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28. Philosophy of Mathematics: Why Nothing Works

Instead of describing the main trends in the philosophy of mathematics, I have decided to begin with the theme "why nothing works." But I am not simply being perverse: explaining why and how it seems that "nothing works," that is, why and how it is that *every* philosophy seems to fail when it comes to explaining the phenomenon of mathematical knowledge, *will* involve saying something about each of the "main trends." And it will also, paradoxically, serve to point out why philosophy of mathematics is such a crucial field. If there is such a thing as "philosophical progress"—and I confess to an unregenerate faith that this is not a chimera—then it comes from focusing the attention of philosophers on areas and problems where their pet ideas run into trouble, areas where "nothing works." We will not get beyond the present philosophical discussion by arguing one more time whether or not there are "canons of scientific method," or by dragging out the familiar examples from the history of physics; we may get to a new plateau by taking seriously the idea that there are real problems for *all* the standard views—and no area is more likely to make us aware of this than the philosophy of mathematics.

Logicism

Logicism—the view that "mathematics is logic in disguise" and that *that* is what accounts for its certainty—appears to be defunct as far as having present adherents goes. However, it is often overlooked that something of permanent value came out of logicism. I shall not repeat the objections to logicism here—they are too well known to need repetition, I believe—but instead I shall make the following observation:

Since the work of Frege and Russell,[1] we are all much more aware of how much mathematics can be done in logistic systems which are set up to codify deductive logic. Those philosophers who would count second-order logic as logic—and there are some—would hold that *all* of standard mathematics can be formalized within logic (for it can be formalized within second-order logic),[2] even though they would not claim that this makes the epistemology of mathematics any easier (perhaps it makes it harder!); while even if we follow what seems to be the majority fashion, and limit the term "logic" to first-order logic, we still have to recognize that a good deal of what any mathematician would recognize as "mathematics" can be coded into "logic." For example, the whole first-order theory of groups is a fragment of first-order logic. Perhaps all analytic philosophers now recognize that "the nature of logical truth" and "the nature of mathematical truth" are *one* problem, not two—and this is itself a victory for the standpoint of Russell, whose most moderate conclusion was that henceforth it would never be possible to draw a sharp line between logic and mathematics. (Russell changed his view a number of times; but he did not originally believe that "reducing" mathematics to logic showed that mathematics was analytic—unlike Frege, who did believe this, although he employed a notion of analyticity different from Kant's.)

Logical Positivism

For a number of years the logical posivitists made fashionable the view that held that mathematical truths are true simply by virtue of "rules of language." If we take a "rule of language" to be anything like a convention (taking the model for a convention to be something laid down by explicit stipulation), then the view runs into a problem pointed out independently by both Wittgenstein[3] and Quine: the truths of logic (and mathematics) are infinite in number. So "logical truths are true by convention" cannot mean that they are *individually* true by convention (one act of stipulation per logical truth); it can only mean that they *follow* from conventions—that is, that logical (and mathematical) truths are true by convention in the sense of being the *logical consequence* of conventions. But the use of the notion of *logical consequence* makes such an account of logical truth viciously circular.

In order to avoid this difficulty, Wittgenstein seems to have held that the model of convention has to be replaced by the model of a bare behavioristic practice, a "form of life." But what could the practice *be?* In order to support his view that logical and mathematical truths have *no* descriptive content, the practice would have to be holding certain truths absolutely immune from revision. While such an account overcomes the previous objection (because a practice, like a habit, can be *general*—holding infinitely many truths immune from revision can be the result[4] of finitely many habits), it seems to be a distortion of actual mathematical practice. Consider the statement that a logistic system is *consistent,* for example. We do *not* hold such a statement absolutely immune from revision. In fact, no matter how good my "proof of consistency" may be, I will give up the claim that the system is consistent if I actually *derive a contradiction.* Thus the *observation that a calculation actually has a certain result* has a kind of "brute fact" character (somewhat analogous to the character of an observation statement in empirical science) which enables it, in certain circumstances, to overthrow (or force modification in our statement of) even the best entrenched general principles. The fact is that there is a certain "synthetic" element in at least *combinatorial* mathematics, and it is the failure of "rule of language" accounts to acknowledge this that ultimately makes them simply unbelievable. However, once we grant that there is at least *one* mathematical fact which is not simply our stipulation (nor yet our "form of life")—that, for example, the *consistency* of our stipulations/practices is not itself just another stipulation or practice—then logical positivist/Wittgensteinian accounts of logical and mathematical truth are seen to be bankrupt.

