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HOLISTIC REALISM: A RESPONSE TO KATZ ON HOLISM AND INTUITION

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In his book, *Realistic Rationalism* (henceforth *RR*), and his article, "Mathematics and Metaphilosophy"(henceforth MM), Jerrold Katz develops and defends a philosophy of mathematics that is realist in ontology and rationalist in epistemology.[1] On his view, mathematics contains a body of necessary truths about certain abstract entities that are known a priori though the exercise of reason. Katz believes that his epistemology answers the familiar challenge, so well articulated by Paul Benacerraf,[2] to mathematical realists that they explain how beings like us can acquire knowledge about causally inert, nonspatial, nontemporal, mathematical objects. Katz thinks the key to responding successfully to this challenge is to develop a "no contact epistemology," that is, one that posits no physical process connecting us to mathematical objects. By basing his account of mathematical knowledge upon reason and intuition, Katz believes that he has avoided mysticism, such as that associated with Plato's epistemology of recollection, and has given a deeper account than, say, Frege's view that we grasp or apprehend abstract objects[3] or Godel's brief comparison of mathematical intuition with ordinary sense perception.[4]

Katz thinks that the only other no contact epistemology worthy of consideration is W. V. Quine's confirmational holism. But he argues that this falls prey to his own Revisability Paradox and is inconsistent (*RR*, 72–4; MM, 375).

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[1] Jerrold Katz, *Realistic Rationalism*. Cambridge, MA: MIT Press, 1998; Jerrold Katz, "Mathematics and Metaphilosophy," *Journal of Philosophy* 99 (2002): 362–90.

[2] Paul Benacerraf, "Mathematical Truth," reprinted in *Philosophy of Mathematics*, 2nd ed., ed. Paul Benacerraf and Hilary Putnam. Cambridge: Cambridge UP, 1983; pp. 403–420.

[3] Gottlob Frege, "The Thought," trans. A. Quinton and M. Quinton, *Mind* 65 (1956): 289–311.

[4] Kurt Godel, "What Is Cantor's Continuum Problem," *Philosophy of Mathematics*, 2nd ed., ed. Paul Benacerraf and Hilary Putnam. Cambridge: Cambridge UP, 1983: 483–84.

We think that the Revisability Paradox raises interesting questions for systematic views concerning rational belief revision, but we shall argue that it does not vitiate Quinean holism. We shall also criticize Katz's response to Benacerraf's challenge. To be sure, the best way to answer it is to develop a no contact epistemology for mathematics, but we do not see how Katz's account has gone significantly beyond those he criticizes in either its clarity or lack of superficiality. We will begin our discussion with the Revisability Paradox.

THE REVISABILITY PARADOX

Exposition of the Paradox

According to Katz, Quine's epistemology is constituted by three fundamental principles: The law of noncontradiction (LNC), which enjoins us to revise our belief systems when they fall into contradiction, the principle of universal revisability (UR), which permits us to revise any part of a system, and the principle of simplicity, which tells us to revise so as to obtain the simplest result. Katz sees this as generating the following problem: "Since the constitutive principles are the premises of every argument for belief revision, it is impossible for an argument for belief revision to revise any of them because revising any one of them saws off the limb on which the argument rests. Any argument for changing the truth value of one of the constitutive principles must have a conclusion that contradicts a premise of the argument, and hence must be an unsound argument for revising the constitutive principle." Thus Quine's epistemology is "inconsistent," since some statements, namely, those constituting the epistemology, are not immune from revision (*RR*, 73).

Before we proceed further some clarification is in order. First, Katz sees theory and belief revisions as ensuing from arguments to the effect that our theories must be modified in certain ways. But it seems more appropriate to think of these revisions as resulting from decisions to implement them. The model to use is not that of deduction but rather that of deliberation. On this model, the principle that our system should be consistent entails that we must revise it when it is not. It generates occasions for revision, and puts us into the deliberative mode. The principle that no statement is immune to revision defines the set of options for us to consider. The various virtues or marks of good theories (of which simplicity is just one) help us to determine whether a proposed revision is acceptable. Still, Katz's point survives, because if asked to justify our revision, we would produce an argument to the effect that we had to revise (by noncontradiction); our choice was among the options (by universal revisability); and it was an acceptable option (by the virtues).

