Book Review

S. G. Shanker. *Wittgenstein and the Turning Point in the Philosophy of Mathematics*. State University of New York Press, Albany, 1987. 358 pages.

1 Introduction   Just about every aspect of Wittgenstein's philosophy of mathematics is treated in this ambitious book. First, Shanker has important historical goals. He wants to clarify shifts in thought between the creation of the *Tractatus* [9] and Wittgenstein's return to philosophy in 1929, to explain Schlick's influence during the 'middle period' (1929 to the mid-1930's) and to show how middle period writings illuminate the more mature ideas in *Remarks on the Foundations of Mathematics* [13]. Thus the works most often cited are middle period: *Philosophical Grammar* [10], *Philosophical Remarks* [11], and Waismann's record of conversations [8]. Still, the basic aim seems to be to persuade philosophers of mathematics that Wittgenstein's later work makes a significant contribution to the field. I will focus on topics Professor Shanker takes up in this regard rather than issues of philosophical development *per se*.

2 Wittgenstein's anti-epistemological method   A basic premise of Shanker's is that Wittgenstein rejects "traditional epistemology", the "foundationalist" presupposition that "the source of our beliefs must be identified or their grounds be justified in order to *eliminate* sceptical doubts" (p. 33).[1] Wittgenstein is not claiming that foundationalism is *false*, but "Wittgenstein sought to demonstrate the *unintelligibility* of foundationalism and in the process to elucidate the proper nature of epistemology" (p. 34, italics mine). Its role is to uncover the "logical syntax" of propositions, here mathematical ones. This metaphilosophical view informs the book throughout; e.g., Shanker holds that Wittgenstein thinks Hilbert's Program is philosophically misguided since, without the foundational need to 'secure' mathematics, no justificatory need of consistency proofs exists (Chapter 6). On the traditional question of whether or not we have mathematical certainty, Wittgenstein is said to believe that "mathematical truths are 'certain' in . . . that all possibility of doubt has been grammatically excluded: it simply *makes no sense* to doubt the truth of mathematical propositions" (p. 285).
    Chihara labels such "grammatical" or "logical" approaches "left-wing" (see [2], p. 105, note 9). Left-wingers emphasize questions of meaning, and deny that

Wittgenstein states theses. So Shanker's approach is in sharp contrast to that of Crispin Wright, Wittgenstein's other major commentator. (Amusingly, Chihara dubs Wright an "extreme right-winger" (see [2], p. 105, note 9).) Even if one does not generally endorse Shanker's grammatical approach, he shows it has certain payoffs. As I discuss in Sections 3 and 5, respectively, Shanker has advanced the literature by clarifying Wittgenstein's enigmatic remarks about consistency proofs and the point of his requirement that mathematical proofs be surveyable.

3 Consistency   On the issue of consistency Shanker's interpretation of Wittgenstein seems to me to have advanced the literature. We know that Wright's interpretation, resting as it does on attributing to Wittgenstein the thesis that logic is antecedent to truth ([14], pp. 301–376), has been persuasively criticized by Chihara ([2], pp. 100–103). The account Shanker gives seems immune to such criticism and faithful to the texts.

Wittgenstein, Shanker explains, claims that mathematical propositions are "norms of representation". They are not "*about* anything", lacking descriptive content (p. 224). So they cannot be assertions admitting of degrees of reliability. This is why Wittgenstein rejects "the very premise that it makes sense to speak of the *reliability* of mathematical knowledge in the first place, let alone of placing it on a more rational basis" (p. 222). Hence, no consistency proof could increase the reliability of a mathematical system or endow it with any degree of certitude it lacks. Hilbert's mistake is to "accept the orthodox demarcation between 'mathematical reality' and 'mathematical knowledge'", "the abiding problem whether and if so when we can be certain that the two are in harmony" (p. 225). Hilbert has overlooked that consistency proofs are just that — just further pieces of normative mathematical practice, and normative import as such is distinct from philosophical content (for instance, see Shanker's remarks on pp. 156–158 and 214–215).