[*Added in 1993: This is not my present reading of Wittgenstein, for which see Chapter 12 of the present volume.*]

Formalism

The formalists did not really attempt to provide an epistemology of logic, or even an epistemology of finitist combinatorial mathematics. Hilbert seems to have thought the former was too evident to *need* an epistemology while the latter dealt with properties of concrete objects (marks on paper) that were likewise so evident that little or no epistemological discussion was needed. It was set theory and non-constructive mathematics that needed some kind of commentary, and the commentary Hilbert provided would have it that these are just "ideal" (and in themselves meaningless) extensions of "real" (finite combinatorial) mathematics.

While there are many objections to formalism that are well known (and they will not be reviewed here), there is one difficulty that is not well known and that deserves a brief exposition.

This difficulty arises from the fact that the locutions of set theory are employed in empirical as well as mathematical statements. Suppose I say, for example, "There are just as many stars in galaxy A as in galaxy B." On the most natural reading, this means that *there exists a one-to-one correspondence between the stars in galaxy A and the stars in galaxy B*. Indeed, suppose the statement is true. Then on a realist conception of truth, there has to be something that makes it true—and the obvious candidate for the "something" is just the *one-to-one correspondence* (or any one of them, since there will be many). But if such objects as sets, relations, one-to-one correspondences, and so on, are just *fictions,* then this "something" (or these "somethings") don't really exist. So there *isn't* really "something" that makes the statement true, after all. So how can it *be* true?[5]

In short, the formalist seems to be really a kind of philosophical nominalist—and nominalism is (it is generally believed) inadequate for the analysis of empirical discourse. Even so simple a statement as "The distance from electron A to proton B is *d*" would appear difficult for a nominalist/formalist to explicate. If *numbers are marks on paper,* then does the statement just mentioned say that two elementary particles stand in a certain relation to some marks on paper? *What* relation? (An extreme operationist might not be embarrassed by this question, but the difficulties with operationism in the philosophy of empirical science are decisive. And they will be inherited by formalism if the formalist takes an operationist tack.)

Platonism

In view of the difficulties with formalist and logical positivist accounts, it is not surprising that there has been a certain revival of *realism* in the philosophy of mathematics as well as in the philosophy of empirical science. This revival is also, in part, a product of a certain feeling that formalist and logical positivist accounts have little to do with actual mathematical practice. Hao Wang expresses this very well in his *From Mathematics to Philosophy.*[6] To the working set theorist it does not seem at all that the axioms of set theory (the axioms which describe the so-called iterative conception of set), including replacement and choice, are in any way either "meaningless" or mere "conventions." The more one works in set theory, the more it seems that these axioms are *forced upon one,* as Wang puts it. The problem (as he recognizes) is to come up with an account that justifies this feeling that particular axioms are "forced."

The most straightforward realist account is the one advanced by Kurt Gödel.[7] According to Gödel there really are mathematical objects, and the human mind has a faculty different from but not totally disanalogous to perception with the aid of which it acquires better and better intuitions concerning the behavior of the mathematical objects.