Second, in most cases no revision (or new system) will emerge from our options as the uniquely best choice. It is likely that the options are not even partially ordered by some relation for comparing their virtues, and, hence, that none will be optimal, much less the best. We may only be able to claim that our choice is among those that are acceptable.[5]

Finally, other tensions besides inconsistency may prompt revising a system. For example, we might revise our beliefs simply because we judge their implications to be too improbable. Thus we might come to believe that a black bear is roaming our neighborhood, because someone claims they saw one there. If an extensive search using dogs, heat sensors, and so forth fails to reveal traces of one, we are likely to revise our beliefs, despite their consistency with our newly acquired beliefs about the unsuccessful bear search. Furthermore, we sometimes revise a theory just because we have figured out a way to make it better. A biologist, for example, may be able to improve upon a theory accounting for a trait by postulating one gene rather than two interacting ones.

Of course, none of these points affects the basic worry that no matter what our epistemology's constitutive principles are, we cannot give a sound argument for revising them, and so no epistemology of belief revision can apply to itself. Katz may have taken aim at Quinean holism, but if he has succeeded at all, he has brought down more than one quarry. We have a general argument to the effect that no epistemology of belief revision is revisable since revising it would require an argument using its own principles as premises.

We should also take note of a variant of Katz's paradox that highlights the principle that no statement is immune from revision. Katz writes, "Looked at from the right angle, universal revisability already flashes the signal Paradox! Paradox! Paradox! Unrestricted universal revisability sanctions the dangerous move of self-application, which is a familiar feature of Paradox. From the applicability of the belief-revision epistemology to itself, it follows that a revisable principle is unrevisable" (*RR*, 74).

Notice that Katz is not maintaining that it is the application of Universal Revisability to itself alone that leads to paradox. This is to the good; for this self-application would not entail that the principle is false, perhaps not even that it might be false, but only that it is revisable. Rather it is the previous result that the principles of an epistemology of belief revision are themselves unrevisable that is supposed to entail that the Quinean epistemology is committed to holding that an unrevisable principle is revisable.

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[5] Isaac Levi, *Hard Choices*. Cambridge: Cambridge UP, 1986.

A First Resolution—Katz's Argument Is Flawed

Suppose that we have an argument with P, one of the principles of belief revision as a premise that concludes that we should revise P. This is not to conclude that P is false or ~P. So contrary to Katz, such an argument need not be unsound. Of course, if we implement its conclusion we will come to believe that P is false. So we will come to see the argument as unsound.

Does this differ from falsification in science? Using and believing T we predict P. We observe that ~P, so we conclude that T is false. We can still see that there is a valid argument for "if T then P." We just take our original argument as a conditional proof. Similarly, we see the original argument with P as its premise as an argument to the effect that if P then anyone who believes P should not believe it. As a result of our original argument we may no longer believe P, but this could be a kind of justification for us of our belief that ~P. We already have good reasons for believing that the Earth is not flat. If we learn that even if the Earth is flat, no one would be justified in believing it, how could that cause us to doubt that the Earth is not flat?

So it does not look like Katz's general argument works. We can have an argument, A, based upon some of the constitutive principles for revising one of the others. It will not be an argument that the principle is false, but rather one that it should be revised. After we reject that principle we can regard A as an argument that if the principle were true, then anyone who believed it should revise that belief. If we had an argument using the principle as a premise that it was false, we could turn this into an indirect proof of the principle's negation. Either way it does not look like we are bound to maintain our principles of belief revision no matter what.

Let us look at how this works out in the Quinean case. Suppose that X = {UR, S, W, . . .} is the set of our current beliefs, and it implies a contradiction. Then by LNC we must reject one or more members of this set. By UR we may reject any member. Suppose that rejecting UR itself in favor of UR' produces the best revision. Does this make the argument for revising UR fail?