Wittgenstein's view of the matter can be expressed in terms that do not entail that mathematical propositions lack descriptive content or that metamathematical proofs have no epistemological status. Shanker gives an exposition of Wittgenstein's 'critique' of consistency in *Lectures on the Foundations of Mathematics* (see [12], Lectures XIX–XXIII), where the considerations appealed to are quite distinct. In *LFM*, Lectures XXI–XXIII, Wittgenstein is determined to illustrate how inconsistent systems can be usefully applied. We might use them, say, to build bridges.[2] Does this imply that we would be likely to err in the process? To the contrary, insists Wittgenstein, it could be highly improbable that errors would be made during our construction of the bridge. If the physical assumptions we use are confirmed and our calculations doublechecked, the likelihood of mistakes is strongly reduced. Turing objects that "with the ordinary kind of rules which one uses in logic, if one can get into contradictions, then one can get into trouble" ([12], p. 219). But, as Shanker rightly notes, Wittgenstein's reply deserves our careful attention, since it expresses his fundamental attitude toward consistency proofs: "If a contradiction may lead you into trouble, so may anything. It is no more likely to do so than anything else" ([12], p. 219).

As we know, Wittgenstein said, problematically, that so long as a contradiction is "hidden" within a system the latter is "as good as gold" ([12] p. 219).

Critics seem to think that he means that falsehoods are as good for understanding the world as are truths! Shanker helps to discredit this interpretation. First, 'discovery of a contradiction' for Wittgenstein means that we have a "stalled move in the calculus" which interferes with applying the system (p. 252). If we derive a "technical" contradiction, a formula "*p* and not-*p*", we stumble upon something we do not know how to use; it "blocks" the game (cf. pp. 232–238 and 251–252; [12], p. 223). Thus the "very notion of a 'hidden' contradiction is . . . absurd" (p. 252). Contradictions are formulas for which we have no nonstipulative use because "all information has been cancelled out" (pp. 237 and 256); deriving one requires that we decide how to proceed, how to restore meaning to our activities. Notice that no mathematical error is in question; there is only a nonsensical formula with which we must deal before we can continue. There is, however, a sense in which Wittgenstein allows that there can be 'hidden contradictions'. For he does not fail to see that contradictions can be derivable in a system, yet remain underived. In such a case, unless some problem clearly arises as we apply the system, it remains "as good as gold". By hypothesis, no trouble arises.

Chihara's well-known attack on [12] often rests on misstating the point we have just mentioned. He expresses Wittgenstein's point by saying that "so long as the contradiction is hidden, it is doing no harm, and if it comes out into the open, it will do no harm . . . it is a simple matter to make *ad hoc* stipulations to prevent us from drawing any unwanted conclusions from a contradiction" ([1], p. 99). Granted, at one stage of his interchange with Turing, Wittgenstein states that we might "simply say, 'This is of no use — and we won't draw any conclusions from it'" ([12], p. 209). This does not warrant Chihara's reading, and there is no clear textual evidence implying that Wittgenstein generally views the revision of inconsistent systems as a "simple matter"!

Chihara is concerned with Wittgenstein's rejecting Turing's reason for being unpersuaded by the suggestion made in the preceding quotation. Turing says, "if one made that rule, one could get round it and get any conclusion which one liked without actually going through the contradiction" ([12], p. 220). Ensuing passages clearly indicate that Wittgenstein does not reject Turing's contention, but is simply not interested in it, in what "could" happen. Chihara simply ignores these passages. Yet their straightforward upshot is that the fact that a contradiction is proved in a mathematical system *does* not lead people "through doors into places from which they could go any damn where. It isn't true that this happened with Frege's logic. If they did this, Frege's logic would be no good, would provide no guide. But it does provide a guide. People don't get into these troubles" ([12], p. 228). The issue is whether the derivability of contradictions is *likely* to yield application-problems in this or that context.

The point is clear earlier in Wittgenstein's and Turing's exchanges. Turing claims ([12], p. 218) that it is "almost certain" that use of inconsistent calculi will lead to application-problems. In some cases, Wittgenstein admits, problems may be more likely to occur; but he holds that in no case is this due *merely* to the fact that "*p* and not-*p*" is provable ([12], p. 219). As Shanker notes, the likelihood of errors increases in cases where it is probable, as Turing envisages, that system-users apply "a *contrariety* hitherto unthought of" (p. 238). Suppose someone derives "$7 + 5 = 12$", while someone else derives the contrary propo-

sition "7 + 5 = 13", and subsequently these equations are applied in building
a bridge. Mistakes, e.g., about quantities of needed materials, may readily be
made. And so it may be likely in this situation that the bridge built will soon
collapse. Still, as Shanker insists on Wittgenstein's behalf, this is an "application-
problem", not a mathematical one. Whether contraries get applied in a way
resulting in disaster is an *empirical* question (pp. 253–254; [12], p. 215). As Witt-
genstein put it, "The trouble described is something you get into if you apply
the calculation in a *way that leads to* something breaking" ([12], p. 219, italics
mine; cited on p. 253).

