sense of this idea, and has made relatively little sense of modal operators themselves. Despite this and Quine's influence, modal operators have made a comeback in recent technical philosophy of mathematics.[6]

Substitutional quantification is also popular with nominalists. Instead of requiring the existence of *F*, its truth a substitutional "$\mathcal{E}xFx$" counts as true if and only if "Fx" has a true substitution instance.[7] This will count "$\mathcal{E}x$(x is a flying horse)" as true so long as we take "Pegasus is a flying horse" as true. Since some philosophers demur at counting the latter as false, they can use substitutional quantification to analyze talk of fictional entities. Fans of modal logic have also used it to deal with problems arising in interpreting quantifications containing modal operators. What is more significant for our purposes is that philosophers of mathematics have proposed using it to gain the formal advantages of having classes without having to pay the price of admitting them into one's ontology.

[5] The acceptability of higher-order logic is a complex and technical issue. Evaluating it and Quine's arguments would take a chapter in itself. Fortunately, this volume contains two chapters devoted to second- and higher-order logic (25 and 26).

[6] See chapters 1, 15, and 16.

[7] I have used an italicized "$\mathcal{E}$" to distinguish it from the ordinary (or objectual) existential quantifier. A substitutional "$\mathcal{E}xFx$" can be true without *F*'s existing so long as we count one of its substitution instances as true. (E.g., some philosophers count "$\mathcal{E}x$(x is a flying horse)" as true by virtue of the supposed truth of "Pegasus is a flying horse.") On the other hand, the objectual "$\exists xFx$" can be true without having a true substitution instance when the *F*s are unnamed. Thus "$\exists x$(x is an unnamed real number)" is true while its substitutional counterpart is false.

Although substitutional quantifiers form no part of Quine's canonical notation, he appears to think that, unlike modal operators, they are not intrinsically unacceptable. However, nominalists should draw no comfort from this.

> This does not mean that theories using substitutional quantification and no objectual quantification can get on *without* objects. I hold rather that the question of the ontological commitments of a theory does not properly arise except as that theory is expressed in classical quantificational form, or insofar as one has in mind how to translate it into that form. I hold this for the simple reason that the existential quantifier, in the objectual sense, is given precisely the existential interpretation and no other: there are things which are thus and so. (Quine 1969c, 107)

In Quine's view, rewriting mathematics using substitutional quantifiers would amount to abandoning the quest for ontological economy rather than achieving it.

Today realists in the philosophy of mathematics use Quine's criterion largely to argue for the existence of mathematical entities. "To do science," they claim, "we use variables ranging over mathematical entities and are, consequently, committed to their existence." (More on this in the section 4 below.) Anti-realists, on the other hand, often appeal to Quine's criterion to measure the success of their various attempts at ontological economy. "My system has no variables ranging over mathematical entities," they argue, "so, we need not be committed to their existence." Ironically, it's unclear how successful these anti-realist attempts have been, because they employ languages that exceed Quine's canonical notation.[8]

3.3. Ontological Naturalism: Science Is the Ultimate Arbiter of Existence

Quine often emphasizes that his criterion tells us only what a theory says exists. It does not tell us what does exist. That, you will recall, is the job of science; this is Quine's naturalism.

> ... naturalism: the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described. (Quine 1981a, 21)

But Quine's criterion still has a role to play, since by applying it to the theories that science affirms, we determine what, according to science, exists.

Philosophers are not entirely out of the picture, however. Though they cannot transcend science, they can work within science and propose clarifications and

[8] See chapters 15 and 16 in this volume. Jody Azzouni pursues an atypical anti-realist program through rejecting Quine's criterion and arguing that to "quantify over" mathematical entities is not ipso facto to presuppose their existence (Azzouni, 1998).

ontological reductions. It's in this spirit that Quine regards his own proposals for reducing mathematics to set theory. Here again is part of an earlier quote:

> The same motives that impel scientists to seek ever simpler and clearer theories adequate to the subject matter of their special sciences are motives for simplification and clarification of the broader framework shared by all sciences.... The quest of a simplest, clearest overall pattern of canonical notation is not to be distinguished from a quest of ultimate categories, a limning of the most general traits of reality. (Quine 1960, 161)