The argument would run:

1. X implies a contradiction, UR belongs to X, and revising UR in favor of UR' yields the best consistent revision of X, and LNC and UR and a principle requiring us to accept the best belief revision (AB). Premise
2. Some member of X must be revised   By 1, using LNC
3. Revising any member of X is an option   By 1, using UR
4. Revising UR is an option   By 1, 3

5. Revising UR in favor of UR' is the best option By 1
6. So UR' must replace UR in our belief set. By 1 and 5, using AB.

Because we are assuming that UR does not forbid its own revision, the argument will look sound prior to the revision of UR. Now suppose that UR' allows for the revision of any of our beliefs (or at least of UR), then the argument would also be judged sound after revising UR as UR'. A problem would arise only if UR' prohibits the revision of UR! Even so, we could regard the argument as one to the effect that if UR is true, then we should believe UR'. This is a consistent though somewhat perplexing result. We would come to revise our belief that UR and believe UR', yet UR' tells us that if we still believed UR, we should not revise that belief! Furthermore, after revising our belief in UR we could come to regard our argument for revising UR as an argument to the effect that if UR were true, then anyone who believed it should revise it in favor of UR'!

Another Resolution

Whether or not the critique of the last section is correct, there is another and ultimately deeper resolution of the Revisability Paradox. Notice that the paradox arises when we apply the fundamental principles of the Quinean epistemology to themselves. Thus an obvious response is to restrict their scope so that they are no longer self-applicable. We would no longer have "no statement is immune to revision" but rather something like "no statement of science (including the affirmations of mathematics and logic) is immune to revision." This restriction would still capture the main features of Quinean holism, namely, its refusal to recognize any epistemic distinction between mathematics, logic, and the rest of science, and its insistence that experience bears upon fairly large bundles of statements rather than individual hypotheses. It is plausible that Quine intended his epistemology this way, but we will not press the issue here. (It is also a resolution that Katz, himself, seems to endorse [MM, 387].)

Although we think this is the proper resolution of Katz's paradox, we can think of a number of problems with it. The first is that it seems to presuppose that one can distinguish science from nonscience or at least the methodology of science from science proper, and thereby between certain normative and descriptive claims. In light of Quine's famous statements that naturalized epistemology is a branch of psychology and that philosophy is continuous with science, one wonders how Quineans can rest comfortably with these distinctions.

Again, we do not want to get into questions of exegesis, but Quine explicitly embraces some normative epistemology. Quinean holists can live with rough distinctions between normative-descriptive epistemology and between science and its methodology, so long as they do not claim that we have a priori knowledge of the normative. In so far as we see methodology as telling us how to pursue the goals of science it can even be a scientific question as to how successful a given methodology is.[6]

Another worry with this resolution is that we might be left with no means for revising methodology itself, since we cannot appeal to a methodologically transcendent a priori. Methodological claims of the form "Using method M is likely to achieve results G" fall within the scope of scientific assessment (if they are stated with enough precision), and will not be problematic. The same cannot be said for normative claims of the form, "People who want to achieve G *ought* to use a method that is likely to achieve it," much less for those of the form, "Goal G is a *worthy* goal." For it is difficult to see how we could give a naturalized account of our knowledge of these claims, if we have any such knowledge. To be sure, quite a few philosophers have argued that something like the scientific method, broadly construed, can be applied to normative claims, even ethical ones, and they have pointed out that in accepting scientific hypotheses we use various norms and values that are not empirically grounded. We have no quarrel with these points, but they do not help much in naturalizing the epistemology of norms. For they do not get around the fundamental role that normative intuitions—as opposed to sensory observations—play in normative epistemology. Without a naturalized account of intuitions and the knowledge that they are supposed to furnish, we don't see how Quineans and like-minded naturalists could countenance normative knowledge.