The trouble with this sort of Platonism is that it seems flatly incompatible with the simple fact that we think with our brains, and not with immaterial souls. Gödel would reject this "simple fact," as I just described it, as a mere naturalistic prejudice on my part; but this seems to me to be rank medievalism on *his* part. One does not have to be an "identity theorist" in the philosophy of mind (that is, one who holds that sensations, intuitions, and perceptions are identical with brain events) to recognize the difficulties with the kind of dualism that Gödel believes in. We cannot envisage *any* kind of neural process that could even correspond to the "perception of a mathematical object." And if we hold that mental events (such as the event of "intuiting" a new mathematical fact) may not even correspond to brain events (which is Gödel's position, as I understand it), then how do we account for the large role the brain is *known* to play in *ordinary* perception, not to mention *memory,* speech processing, and so on? The idea that the brain is a cybernetic device which stores information, computes from that information, and controls the body—all without interference from a mysterious "soul"—is based on a vast amount of progress in a half-dozen sciences. To say, as I did, that we "think with our brains," does *not* seem to me to be foot-stamping naturalism after three hundred years of progress in physics and biology (not to mention more recent sciences). To me it seems that Gödel is trying to escape into traditional ideas because of the difficulty of coming up with new ones which will account for the phenomenon of mathematical knowledge; but I cannot believe that the solution *will* come in such a return to the past.

Holism

The argument against formalism that I gave above—that formalism/nominalism is inadequate for the ontological needs of *empirical* science—is not in Quine's work in quite the form in which I stated it, but it is in the spirit of much of Quine's writing.[8] Quine has all along contended that mathematics has to be viewed not all by itself but rather as a part of the corpus of total science, and that the necessity for quantification over mathematical objects (Quine would say: quantification over *sets*) if we are to have a language rich enough for empirical science is the best possible reason for taking the "posit" of sets exactly as seriously as we take any other ontological "posit"—say, the posit of material objects. Sets and electrons are alike for Quine, in being objects *we need to postulate* if we are to do science as we presently do it. Perhaps we will find some other way to do science in the future; but then we can change our philosophy as our science changes.

This sort of holistic pragmatism is attractive in that it (1) recognizes what the logicists were for a long time alone in recognizing, that we must account for the use of mathematical locutions in empirical statements, not only in statements of pure mathematics, and (2) provides a good reason for being a realist about the existence of sets without postulating mysterious immaterial souls, or mysterious faculties of *perceiving* sets or other mathematical objects; but upon closer examination it too runs into serious difficulties. Quine seems to be saying that science *as a whole* is one big explanatory theory, and that the theory is justified *as a whole* by its ability to explain *sensations*. Even if we think that Quine's own reductionism (that is, his insistence that all mathematical objects *must* be identified with *sets*) is not really entailed by his holism, but is an independent view of his, the idea that what the mathematician is doing is contributing to a scheme for explaining *sensations* just doesn't seem to fit mathematical practice at all. What does the acceptance or non-acceptance of the Axiom of Choice (or of a known-to-be-consistent but *not* accepted principle like the axiom "V = L" that Gödel once proposed but later gave up) have to do with explaining sensations?

Quasi-Empirical Realism

It seems to me that Quine's account is too attractive simply to jettison, in spite of the difficulty just pointed out, because it does indicate a direction in which one can move if one wishes to be a realist without being a metaphysician. Thus I once tried to develop an account which might be called "quasi-empirical realism" (in "What Is Mathematical Truth?"). There are two main modifications which this account makes in the holist story.

The first modification is to add *combinatorial facts* to *sensations* as things we wish mathematical theorems to explain and to subsume under general "laws." The principle of mathematical induction, for example, bears the same relation to the fact that when a shepherd counts his sheep he always gets the same number (if he hasn't lost or added a sheep, and if he doesn't make a mistake in counting) no matter what order he counts them in, that any generalization bears to an instance of that generalization. (That a finite collection receives the same "count" no matter what order it is counted in is *equivalent* to the principle of mathematical induction.) People have the capacity to notice combinatorial facts and the capacity to generalize them. If empirical science is, as Quine says, a "field with experience as its boundary conditions," then why should we not view mathematical science as a field with combinatorial facts which can actually be noticed by the calculating mind (or brain) as *its* boundary conditions?