What bothers Chihara about this position is presumably the lack of room
for the idea that inconsistent systems are *mathematically erroneous*. Indeed,
Wittgenstein denies this: "We have an idea of the sort of mistake which would
lead to a bridge falling. (a) We've got hold of a wrong natural law . . . (b) There
has been a mistake in calculation . . . " ([12], p. 211). (b) is not to be confused
with a system's being mistaken 'in itself,' e.g., by being inconsistent ([12], p.
218). A system can only be wrong *for* purposes to which it is to be put, and this
is revealed by the facts of experience, e.g., finding out that using the system's
rules has interfered with achieving our goals. The likelihood that a set of rules
promotes our chosen ends, including that of knowledge-seeking, rests on fea-
tures of the world which determine whether it 'cooperates' as we make specific
uses of those rules. Wittgenstein goes so far as to assert, "It may be that if one
throws dice in order to calculate the construction of the bridge it will never fall
down" ([12], p. 218). *Mathematics* cannot settle such a matter. The world of fact
can.

The relevance of the distinction between mathematical and factual ques-
tions to Wittgenstein's discussion of consistency has not been well-recognized.
A virtue of Shanker's book is its constant appreciation of the importance of this
distinction in Wittgenstein's philosophy of mathematics. Chihara says that for
Wittgenstein "it is a confusion to suppose that the disaster [of a bridge collaps-
ing] could have resulted from using an *unsound* system" ([1], p. 100). Now Witt-
genstein has granted that using inconsistent systems might permit us to derive
contrary equations and to apply these disastrously. By the same token, a situ-
ation could also be such that, had contrary propositions *not* been applied, the
bridge would *not* have fallen. Here there is a reasonably good sense in which
a disaster is 'traceable' to the use of contrary equations. So, did the disaster come
about due to "an *unsound* system"? The answer for Chihara is that in a very real
sense it did.

Further, if the physical circumstances of the situation were not practically
manipulable, there is no reason to think Wittgenstein would deny the rationality
of trying to rid our rules of their inconsistency. He evens says that, were we to
use "a calculus in which a man was liable to go wrong", e.g., liable to derive and
misuse contraries, we might well find "we had neglected to make the rules strin-
gent enough" for the situation at hand ([12], p. 222). Following Wittgenstein's
advice, suppose we show that a restricted set of rules is 'stringent enough' to
avoid some application-problem we encountered when using an old, 'looser' set
of rules. If Wittgenstein's position is right, it cannot be assumed that showing
this requires us to establish consistency. Whether new rules are more useful than
the older ones—or useful at all—will continue to depend on empirical features

of the situation in which application of the rules is made. If the status of the question of mathematical applicability is empirical, its status does not change simply because we have 'tinkered' with our rules!

I have tried to convey the virtues of Shanker's exposition. Some of its short-comings include occasional inaccuracies; e.g., Shanker says, "the answer to Turing is that you cannot get any conclusion you like *without* going through the contradiction, and this is something we need not do" (p. 243). This is a mathematical claim, thus violating the prohibition of "meddling with the mathematicians" ([12], p. 223), a prohibition Shanker recognizes.[3] His claim also seems wrong that if contraries are derivable in a system there must be "different *Beweissysteme* incorporated in the theory" (p. 253). I find no argument for this, and it is implausible. Nor is the assertion plausible that applying contraries is "categorically similar to the case of building a faulty bridge by misapplying a single method of calculation" (p. 253). "Misapplication" suggests using mathematics under some false empirical assumption, say, applying arithmetical equations to objects which spontaneously disappear. The case of using a system which permits us unwittingly to apply contraries seems rather different. Finally, readers might well be put off by some unfortunate overstatements; e.g., "if there is no skeptical problem to answer, Hilbert's Programme stands exposed as the illegitimate offspring of a philosophical misadventure inspired by epistemological chimeras" (p. 277). At this point in the book, Shanker has not *argued* for the view that no formulable skeptical problem about mathematical knowledge exists.