For this reason we are inclined to take a noncognitive (or nonfactualist) approach to normativity, so that in the cases in question there is nothing to know, and no normative epistemological knowledge to naturalize. (Though, of course, one can still ask why we make normative claims, and why they appear to be knowledge.)

We deny that revising methodological and other norms comes about through acquiring normative *knowledge*. But we do not mean to exclude methodological changes arising through rational means. Historically, arguments for revising methodological norms have employed normative premises, even if only implicitly. For example, when physicists argue against the requirement that scientific theories be deterministic by pointing to the ascendance of quantum mechanics, they are indicating that the requirement is standing in the way of the goal of bringing an important set of phenomena within the purview of physics. Similarly when logicians argue against the requirement that a formal logic have a complete proof

[6] W. V. Quine, *Pursuit of Truth*. Cambridge, MA: Harvard UP, 1990. See pp. 19–21. See also W. V. Quine, "On the Nature of Moral Values," reprinted in W. V. Quine, *Theories and Things*. Cambridge, MA: Harvard UP, 1981; pp. 55–66.

procedure by pointing to the ability of second-order logic to provide categorical formulations of number theory and analysis, they too are pitting one normative requirement against another. If Hume is right, there is no avoiding the use of normative premises when arguing methodology or other normative matters, but so long as a naturalized account of our acceptance of the premises is available, this is not problematic in itself.

Instead of maintaining that we acquire norms through a priori means, we hold that we find ourselves with a collection of culturally conditioned norms and values, which we may or not modify in the light of experience, arguments, and changes in our condition. Normative argument can lead us to change our values, goals, and priorities—we have just mentioned two examples—but sometimes we change them as an *unreflective* response to changes in our circumstances. An octogenarian is less likely to value sex than an adolescent. An emerging nation will put more emphasis on practical knowledge than on theoretical speculation. And years of receiving frustrating counterexamples can lead philosophers to abandon research programs. Furthermore, reflective revisions might follow upon nonreflective ones. Instead of revising a principle P of methodology M unreflectively—say, by simply not finding it as binding anymore—we might revise some other feature of M unreflectively, thereby initiating M' and then using principles of M' to argue for revising P.

Might there be immutable normative principles, and might this not include rules of logic? The question needs further specification. Fans of the a priori, for example, do not hold that a priori factual claims are not revisable *simpliciter*, but rather that they are not revisable (or should not be revised) in the light of sensory evidence. To address our question, we need to specify the basis for reflectively revising methodological principles. Given what we said above, a good candidate for this basis is normative argument. Now at any given time it could happen that some principle was so fundamental to any rational discussion that no argument based upon other normative considerations could override it. Some debates about intuitionist logic and Dialethesism seem to indicate that certain parts of logic are fundamental in this way. The protagonists seem to talk past each other, so that what is a perfectly good argument to one either begs the question or involves an obvious fallacy to the other.

But once we admit that methodology can change, we see no reason for ruling out changes in the relative weights of methodological norms, and thus no reason for thinking that a principle that previously could not be overridden might be. Other changes, such as changes in our goals, could also lead to overturning previously inviolate principles, and, any of these changes might come about through processes in which reflection plays no role.

Unreflective revisions of normative principles, that is, cases where one finds that their normative intuitions have changed without being able to underwrite the changes with an argument, frequently occur in response to behavior. If many members of a culture or at least enough of its influential members engage in an initially deviant conduct, the culture's norms may change to legitimate the conduct. This goes for methodology as well as for language and morals. The changes in these cases come about through the initially deviant behavior generating new normative intuitions that in turn cause us to strive to bring our normative systems back into reflective equilibrium.