3.4. Introducing New Objects: Positing

Our focus so far has been on how we justify countenancing various objects. Though one might cite a local conception of evidence as an immediate justification for recognizing an object, ultimately one tacitly appeals to the success of a broader system committed to the type of thing in question. For example, having digested a proof of Cantor's Theorem, we may feel fully justified in countenancing uncountable sets. But this is predicated on our prior acceptance of sets themselves, and of whatever set theory might be needed for carrying out the proof in question and for inferring the existence of uncountable sets. With Quine, we might justify acquiescing in sets by citing their benefits to mathematics. We could in turn justify this by pointing to the importance to science as a whole of having a flourishing mathematics.[9]

Now, what we have observed concerning sets applies to other types of objects—even ordinary physical bodies. You know there's an owl out in the dark, because you heard it screech, and you know an owl's screech. But here you are already presupposing owls and a rich body of beliefs about them, and all these are contained in a framework provided by your beliefs about birds, animals, and physical bodies.

Once we have in place a framework that countenances objects of a given kind, we are in a position to identify and authenticate objects of that kind, whether we do so via observation, instrumentation, or theoretical deduction. But how do we come to add new objects of various types to our ontology in the first place? Even when, according to later theory, we have been observing a type of object with our unaided senses all along, we seem to have a serious problem. For prior to having a framework countenancing the objects in question, we need not even be aware that we are observing any definite thing at all. For example, a layperson looking at the sky on a clear night may be aware of only the stars and the moon, while astronomers will be aware of galaxies, and much more. The difference between the

[9] See, for example, Quine (1981a, 13–16).

layperson and astronomers is that the latter have hypothesized a much richer ontology along with a rich theory of its members' behavior and their observable effects. To introduce an ontology in this way is to *posit* it. But while to posit some things is similar to making up a story about them—at least it is initially—this does not mean that astronomers have created the heavens.

> ... Considered relative to our surface irritations, which exhaust our clues to external physical objects, the molecules and their extraordinary ilk are thus much on a par with the most ordinary physical objects. The positing of these extraordinary things is just a vivid analogue of the positing or acknowledging of ordinary things: vivid in that the physicist audibly posits them for recognized reasons, whereas the hypothesis of ordinary things is shrouded in prehistory....
>
> To call a posit a posit is not to patronize it.... Everything to which we concede existence is a posit from the standpoint of a description of the theory-building process, and simultaneously real from the standpoint of the theory that is being built. (Quine 1960, 22)

Quine no more admits existence by fiat than he does truth by fiat.

This has very important consequences for Quine's philosophy of mathematics. Mathematics, at least on the realist reading of it that Quine favors, is about objects that have no place in space or time, and no effects upon our sensory apparatus. How, one wonders, could we have ever come to have any knowledge of such things? Not through intuition or other a priori insight—at least not if Quine is right. But there is nothing mysterious about our building theories that posit mathematical objects, for theory construction itself requires no contact with the things the theory purports to concern. And, if the theory forms a workable part of our overall system, no matter what its subject may be, then the entities to which it is committed (via Quine's criterion) have as much title to existence as "ordinary things" hypothesized "in prehistory."

4. The Indispensability Argument for Mathematical Realism

Everyone grants that mathematics is very useful to the pursuit of science. It gives science the wherewithal for representing empirical findings through statistical and other numerical means and for explaining these findings using such concepts as those of acceleration, state vector, random mating, allelic frequency, expected utility, and welfare function. Moreover, mathematical laws permit scientists to deduce nonmathematical conclusions from assumptions, such as Newton's laws of motion, that are formulated in a mix of scientific and mathematical vocabulary. Eliminating mathematics would thus drastically alter the practice of working science.

But what if the theoretical purposes of mathematics could be accomplished using a more parsimonious ontology without any reduction in the overall simplicity and economy of the resulting scientific theory? Quine would heartily approve, but he would not ask scientists to stop using mathematics. He would merely claim that since mathematics could be excised from the canonical formulation of science, science (and thus we) should no longer acknowledge its truth or ontological commitments.