4 Mathematical meaning   Most commentators[4] believe that Wittgenstein sees meaning in mathematics as given by proof, excepting 'propositions' that serve as axioms. The exegetical question is how far to extend the general point and how much meaning proofs bestow. Shanker adopts the standard view that meaning is given *completely* by proofs in the cases where proof determines it. As to which mathematical propositions have their meaning so determined, he proposes a new idea. The edict, "the proof is part of the grammar of the proposition" ([10], p. 370, cited on p. 86), extends only to proved propositions whose proofs were not available as a matter of course in an existing *Beweissystem*. Proving these requires construction of a new rule or an entirely new system (pp. 83 and 97). Such a proposition is not an 'ordinary' mathematical one. Prior to proof, it has the special status of being a "conjecture" (p. 97).

Wittgenstein contends that no mathematical proposition can have meaning apart from *all* proof systems. Shanker insists that this allows for 'routine' calculation, a 'piece of homework' in which one understands the problem to be solved before one solves it. The meaning of an ordinary equation like "14 × 14 = 196" is *not* determined by its proofs, viz. arithmetical calculations. Calculating an equation differs radically from trying to prove a conjecture—trying to give meaning to an expression that, so far as one can see, *may* have none (pp. 84 and 96). If one fails to get proof or disproof here, "certain apparent problems lose their character as problems" ([11], p. 148, cited on p. 96). Finding that an undecided formula is undecidable is to discover that it poses only an "apparent problem" and is only an *apparent proposition*.

Hence for Wittgenstein the set of mathematical propositions includes *rou-*

tine expressions; e.g., arithmetical equations or the Pythagorean Theorem (p. 85), whose method of solution is given by available procedures and *less-than-routine* expressions that have nonetheless been proved, e.g., the claim that Euclid's Parallel Postulate is independent of his remaining postulates (pp. 83–84). Shanker wants to 'save the appearances': On the one hand, it seems obvious that we can understand an equation prior to grasping a proof of it; on the other, it seems reasonable to say that, if proving *p* requires wholly new proof-procedures, *p* is not wholly comprehensible from the perspective of our current system.

The claim that a proof gives meaning to what is proved has a paradoxical air about it: if more than one proof of *p* were given, wouldn't these give distinct meanings to '*p*', and hence determine distinct propositions p1 and p2? This objection to Wittgenstein is in fact so bad it is hard to take seriously. Yet Wittgenstein uses it to raise the question of what it means to say that two proofs "prove the same" thing (see [13], III 58 and VII 10 for two answers). Shanker says that Wittgenstein permits multiple proofs of *p* but only *within* a proof-system. And, he says, *p*'s proofs are also in a sense identical; they are 'the same proof' in virtue of appealing to "methods" that are unified by one system's procedural framework. By contrast, two proofs cannot serve to prove one proposition when they use methods from "autonomous *Beweissysteme*" (p. 86).

I think that Shanker's account here is highly original. My worry is that the later texts seem to make no such distinction. And can Wittgenstein consistently avoid saying that propositions provable in an existing system originally get their meaning from specific *proofs*? After all, to show that "14 × 14 = 196" is a proposition *of* the system—an *arithmetical* proposition—we must surely show some connection or other which it has with the set of rules called "arithmetic". What except a proof could establish the right sort of connection? The problem is how we should take these remarks: "a mathematical proposition is only the immediately visible surface of a whole body of proof and this surface is the boundary facing us"; "A mathematical proof is an analysis of the mathematical proposition" ([11], pp. 162 and 153, cited on pp. 91–92).