While it is undeniable that normative systems evolve, and often do so as the result of unreflective behavior leading to changes in them, are such changes legitimate? If we ask someone, "Why did you do that [reprehensible act]?" and they reply "I wasn't thinking," often we respond "Well, you should have!" Suppose that, on the basis of examples such as this, we grant that some unreflective changes in our normative systems are not legitimate because they result from illegitimate deviant behavior or intuitions. On what basis will we make room for legitimate deviance? For is it not a matter of definition that deviance contravenes its governing system? While this worry seems true of behavior, it need not be true of intuitions concerning correct behavior. For example, while it is against the law to prevent a person from criticizing the government, no law prohibits one from hoping that someone will stop the critic. Thus, deviant behavior might prompt nondeviant reconsideration of the very system prohibiting the behavior, and this in turn to a permissible revision of the system that makes the behavior acceptable. Some of the changes in contemporary sexual mores, dress codes, rules of grammar, and pronunciation come readily to mind.

This is to approach the question of legitimacy from the perspective of the system in force. If a system prohibits its own revision, then, of course, there is no way that it can acquiesce in changes to it, no matter how they arise. Since such dogmatism seems unacceptable, one wonders whether one can raise the question of legitimacy independently of a system of norms. Realists about the correctness of norms would certainly answer that one can. Such realism seems plainly wrong when it comes to the evolution of linguistic norms. Languages evolve, and there is no absolute right or wrong about it, though one might assess the changes as good or bad with respect to some purpose, such as international communication or ease of spelling or learning. But in the case of morals and methodology the realist's affirmation of independent standards is more plausible.

As a matter of practice, we evaluate behavior with respect to normative systems we now accept. Our own systems view deviance with differing degrees of severity, depending both on the type of deviance (e.g., whether it be linguistic or moral) and the cases at hand. Something similar applies to the ways we regard the actions of those who live under systems different from ours. We think it is fine for the French to speak French in France, but not fine for certain countries to punish thieves by amputating their hands. Here, of course, we are operating within our own mores, and not attempting to judge as we would were we living under the other mores.

However, when we deviate from our own systems we are left to our consciences, through which we cannot but judge right those actions that we sincerely take to be right. Indeed, for antirealists the question of the legitimacy of rejecting or sincerely deviating from one's previously accepted system of norms simply does not arise. For no independent standard for answering it is possible. As a consequence there is nothing intrinsically incorrect in rejecting a methodological absolute even if one's methodology prohibits doing so. In practice the realist's situation is no different. Whereas realists believe in facts of methodological correctness that obtain independently of the methodology one happens to espouse, they have no more independent access to those facts than the antirealist. Both realist and antirealists will judge right those actions that they sincerely take to be right, though, of course, each will give a different account of what this judging means.

So contrary to Katz, Quinean empiricism can survive (whatever survives of) the Revisability Paradox. Quineans can recognize a revisable normative epistemology/methodology without having to invoke some source of normative knowledge, a priori or otherwise.

INTUITION AND REASON

The keystone of Katz's "no contact" epistemology is his account of mathematical intuition. Mathematical knowledge arises through the use of intuition, deductive reasoning, and the systematization. The latter is a process through which we form systematic connections between clear and correct insights and intuitions and use the resulting body of theory to correct unclear intuitions. It also yields general truths that are not provable or immediately known through intuition but which can be shown to be indispensable to the best systematization of truths known through intuition and proof. Katz mentions Church's thesis and the principle of mathematical induction as examples of knowledge gotten through systematization (*RR*, 47; MM, 375). Intuition, on the other hand, is how we know truths, such as "4 is composite," that are "too basic for proof and too highly untheoretical for indispensability arguments." (MM, 376). Both intuition and deductive reasoning involve exercising the same "natural faculty." This is how Katz puts it: "We can say that reason is rationality in application to deductive structures and intuition is the same faculty in application to elements of such structures. We can think of intuition as reason in the structurally degenerate case" (MM, 381). This gives him a dialectical advantage, he claims, for "given that intuition is part of reason, philosophers either have to reject both intuition and reason as mystical, or accept both as aspects of one and the same natural faculty" (MM, 381).

Furthermore, by being "a natural faculty like thinking and imagining," intuition is no "more of a mystery than thinking or imaging." Moreover, "it is generally recognized that psychology cannot tell us much about the workings of thought and imagination, yet we do not hear the cry of 'mystical' or 'mysterious' raised about them". Thus those who complain that Katz has grounded his epistemology in mysticism or mystery are employing a "double standard" (MM, 376).