This suggestion does *not* commit one to Mill's view that such a principle as the principle of mathematical induction is known to be true by *Baconian* "induction."[9] For, as Wittgenstein points out, Mill's account (Wittgenstein does not mention Mill by name, but he clearly has him in mind) may be correct as a description of how we first came to believe some form of a mathematical truth (for example, that the cardinal number of the sheep doesn't depend on the order in which we count them), without being correct as a description of the *present status* of that truth. Yet as Quine points out, we can recognize that such a principle as the principle of mathematical induction has a special status—that it would take something virtually unimaginable to cause us to revise it (such as discovering a contradiction in the first-order theory of natural numbers?)—without conceding that the status is the status Wittgenstein calls being a "rule of description" (that is, being *analytic,* though Wittgenstein doesn't use the term). A sophisticated "quasi-empirical realist" can grant that mathematical truths attain the status of being "*a priori* relative to our body of knowledge," as some physical laws do, without conceding that *that* status is the same as the logical positivists' "rule" status. We can be empiricists without being either Millians or positivists.

The idea that there is something *analogous* to empirical reasoning in pure mathematics has also been advanced by Lakatos and even by Gödel, who is much too sophisticated to think that acts of "perception" are *all* that is involved in mathematical "self-evidence," "plausibility," and so on. The fact that two philosophers as radically opposed on fundamentals as Quine and Gödel have both been led to recognize the presence of such an element—an element which resembles "hypothetico-deductive" reasoning in empirical science—in pure mathematics is certainly striking and suggestive.

Quine recognizes that even in empirical science there are considerations other than predicting sensations which play an important role. He speaks of "conservatism"—the desire to preserve principles that have long been "central" to the "field"—and of "simplicity," which occasionally makes us fly in the face of "conservativism" when a radical change at the center leads to far-reaching simplifications of the whole system.

The second modification I propose to make in Quine's account is to add a third non-experimental constraint to his two constraints of "simplicity" and "conservatism." (Of course these are not really *single* constraints.) The constraint I wish to add is this: *agreement with mathematical "intuitions,"* whatever their source.

On my view, mathematical "intuitions" are not mysterious "perceptions" of mathematical objects, nor do they have a single source. The Mill-Wittgenstein story—that mathematical induction (in the form of the "sheep-counting principle") started out as a Baconian induction and was elevated to a different status along the line—seems right for mathematical induction, but not for set theory. Quine himself gives a plausible account of the origin of the "self-evidence" of the comprehension axioms of set theory (these say that every condition determines a set, if we ignore the problem of avoiding the Russell Paradox). In *Ontological Relativity and Other Essays,* Quine points out that quantification over predicate letters occurs in natural language quite unconsciously as a mere device for avoiding awkward repetition of whole predicate expressions. In effect, the use of what Quine calls "virtual" classes (that is, class abstracts which can easily be *eliminated* from discourse) leads automatically to quantification over predicates (which commits us to at least *predicative* set theory); and quantification of predicates leads to precisely one of Cantor's two notions of a set: *the extension of a predicate.* The fact that the origin of the idea that every condition determines a set may have been something as mundane as everyday linguistic habits of avoiding the repetition of long expressions does not mean that the existence of sets must be questioned even after we have erected a successful theory (which, we hope, avoids the paradoxes). "To the enlightened mind, illegitimacy of origin is no disgrace," Quine wryly comments.

Quasi-empirical realism, if it succeeded, would have two striking virtues: (1) the virtue that "intuitions" can be *explained* (no *monolithic* notion or mysterious faculty); and (2) the virtue of directing attention not only to the various reasons for which and processes by which new axioms are adopted in mathematics (a remarkably neglected topic!), but also to the various forms of "plausible reasoning" short of proof that occur in mathematics (a topic Polya was extremely interested in).