> ... not that the idioms thus renounced are supposed to be unneeded in the market place or in the laboratory.... The doctrine is that all traits of reality worthy of the name can be set down in idiom of this austere form if in any idiom. (Quine 1960, 228)

Although Quine attempted to eliminate mathematics from science and applauded efforts aimed at showing that the mathematical needs of science can be reduced, he came to believe that most classical mathematics is indispensable to science (Quine 1960, 270).

Since there is, so far as we know, no way of eliminating mathematics from the "austere idiom" of the canonical formulation of science, we are bound to admit the existence of those mathematical objects that science posits. This argument, which is rooted in Quine's writings and was propounded explicitly by Hilary Putnam, has become known as the Indispensability Argument for Mathematical Objects.[10]

We can formulate a more explicit version of an indispensability argument as follows: First, mathematics is an indispensable component of natural science. Second, thus, by holism, whatever evidence we have for science is just as much evidence for the mathematical objects and the mathematical principles it presupposes as it is for the rest of its theoretical apparatus. Third, whence, by naturalism, this mathematics is true, and the existence of mathematical objects is as well grounded as that of the other entities posited by science. I call this the Holism–Naturalism (H–N) Indispensability Argument. It is clearly based upon principles that Quine accepts, although it is not as clear that it accurately paraphrases his or Putnam's arguments.

Now lots of philosophical energy and talent—including some of Quine's—has been spent trying to undermine the first premise of this argument by showing that mathematics is dispensable from science.[11] More recently, however,

[10] Cf. Putnam: "So far I have been developing an argument for realism along roughly the following lines: quantification over mathematical entities is indispensable for science, both formal and physical: therefore we should accept such quantification; but this commits us to accepting the existence of the mathematical entities in question. This type of argument stems, of course, from Quine, who has for years stressed both the indispensability of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes" (Putnam 1971, 57).

[11] See chapters 15 and 16 in this volume.

philosophers have questioned other aspects of the argument. Neither Penelope Maddy nor Elliott Sober thinks that we can count on science to provide evidence for the truth of mathematics. As we saw earlier, Sober claims that scientific testing fails to confirm the mathematics used in science.

Maddy's criticism is based upon observing that much of the mathematics used in science occurs in theories, such as the ideal theory of gases, that scientists openly acknowledge as false yet still employ. She argues that this raises the possibility that the confirmation coming from membership in scientific theories that are accepted as true covers too little mathematics to be of comfort to mathematical realists. In short, too much of mathematized science may fall outside the scope of the H–N argument's naturalism premise to support mathematical realism (Maddy 1992, 281).

It is not clear how Quine or Putnam would respond to this criticism, for it is not even clear that the H–N argument is theirs. But one can set aside Maddy's worry by moving to another version of the indispensability argument. For whatever attitude scientists take toward their own theories, they cannot consistently regard the mathematics they use as merely of instrumental value. Take Newton's account of the orbits of the planets as an example. He calculated the shape of the orbit of a single planet, subject to no other gravitational forces, traveling about a fixed star. He knew that no such planet exists, but he also believed that there are mathematical facts concerning its orbit. In deducing the shape of such orbits, he presumably took for granted the mathematical principles he used. For the soundness of his deduction depended upon their truth. Furthermore, in using his (mathematical) model to explain the orbits of actual planets, he presumably took its mathematics to be true. For he explained the orbits of planets in our solar system by saying that they approximate the behavior of an isolated system consisting of a single planet orbiting a single star. For this explanation to work, it must be true that the type of isolated system (Newtonian model) has the mathematical properties Newton attributed to it. This illustrates that even when applying mathematics to idealizations or theories they know are wrong, scientists use it in a way that commits them to its truth and ontology.

Reflecting on this leads one to the Pragmatic Indispensability Argument, which runs as follows:

1. In stating its laws and conducting its derivations, science assumes the existence of many mathematical objects and the truth of much mathematics.
2. These assumptions are indispensable to the pursuit of science; moreover, many of the important conclusions drawn from and within science could not be drawn without taking mathematical claims to be true.
3. So we are justified in drawing conclusions from and within science only if we are justified in taking the mathematics used in science to be true.