Katz also claims that his epistemology involves no "causal cross-over" between the abstract and concrete domains (MM, 376). Despite this, intuition and reason, more generally, have the ability to warrant a priori knowledge about abstract entities. In criticizing other views, he also tells us that intuition is not a matter of hearing an inner voice or having a feeling of conviction. Rather the "intuition we have of the truth of a mathematical principle—for example, the pigeon-hole principle—is nothing like this. There is nothing linguistic about such an intuition; it is a purely intellectual *grasp* of a mathematical structure" (MM, 377, our emphasis).

As one might suspect, some critics of Katz's views (including this paper's senior author)[7] have objected to his epistemology for positing mathematical intuition as a source of knowledge. Anticipating such complaints, Katz argued in *Realistic Rationalism* that only by appealing to intuition can we account for our knowledge of elementary, unprovable truths in mathematics, logic, and the other formal sciences (*RR*, 45). In his recent article he reiterated that intuition is "indispensable to any adequate account of our epistemic faculties" (MM, 363).

We disagree. We do not think that Katz has made a convincing case that invoking intuition is indispensable in any adequate epistemology for mathematics, and we do not think that he has begun to address the questions concerning intuition that make it so mysterious to many philosophers.

One reason that we do not think intuition is an indispensable element of the epistemology of mathematics is that we think Katz presents a false picture of mathematical justification—one that presupposes that in order to know any mathematical truths some mathematical truths must be known immediately (*RR*, 43–43; MM, 381). Although we do not deny that proof plays a major role in organizing and communicating mathematical knowledge, we do not subscribe to the view that justifying a mathematical claim requires deducing it from statements that are known immediately and, to use Frege's phrase, neither need nor admit

[7] See, for example, Michael D. Resnik, "Review of *Realistic Rationalism*," *Journal of Philosophy*, 96 (1999): 207–211.

of proof. It would take us too far afield even to sketch an alternative picture of mathematical justification, but there a number of antifoundationalist epistemologies of mathematics on the books these days.[8] The other reason for our skepticism is that we do not think that there is such a phenomenon as mathematical intuition or at least clearcut cases of it. It is a bit tricky even to describe the supposed phenomenon, so let us look at two of Katz's examples instead. One of these concerns the claim that the number 4 is composite. This is supposed to be too basic for proof, and, thus, supposedly known by intuition. But it seems to us that it does have a proof, albeit a very simple one. Here it is: since 2 times 2 is 4, 2 divides 4; and thus by definition 4 is composite. Perhaps, "2 times 2 is 4" is too basic for proof, although this is debatable too since we can derive it from "2 + 2 = 4" and a definition of multiplication. Now one would think that if there were such a thing as intuition, there would be no doubt in our minds that these statements need no proof. Furthermore, there should be no doubt that the claim that 2 is the *only* number that yields the same result when added to itself, multiplied by itself and raised to itself does need a proof. (Does the fact that 2 is such a number need a proof? Or is the conjunction of three basic mathematical truths also a truth that needs no proof?)

Katz's other example is the so-called pigeonhole principle. We find what he says worth quoting:

> Even mathematically naïve people see that, if *m* things are put into *n* pigeon-holes, then when *m* is greater than *n*, some hole must contain more than one thing. We can eliminate prior acquaintance with the proof of the pigeon-hole principle, instantaneous discovery of the proof, lucky guesses, and so on as "impossibilities." The only remaining explanation of the immediate knowledge of the principle is intuition. (*RR*, 45)

Perhaps, it is a mathematical weakness on our part, but the pigeonhole principle has never been transparent to us; we usually need some sort of visual aid, a proof of sorts, to render it obvious. Again, one would think that if the principle can be known by merely exercising a "natural faculty," something that even "mathematically naïve people" can do, then it should not have been a stumbling block for us.