Now, Shanker provides a reasonably cogent answer. A truth table for "*p* and *q*", he notes, may be said to give an "analysis" of this specific conjunction without thereby being regarded as the final source of its meaning. The table gives "a more explicit formulation, a 'translation', of that proposition" (p. 92). Analogously, we may view a calculation as giving a more explicit formulation of an equation's meaning without thereby functioning as a *source* of meaning. There are different ways of calculating an equation, but these comprise the same "articulation" of "the network of relations which underpin the role" of the equation "*qua* rule of syntax". Calculatory methods 'translate', but do not *constitute*, the sense of the equation. According to Shanker, this interpretation is compatible with the fact that "We cannot treat any expression as a mathematical proposition unless it belongs to a proof-system" (p. 92). A mathematical proposition has a definite meaning precisely in its role as a "rule of syntax". This role, however, makes a connection between the proposition and a *system* of proof, not any single proof. Our conjectures, though, gain definite meaning from *specific* proofs. An unproved conjecture has merely a "heuristic" role; it gives the mathematician some idea of directions to pursue (cf. pp. 110–117). Yet, "what-

ever the heuristic function which it exercises..., the sense of the *Beweissystemlos* verbal expression does not *change* after a proof has been constructed: rather, it *emerges*" (p. 106). This solves a different, more serious paradox. As Wittgenstein queries, how can any proposition be proved if its proof gives it meaning? For it appears that a *different* proposition must be proved from the one we set out to prove. On Shanker's interpretation, what is unproved is not a proposition; a determinate proposition comes to exist when the proof of it is concluded. The proposition "emerges".

Shanker also argues (pp. 93–99) that Wittgenstein accepts the law of excluded middle. (This is a startling idea since many assume that his anti-realism involves rejection of this law. But Shanker holds that Wittgenstein is not an 'anti-realist'; see Section 6 below.) Shanker argues that since undecided formulas do not express *propositions*, and so have no truth values, the law of excluded middle must hold. On the basis of evidence in middle texts, he supposes that Wittgenstein takes the law's variables to range over the set of *propositions* and claims that the mark of a "proposition" is its satisfying the law (pp. 97–98). Thus no undecided expression can *violate* the law. I find this interpretation puzzling. Assume the intuitionists similarly interpret the variables in the law of excluded middle. Then they can *reject* the law, as they *do*, only by allowing that undecidable formulas *express propositions*. Yet the intuitionists agree with Wittgenstein that undecidable sentences make no definite assertions, hence express no propositions. Acceptance or rejection of the law seems to turn, then, on the superficial issue of the range given to its variables. I do not think this is a useful way to illuminate notable differences between Wittgenstein and the intuitionists.

Other Wittgensteinian remarks about the law of excluded middle ([13], V 9–23) may need consideration here. In these passages, Wittgenstein is far more intent on criticizing the law than his middle period thought supports. He says, "When someone sets up the law of excluded middle, he is as it were putting two pictures before us to choose from, and saying that one must correspond to the fact. But what if it is questionable whether the pictures can be applied here?" (V 10). In context, the argument seems to be that the law is *incorrect* since it does not 'apply as widely' as we assumed. More specifically, the passages are designed to show that the infinitary statements Wittgenstein discusses are not meaningful because they violate the law of excluded middle. What *violates* the law, though, must serve as a counterexample to it. This illustrates just one instance of what may be important discrepancies between the middle and the later work.

5 Surveyability   There is, of course, good reason to contrast the views of intuitionists to Wittgenstein's, viz., his general distaste for psychological approaches to knowledge. Shanker takes good account of Wittgenstein's anti-psychologism (cf. pp. 93 and 228–229), and it is central in Chapter 4's treatment of surveyability. Most novel is Shanker's claim that "surveyability" is not an epistemological notion. Surveyability has nothing to do with our mental powers or "recognitional capacities". Proofs satisfy the requirement of surveyability if they do "not confuse infinite processes with finite totalities, or experimental with mathematical techniques" (p. 128). This constraint does not in any obvious way concern our epistemological powers.

The first section of Chapter 4 lays out this general idea, while the second

and third focus on more particular topics. The second section critiques proba-
bilistic 'proofs' like those which Rabin's computer program supposedly gives of
the probable compositeness of numbers (pp. 131ff). To allow that Rabin's pro-
gram yields *nondeductive proofs* would undermine Wittgenstein's distinction
between mathematical and empirical considerations, and to uphold that distinc-
tion is a major goal of the chapter. In this same section, Shanker expresses the
allegedly Wittgensteinian position that a computer cannot construct a proof, nor
a pocket calculator a calculation (pp. 158-159, note 25).[5] So it could look as if
the salient point is that proofs must not be 'mechanical', that only 'non-
mechanical beings' like us can prove things. I ignore this in what follows. Not
only will many find it implausible as an interpretation of Wittgenstein, but the
point is incidental to the more important one that surveyability should be charac-
terized in nonepistemological terms.