Notice that, unlike the earlier H–N Indispensability Argument, this one does not presuppose that our best scientific theories are true or even that they are well supported. It applies wherever science presupposes the truth of some mathematics. Thus, as we noted earlier, it applies even to the mathematics contained in those refuted scientific theories that scientists still use and to the mathematics of idealized scientific models. Furthermore, the argument, at least as it stands, contains no claim that the evidence for science is also evidence for mathematics. We can extend this argument to infer that we should acknowledge the truth of mathematics on pragmatic grounds. For given that we are justified in doing science, we are justified in using (and thus assuming the truth of) the mathematics in science, because we know of no other way of obtaining the explanatory, predictive, and technological fruits of science.[12]

Since much standard mathematics is used in science, the indispensability arguments support realism about many parts of mathematics. Yet, as Quine was aware, and Maddy and others have emphasized, indispensability arguments fail to cover the more theoretical and speculative branches of mathematics. Currently science neither needs nor employs this mathematics, and it does not even help in simplifying and systematizing the mathematics that science does apply. Thus it is not part of the Web of Belief, and not connected even indirectly to experience.

5. Ontology and Ontological Relativity

Quine has frequently urged that we take the ontology of mathematics to be one of classes or sets. Numbers, functions, vectors, groups, spaces, and so on are to be reduced to them in the familiar ways.[13] This is because we need to use classes for many purposes, both mathematical and nonmathematical, and we obtain a simpler ontology by having just classes rather than classes plus other mathematical and abstract objects. Here is how Quine argued for countenancing classes in *Word and Object*:

> The versatility of classes in thus serving the purposes of widely varied sorts of abstract objects is best seen in mathematics, but it spills over. . . . Such is the power of the notion of class to unify our abstract ontology. To surrender this benefit and face the old abstract objects again in their primeval disorder would be a wrench, worth making if it were all. But we must remember that the utility of classes is not limited to explication of the various other sorts of abstract objects. The power of the notion on other counts . . . keeps it in continual demand in mathematics and elsewhere as a working notion in its own right. . . . Thus it is that one resolves to keep classes and somehow excise the paradoxes. (Quine 1960, 267)

[12] For further discussion of this argument see Resnik (1997), and for a thorough discussion of indispensability arguments for mathematical realism, see Colyvan (2001).

[13] See Quine (1963).

Here Quine was writing both as a philosopher and as a logician, and advocating that we need no abstract objects in all of science but classes. This sounds like first philosophy, but remember that, on Quine's view, the work of clarifying and reducing the ontological requirements of philosophy and mathematics is the same type of work that scientists in other fields do when they clarify and simplify theories in their home disciplines. The work of Frege, Russell, Quine, and others in unifying the foundations of mathematics using class theory is as much a piece of theoretical science as Von Neumann's reformulation of quantum mechanics using Hilbert spaces.

Some philosophers and mathematicians have argued that mathematics has no need for a reduced ontology. One might read such an argument into some of Hilbert's writings. On this reading, all that mathematicians need are clearly specified axioms for the branch of mathematics within which they are working. It is enough that they be assured that some things satisfy those axioms. Otherwise the nature of these things is of no concern.[14] I am not aware of Quine's responding explicitly to such reasoning, but a response is implicit in the passage quoted above. It would run as follows: Mathematics, even on Hilbert's' view of it, needs some ontology in order to be assured that its axioms are not vacuous. There are too few concrete physical objects to fill the bill. (Ditto for mental objects—if you are so rash as to countenance them.) Thus mathematics requires abstract objects. These are best provided through a unified ontology of classes.

Quine was aware of other proposals for ontological foundations for mathematics. For example, some mathematicians have urged that category theory is a much better vehicle for mathematics than set theory, and that it should take over that role. In *Set Theory and Its Logic* (1963), Quine does mention that category theory is useful for dealing with very large collections. But he does not defend sets and classes against the attacks from advocates of category theory in that book, and I am not aware of any place where he does.

As late as *Theories and Things* (1981c) Quine runs through his usual brief for an ontology of physical objects and sets of physical objects. But then he takes the argument a step further. In the name of simplicity we reduce physical objects to the space–time regions they occupy, and these in turn to sets of space–time coordinates relative to some fixed coordinate system. But space–time coordinates are ordered quadruples of real numbers, and these in turn reduce in familiar ways to sets built up from the empty set, leaving nothing but pure sets (Quine 1981a, 16–18).