Katz writes: "Proofs thus establish the necessity of their conclusions because their reasoning is so tight that no room is left for rational doubt"(MM, 367). Thus he might argue that intuition must be indispensable to a realist epistemology, since only by invoking it can we explain how mathematicians establish mathematical theorems beyond any rational doubt. Even if we grant that mathematics gives us some results that are beyond rational doubt, we do not see how this will

[8] Michael D. Resnik, *Mathematics as a Science of Patterns*. Oxford: Clarendon, 1997.

help his case. Katz does not seem to draw a distinction between those theorems that are beyond rational doubt and those that are not. Yet, it seems that he should. Some theorems are based upon principles, such as mathematical induction, which are obtained through systematization, and this process does not establish the necessity of its results (*RR*, 47–8). Moreover, Katz speaks of using systematization to correct unclear intuitions, and emphasizes that intuition is fallible (MM, 377). So it seems that the mere fact that a theorem has been proven does not establish it beyond rational doubt; rather it must be proven from intuitively known premises which themselves are beyond rational doubt. But what else must be added to an intuition to make it one that establishes something beyond rational doubt? Katz does not say. We are inclined to think that he has mistaken a psychological or sociological feature of theorem-proving with an objective epistemological one.

We conclude that Katz has not shown that intuition is an indispensable component of any adequate realist epistemology for mathematics—even granting him the controversial assumption that mathematics yields a priori knowledge of necessary truths of which there can be no rational doubt. What about the other claim that Katz makes for his view, namely, that mathematical intuition is not mysterious?

One difficulty we have in assessing this claim is that we are uncertain of the standards of clarity or nonmysteriousness that are to be in play. Katz chides Quine and Wittgenstein for their naturalism; so we would beg the question by using naturalist doctrines against him. Presumably, this would be the case if we were to claim that contemporary cognitive science recognizes nothing remotely like the rational intuition of mathematical objects, and so it is illegitimate for Katz to appeal to it. On the other hand, he tells us that we have intuitions through exercising a natural faculty; and he compares intuiting with thinking and imagining, processes which contemporary psychology does recognize. Thus it is fair to assume that intuition is at least something that could be a subject of scientific study.

But we find it hard to see how science could study intuition, given what Katz says about it. On the one hand, Katz emphasizes that intuition involves no contact between the person intuiting and the object intuited. Yet, on the other hand, he repeatedly speaks of it in terms that suggest some contact between them. In differentiating intuition from the Wittgensteinian idea of an inner voice, Katz says "there is nothing linguistic about intuition; it is a purely intellectual grasp of mathematical structure" (MM, 377). How can we grasp an object without any contact with it? Katz says that intuition is an "immediate apprehension of a mathematical structure that excludes all possibility of mathematical objects with that structure not having a certain mathematical property. Hence knowledge that 4 is composite is based on an immediate apprehension that its arithmetic structure excludes all possibility of its not being composite" (MM, 382). Again, the idea of an immediate apprehension of a mathematical structure makes one think of a contact with the mathematical structure itself. What else would the apprehension consist in if it has to be immediate?

Katz states that mathematical intuitions involve some sort of mental representation. He says that "the representation functions to focus our thoughts on the mathematical object(s) in question" (MM, 382). But if this is true then, intuition does involve some contact with an object through a representation of the object itself. The representation helps to focus on the object, by being somehow present to the mind and by representing the object accurately.

Now, Katz says that the relation between the representation and the object in intuition does not need to be causal (MM, 382). But if it is not, he should explain what type of relation it is and what type of relation there is between the representation and intuition as a faculty of apprehension. Do we intuit that 4 is composite because of the way we represent the number 4? Furthermore, it is not clear how his position on representation is compatible with his rejection of George Bealer's idea that having an intuition that A, is just for it to seem to you as A. Katz points out that if intuitions were seemings, they would present us with nothing more than an appearance of mathematical reality and we would face the problem of discovering what the mathematical reality is behind the appearances. Katz says that we cannot solve the problem by taking seemings to be causally related to causally inert mathematical objects. He then adds that if seemings have no causal relation to the mathematical reality of which they are appearances then it is not clear what ensures that a seeming with which we have an inner acquaintance accurately represents mathematical reality (MM, 379). But why does the same problem not arise for representations of numbers and their properties? Supposedly, a representation can be accurate or inaccurate—that is, in so far as it can represent, it can also misrepresent. So, the same problem that Katz attributes to Bealer seems to effect his own account.