I said at the beginning of this report that "nothing works." This applies, alas, to my own ideas—which isn't to say that I propose to give them up, I hasten to add, but to say that I see great difficulties which show that this can't be the *solution* to the problem of mathematical knowledge, even if it is right as far as it goes, as I believe it is.

The problem is that it is totally unclear what satisfying *this* sort of non-experimental constraint—agreement with "intuitions" whatever their source—has to do with *truth*. Having accepted the stance of realism—which means that we do regard mathematical statements as true or false—and having given a description, however vague, of how mathematical statements come to be accepted, we cannot duck the question: what is the link between *acceptibility* and *truth?* Gödel's mysterious "perceptions" would at least constitute such a link; it is not clear how mathematical "intuitions" do, if at bottom they are just generalizations from the finite on the basis of human psychology, reified forms of grammar, and so on.

"To the enlightened mind, illegitimacy of origin is no disgrace," Quine says. *Why isn't it?* Presumably because adult performance—which, in the case of set theory, means *utility for physics*—is what we judge by, not "origin." But if "origin" is no justification, if only utility for physics—or, ultimately, for explaining sensations—counts, then set theory is just as good *without* the Axiom of Choice, or, alternatively, *with* "V = L." Quine, apparently, would not be disturbed by such a relativist conclusion, but any working set theorist would. We are back with the unsatisfactory version of "holism" discussed in the previous section if we do *not* regard conformity to our intuitions as something of *methodological* significance, and not just psychological significance; we are stuck with a serious epistemological worry—how to explain its methodological significance—if we do so regard it.

Modalism

One objection to Platonism has always been the strangeness of postulating a universe bifurcated into two sorts of entities: physical things and "mathematical objects" (the modern equivalent of Plato's Forms). But the mathematical realist is not really committed to *this* sort of Platonism, with its attendant problem of how we can succeed in thinking about and referring to entities we can have no *causal* transactions with. As I pointed out some years ago[10] and as Charles Parsons has recently pointed out,[11] we can reformulate classical mathematics so that instead of speaking of sets, numbers, or other "objects," we simply assert the *possibility* or *impossibility* (in the sense of *mathematical* possibility or impossibility) of certain structures. "Sets are permanent possibilities of selection" was the slogan. The structures whose possibility or impossibility is talked about can themselves be predicates of physical objects, or predicates of unspecified objects, or even—if one has nominalistic scruples against admitting even first-order properties into one's ontology—concrete things. Mathematics, on this view (which I called "mathematics as modal logic"), has a special *notion*—the notion of possibility—but no special *objects*. While "modalism" has therapeutic virtues (it explains how mathematics is possible without assuming Plato's Heaven), and while Parsons and I both believe it can shed light on the so-called iterative conception of set, it does not speak to the *epistemological* problem. If we give a "quasi-empirical realist" account of how we *know* modal facts, then the problems will be just the same whether we accept the "mathematics as modal logic" picture or the "mathematics as theory of mathematical objects" picture. Once again, "nothing works."

Intuitionism

Since formalism doesn't work and, on the other hand, the various versions of realism we have considered run into apparently insuperable epistemological problems, it may be worthwhile to reconsider intuitionism, which accepts mathematical statements as meaningful while rejecting the realist assumptions about truth (for example, "bivalence"—every statement is true or false) which I have so far presupposed.[12] But there are at least three difficulties with intuitionism: First, intuitionism is apparently an extension of operationism to mathematical language, in content if not in historic origin, and presupposes that *non*-mathematical language can be analyzed in an operationist or verificationist way. The difficulty is not that intuitionists cannot derive the theorems of enough mathematics to "do" physics—Bishop has convincingly shown that they can[13]—but that the interpretation of the logical connectives assumed by intuitionism doesn't "fit" a non-operationist physics. For example, ⊃ ("if-then") is interpreted by intuitionists as meaning that there is a procedure for going from a *proof* of the antecedent to a *proof* of the consequent. While the assumption that there are such things as verifications ("proofs") of isolated statements may be all right in mathematics, it is not in physics, as many authors have pointed out. So what does ⊃ mean in an *empirical statement?*