[14] Hilbert expressed views similar to this in discussing his axiomatic work with Frege. See, for example, Resnik (1980).

Quine's discussion then turns from this reduced ontology to other ontological candidates generated by reinterpreting our theory of the world while preserving its observational consequences. Here "we... merely change or seem to change our objects without disturbing either the structure or the empirical support of a scientific theory in the slightest" (Quine 1981a, 19). To do this, it suffices that we be able to specify a one-one function—Quine calls it a "proxy function"—which maps each object in the original ontology to an object in the new ontology. Then, using a well-known technique from formal logic, we can reinterpret each predicate of our theory so that it is true of something in the new ontology if and only if it was true (as originally interpreted) of this things inverse under the proxy function.

> The apparent change is twofold and sweeping. The original objects have been supplanted and the general terms reinterpreted. There has been a revision of ontology on the one hand and of ideology, so to say, on the other; they go together. Yet verbal behavior proceeds undisturbed, warranted by the same observations as before and elicited by the same observations. Nothing really has changed. (Quine 1981a, 19)

Quine concludes from this that reference is inscrutable, that is, there is no saying absolutely whether our words refer to this or that, but what they refer to relative to a fixed interpretation. As we move from one ontology to another, our words change their reference, too; yet we have arranged for the truth-values of our sentences—the facts, so to speak—to stay the same. There is no fact of the matter concerning the references of our words—at least in the sense that we can vary their references at will while leaving the facts unvaried. Besides calling this the inscrutability of reference, Quine refers to it as ontological relativity: there is no fact as to what the ontology of a theory is absolutely, but only relative to a means of interpreting it in a language we take at face value (Quine 1981a, 19–20; see also Quine 1969a, 50–54 and 1990, 30–34).

Notice how nicely this fits with Quine's holism. According to holism, it is only relative to a local conception of evidence that we can say that a particular experience verifies or falsifies a particular sentence of some theory. Otherwise, experience bears upon the theory as a whole. Similarly, it is only relative to our parochial ontology and interpretation of our language that we can say that a particular word picks out a particular object of our experience. But doesn't something (e.g., our behavior or the causal relations between our words and the world) fix the references of our words? No. Just as there are many ways to reconcile a theory with a recalcitrant experience, so there many candidate references for our words, and no way to single out one. After applying a proxy function, "verbal behavior proceeds undisturbed, warranted by the same observations as before and elicited by the same observations. Nothing really has changed" (Quine 1981a, 19).

Of course, if we take our home language at face value, then we can simply single out an intended interpretation, using our own words to do so. Thus we simply state: the intended model of the Peano Axioms is the natural number sequence. So long as we don't question the reference of "the natural number sequence," we have no trouble distinguishing the standard model from, say, the sequence of even natural numbers or in ruling out an interpretation that construes the successor function, $s(x)$, as $x+2$. But once we question the references of our own words, nothing precludes the phrase "the natural number sequence" from referring to the even number sequence, the finite Von Neumann ordinals, or any progression.

These considerations led Quine to put an emphasis on the logical structure of a theory, since this is what remains invariant under applications of proxy functions. As Quine put it, "structure is what matters to a theory, and not the choice of its objects." The objects "serve merely as indices along the way, and we may permute or supplant them as we please as long as sentence-to-sentence structure is preserved" (Quine 1981a, 20; see also Quine 1990, 31).

Yet after presenting these ideas Quine was moved to reaffirm his realism with respect to ordinary bodies and the theoretical entities of science and mathematics. As a naturalist, he is committed to the ontological commitments of current science. As to his previous reflections that seem to belittle objects, Quine points out that these belong not to ontology but to "the methodology of ontology and thus to epistemology." The considerations showed that we could turn our backs upon external things and classes and "ride the proxy functions to something strange and different without doing violence to any evidence. But all ascription of reality must come rather from within one's theory of the world; it is incoherent otherwise" (Quine 1981a, 21).[15]

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[15] Quine (1990) also emphasizes the primacy of structure but, interestingly, this book contains no brief for a mathematical ontology consisting of just sets. However, Quine (1995) does (cf. pp. 40–42). Furthermore, Quine (1992) reaffirms a mathematical ontology of classes while also emphasizing ontological relativity.

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