   Shanker notes that the greatest challenge to Wittgenstein's nonempirical or
'a priorist' construal of proof comes from the suggestion that even traditional,
noncomputerized proofs involve empirical assumptions (pp. 143-158). We are
referred to a well-known discussion of the Appel–Haken–Koch solution to the
Four Color Problem. Detlefsen and Luker [3] argue that the Appel–Haken–Koch
solution is not 'empirical' *merely* because of the unsurveyability of the reduci-
bility lemma in the proof: they deny that the lemma's lack of surveyability is the
"crucial factor in determining the empirical character of the proof of the 4CT"
([3], p. 803). Shanker believes it is a logical, not an epistemological, point that
proofs must be surveyable. Hence, the issue for him is not whether Appel–
Haken–Koch tried to prove something the *knowledge* of which rests on epistemic
powers we presently lack. Neither is Shanker claiming that in principle no proof
of the conjecture is possible. He emphasizes that "Wittgenstein's response . . .
would surely have been that . . . there is certainly no *a priori* reason to deny the
possibility of discovering a solution for Guthrie's problem (by creating a meaning
for his 'question')" (p. 145). As we know, Wittgenstein thinks philosophers are
not justified in judging whether or not constructions are mathematically sound.

   So why is Shanker certain that Wittgenstein would reject the widely ac-
cepted Appel–Haken–Koch construction? The major reason is this: "the point
is that how an unavoidable set of reducible configurations might have been dis-
covered does not amount to a proof of the same, platonist confusions notwith-
standing" (pp. 151-152). Shanker is referring to the reducibility lemma which,
of course, is ostensibly established by appeal to data obtained by computer.
Shanker holds that the procedure used by the computer for testing configura-
tions for reducibility is not a genuine proof-procedure. His crucial reason for
holding this is that "the process of testing these configurations is 'purely mechan-
ical'. . . what we need to know is not *that* there is an unavoidable set of reduc-
ible configurations, but *how* to generate it; not a *test*, but . . . a *rule* for their
construction" (p. 152).

   There is a connection between this distinction and surveyability: "a proof
must be surveyable in the sense that we can grasp the 'law' forged by the proof"
(p. 153). In Wittgenstein's words, "I must be able to write down a part of the
series, in such a way that you can *recognize* the law . . . no description is to
occur in what is written down . . . " ([11], p. 190, cited on p. 153). So Shanker's
claim is that the reducibility 'lemma' fails to utilize a procedure which provides

us with a "rule" as to *how* to generate an unavoidable set of reducible config-
urations; the computer's procedure merely establishes, as it were by experiment
(p. 153), *that* there is such a set. In general, Shanker's reasoning is from the
alleged absence of (logical) surveyability to there being a mere *experiment* leading
to the 'lemma'; from the latter he concludes that the 'lemma' has no genuine
proof. If this is right, we have no proof of the Four Color 'Theorem'. Shanker
is insisting that, if we understand "surveyability" properly, we see it *is* the cru-
cial element on which Appel–Haken–Koch's so-called proof founders.

I think Shanker's reasoning fails to address the original challenge he took
up (pp. 142–143). If, as Detlefsen–Luker claim, *surveyable* mathematical proofs
may involve empirical assumptions, then the presence of unsurveyable content
in the Appel–Haken–Koch solution — insofar as this indicates *empirical* content —
is no objection to the solution.

Granted, Shanker rejects Detlefsen–Luker's thesis about the empirical char-
acter of traditional proofs. The question then becomes whether he has cast rea-
sonable doubt on that thesis. I fear he does not even begin to cast doubt on it.
A defense of the thesis is given by Detlefsen–Luker using Gauss' proof that the
sum of the first 100 integers is 5,050. Detlefsen–Luker argue that his proof
involves specific empirical assumptions. To support this, they claim that the
proof (on one general conception of "proof") involves the assumption, e.g., that
the result "5,050" was actually obtained. Such assumptions, they believe, are part
of that upon which "our confidence in the results of such a computation [are]
based" ([3], p. 808). Without deciding the merits of their case, it remains that
Shanker's reply that our confidence in Gauss' proof is based rather "on the rules
of arithmetic" is anemic (p. 154). This is what is at issue, and the burden of
proof is on Shanker to make Wittgenstein's distinction between "empirical tests"
and "rules for mathematical construction" precise, if this is to shed light on the
relevant issues.