Second, intuitionists assume a distinction, familiar from phenomenology and neo-Kantian philosophy, between *empirical* facts about the mind and transcendental or *a priori* facts about the mind. For example, the statement "Every number has a successor" does not mean (when interpreted by an intuitionist) that it is actually possible for the empirical mind to "construct" arbitrarily many numbers; it means, roughly, that it is not *a priori* impossible to construct arbitrarily many numbers (and that *this* fact is itself phenomenologically "evident"). To empiricists like myself this appeal to *a prioricity* and to mysterious phenomenological "evidence" is as objectionable as Platonism. Aren't the intuitionists just saying after all that it is *self-evident* that arbitrarily long finite sequences are *possible?* And how is *this* claim *either* an analysis of what possibility comes to *or* an account of the faculty by which we know these *"a priori"* truths?

Third, the problem of the *consistency* of the mind is shrugged off by intuitionists just a little too easily. If there is such a thing as the transcendental structure of the mind, why couldn't it be inconsistent? "It's *evident* that it isn't" is hardly an answer.

What Directions Should Be Pursued?

After this depressing survey of "why nothing works," you may expect me to recommend *giving up* on philosophy of mathematics (and perhaps on philosophy altogether). But it seems to me that while things are dark they are not altogether hopeless. I have already said that there is something that seems to me worth pursing in "quasi-empirical realism" as an approach to philosophy of mathematics, for example. The epistemological difficulty I pointed out might be met, I think, by pointing out that "truth" cannot be taken on the old "transcendental realist" model (as Kant called it) for many reasons. If we instead think of truth as *ultimate goodness of fit,* in a phrase due to Ullian, then the connection between our *criteria* of fit, even if they be partly aesthetic, and *truth* may not appear quite so mysterious. To be sure, this is a program of work and not a "solution." One thing one has to see, if this program is right, is that a simple copy theory of truth is also wrong in empirical science, and that partly "aesthetic" criteria of truth enter there too. Even more important, while one's first reaction to such an account of mathematical truth (let alone empirical truth) may be that it leads back to Quinian relativism, it will turn out, I believe, that the more one works with mathematics and set theory, the less often it will seem the case that there is a genuine "choice" of which system to accept. Wang's observation that the axioms seem to be "forced upon us" may yet find its explanation.

Such a view has affinities with several of the positions I have discussed. Its indebtedness to Quine's holism is obvious. It has an affinity to Intuitionism in abandoning the idea that truth is independent of even ideal verification, but it gives up the idea of fixed *a priori* structures in the mind. It accords well with modalism and with the logicist insistence that our account of logical and mathematical truth be unitary. Perhaps it is even "Wittgensteinian."

Nor need such a view fall into mere idealism or phenomenalism. It is not being denied that there is a "real world" nor affirmed that all there "really is" is sensations and their relations (sensations too are part of "the web of belief"). What is rather being claimed is that knowledge is necessarily a representation of the world, not a Doppelgänger of the world, and that any representation must be the joint product of the world and human psychology (or Alpha Centaurian psychology, or Betelguesian psychology, or . . .). Thus my view is a soft Kantianism—Kantian in insisting on the mind-dependence of all knowledge, but empiricist in rejecting Kant's distinction between the transcendental and the empirical mind and the unrevisable "synthetic *a priori*." Far from being antirealist, it is my claim that such a view is even compatible with a certain version of a correspondence theory of truth—though not one which supports full bivalence. But that goes far beyond this report.

While this approach too may "not work," it seems clear that what is needed in philosophy of mathematics is work that is *philosophical* and not primarily technical.[14] Investigations in the philosophical foundations of intuitionism, investigations in the history of mathematics which shed light on the processes by which mathematics grows and changes, and investigations into "plausible reasoning" in mathematics are among the areas which invite study. Above all, philosophy of mathematics, like philosophy of science generally, must link up with philosophy of language, and especially with the discussion of the deep metaphysical issue of realism as a theory of truth and reference.