Katz could get rid of the notion of representation or somehow maintain that the relation between the representation and its object is such that the representation is always accurate. This, however, would conflict with another of his characterizations of intuition, namely, that it is fallible (MM, 377). For how could intuition be fallible if intuiting never misrepresents an object or never grasps a mere appearance of an object? One wonders, in any case, how intuition can have the other properties Katz attributes to it and also be fallible. If it is the immediate apprehension of a mathematical structure and if, as in the case of a proof, it removes "any possibility for rational doubt that [the mathematical objects] are otherwise that the supposition represents them" (MM 367), then, it seems, an intuition could never be wrong. Intuiting just like knowing and proving would be an achievement, not an activity. The activity could be successful or unsuccessful but the achievement is, in and of itself successful.

We do not see that taking intuition to result from the faculty of rationality clarifies matters. For the only difference between intuition and reason seems to be that in the one case we "grasp" a mathematical structure, in the other we "grasp" a deductive structure.

Contrast intuiting with thinking and imaging. We can think about or imagine things that do not exist. We can think or imagine that something is so-and-so without it being so-and-so. Thus cognitive science is able to study thinking and imagining by studying what happens to people when they think or imagine, and can fairly set aside questions about the properties of the objects thought or imagined. But this could not be the case with intuition. When someone intuits that 4 is composite they stand in some kind of relation with 4 that they don't stand in when they are not intuiting that 4 is composite. If it involves grasping the structure of 4, then it would seem that in studying intuiting cognitive scientists have to study this grasping. Given what Katz says about it, we cannot begin to see how they could.

Contrast Katz's intuition with a postulational approach to mathematical knowledge.[9] Postulating that there are no cardinalities between that of the natural numbers and that of the reals *is* like thinking or imagining that this is so; because it does not ensure that it is so. Positing is merely a method for introducing entities and hypotheses about them into a discourse, and not a method for constructing entities or constituting truths or justifying claims. Thus an epistemology built upon postulation need not explain why or how postulation relates us to its objects.

In the end, it seems that Katz's strategy is to start with certain convictions about mathematics, and then posit entities and processes to account for them. Thus he thinks that mathematics discovers necessary truths about unchanging objects. Taking mathematical objects to be abstract entities nicely accounts for this, and also deals with the apparent need for an infinite mathematical ontology. But this ontological solution leaves one with well-known epistemological problems. In dealing with these Katz seems to have been driven to positing intuition as resulting from a "natural faculty." We have complained that we see little reason to think there is such a faculty. To this we now want to add a final complaint. In positing a new natural faculty, or at least in positing that an extant natural faculty has a new function, Katz is doing a bit of cognitive science. That is, he is introducing an assumption that presumably can be tested by empirical means. Hence, he is

[9] The approach has its routes in Quine's philosophy, but it is more fully developed in Resnik, *Mathematics as a Science of Patterns*.

doing first-level science rather than second-level foundations of science (MM 369). But in so doing he is violating one of his own strictures. Moreover, he is doing just what he says that he is not doing: "My account of mathematical knowledge as a priori knowledge of abstracta is itself a piece of a priori philosophizing" (MM, 389).

Like Katz, we are realists about mathematical objects, but we subscribe to the Quinean holism he so forcefully rejects. One reason he does so is because Quine cannot account for the apriority of mathematics. We do not dispute that, though we do not recognize accounting for the apriority of mathematics as a desideratum. But in view of the previous paragraphs we doubt that Katz can account for the apriority of mathematics either. Katz's other reason for rejecting Quine's holism is that he thinks it is inconsistent. We hope that we have set aside that worry too.

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