Notes

1. Gottlob Frege, *Foundations of Arithmetic*, trans. J. L. Austin (Oxford: Basil Blackwell, 1950), and Bertrand Russell, *Introduction to Mathematical Philosophy* (London: G. Allen, 1919), and *The Principles of Mathematics* (London: G. Allen, 1903).
2. Frege—and Russell-Whitehead—needed at least third-order logic, but different "translations" are known which translate all of set theory into second-order logic (in the sense that each set theoretic statement is true *if and only if* its "translation" into second-order logic is *valid*).
3. Ludwig Wittgenstein, *Remarks on the Foundations of Mathematics*, ed. G. H. von Wright, R. Rhees, and G. E. M. Anscombe, trans. Anscombe (Oxford: Basil Blackwell, 1956).
4. Strictly speaking, the notion of "result" *is* just the notion of *logical consequence* in disguise, and the difficulty mentioned is *not* avoided. On the other hand, if "results" means what the habit would *actually* lead us to do, then no circularity is involved, but references to the actual infinite *or even the very large finite* have to be construed as a sort of mathematical fiction. This is, I believe, Wittgenstein's stand. To ask if a logistic system is *consistent* is not always meaningful from such a hyper-finitist standpoint. But what of the statement that *there is no contradiction with fewer than* 10^{100} lines? This is true or false, even from a hyper-finitist standpoint, and *not* just a question of our "form of life."
5. In "Steps to a Constructive Nominalism," *Journal of Symbolic Logic*, 12 (1947), 97–122, Goodman and Quine proposed a nominalist analysis of "there are exactly as many cats as dogs." But their analysis not only was extremely artificial, but involved the notion "bigger than" (in volume) which is not easily explainable without reference to volume elements, *counting*, and so on.
6. Hao Wang, *From Mathematics to Philosophy* (London: Routledge and Kegan Paul, 1974).
7. Kurt Gödel, "What is Cantor's Continuum Problem?" *Philosophy of Mathematics: Selected Readings*, ed. Paul Benacerraf and Hilary Putnam (Englewood Cliffs, N.J.: Prentice-Hall, 1964), pp. 258–273.
8. See especially *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969) and "On What There Is," *Review of Metaphysics*, 2 (1948), 21–38, reprinted in *Ontological Relativity*; "Truth by Convention," in *Philosophical Essays for A. N. Whitehead*, ed. O. H. Lee (New York: Longmans, 1936), reprinted in his *The Ways of Paradox* (Cam-

bridge: Harvard University Press, 1976); and "Two Dogmas of Empiricism," *Philosophical Review,* 60 (1951), 20–43, also reprinted in *Ontological Relativity.*

9. J. S. Mill, *A System of Logic Ratiocinative and Inductive* (London: Longmans, Green, 1961; originally published in 1843).
10. In "Mathematics without Foundations," *Journal of Philosophy,* 64 (1967), 5–22; reprinted in my *Philosophical Papers,* vol. 1, *Mathematics, Matter, and Method* (New York: Cambridge University Press, 1975), pp. 43–59.
11. Charles Parsons, "What Is the Iterative Concept of a Set?" in *Proceedings of the Fifth International Congress for Logic, Methodology, and Philosophy of Science,* ed. R. E. Butts and J. Kontikka (Dordrecht: D. Reidel, 1977), pp. 335–367.
12. See particularly Michael Dummett, *Mathematical Intuitionism* (Cambridge: Cambridge University Press, 1977).
13. E. Bishop, *Foundations of Constructive Analysis* (New York: McGraw-Hill, 1967).
14. I am not denying that *some* technical work is highly philosophically relevant (for example, the work of Georg Kreisel and that of Harvey Friedman), but most technical work by philosophers of mathematics is of a low quality *and* of doubtful philosophical significance.