Whether we find Shanker's negative assessment of 'computer proof' com-
pelling, his interpretation of Wittgenstein's concept of surveyability is interest-
ing and original. Now it often happens that Wittgenstein refers to surveyability
in terms that seem epistemological (see, e.g., the above quotation from [11]).
So a modicum of skepticism about the anti-epistemological account is reason-
able. Still, Shanker is right in placing heavy emphasis on Wittgenstein's not tak-
ing the "surveyability" of proofs to be something that alters precisely to the
extent that our mental powers might. This, moreover, is consonant with Witt-
genstein's disinterest in psychology and with his 'objectivism' about mathematical
truth (see Section 6 below). The anti-epistemological interpretation can also be
supported by key passages in both the middle and the later work. One needs to
notice Wittgenstein's frequent mention of "rules", "pictures", "procedures", and
"laws" which are, on his view, of the essence of proofs to reveal. He speaks in
numerous contexts as if the pertinent features belong objectively to proofs and
are, as it were, encoded in them. What counts as a *proof*-construction does not
appear, then, to be contingent on specific abilities we might employ in the pro-
cess of scrutinizing such constructions.

I have supported Shanker's 'logical' interpretation of the concept of sur-
veyability in light of important texts, yet intimated that he has not given us a
sufficiently rich analysis of this concept. It is not enough to say that surveyable

proofs are *not* guilty of confusing infinite processes with finite totalities, or experimental with mathematical techniques (see the earlier quotation). We need a more detailed explanation of the distinction between "experimental" and "mathematical" activities. In later texts one finds Wittgenstein remarking on at least two central differences which seem to him to drive a wedge between experiments and mathematical proofs. He comments that to claim that "Proofs must be surveyable" "aims at drawing our attention to the difference between the concepts of 'repeating a proof', and 'repeating an experiment'" (III 55). What is the difference? Wittgenstein says that repeating a proof "means, not to reproduce the conditions under which a particular result was once obtained, but to repeat every step *and the result* . . . proof is something that must be capable of being reproduced *in toto* automatically . . ." (III 55). If one repeated an experiment in an analogous way, so as to repeat its prior result, we would have to insist that the experiment was not proper, but 'rigged'.

A second difference to which Wittgenstein frequently refers is hinted at in this quotation. Consider the "conditions" under which experiments are performed or proofs constructed. The empirical conditions of proof-construction are irrelevant to reproducing any proof. The 'same proof' can and must be reidentified without appeal to features of the environment. With experiments exactly the opposite is the case. To reproduce an experiment one must be able to repeat it *under the same experimental conditions*. It is of the essence of an experiment that at least some of the conditions in which it is first performed are of a kind necessary for repeating that experiment later on.

Once in [13] Wittgenstein goes so far as to say that being surveyable *"really means nothing but*: a proof is not an experiment" (III 39, italics mine). The objective differences between proofs and experiments, between their 'logic', determine what "surveyability" refers to. Wittgenstein's remark signals that: (1) there are facts in virtue of which proofs and experiments must be distinct and (2) some of these facts serve to constitute a proof's surveyability. To understand what it is for a proof to be "surveyable" is to know what these features are. In support of Shanker, the features which most interest Wittgenstein in [13] are not epistemological. They are logical properties of phenomena connected with activities such as reproducing proofs, checking them, reperforming experiments, and so on. Much work needs to be done to develop this type of interpretation of surveyability, and more, clearly, than Shanker has included in the book. I think his proposal, though, is potentially of enormous significance for Wittgenstein scholarship. A fuller interpretation would clarify why Wittgenstein believes that surveyability is essential to proofs. It could also help settle the question of what his reaction to computer-assisted proofs would have been. Although I have said Shanker's remarks about surveyability are incomplete, his interpretation along with his assessment of Wittgenstein's view of consistency proofs are invaluable in furthering our understanding of Wittgenstein's later work.

6 Realism and objectivity   Shanker says that Wittgenstein sees the dispute between 'Realists' and 'Anti-realists' as unintelligible. It "hinges on the answer" to an unintelligible question (p. 57), viz., "Are unproved mathematical conjectures determinately true or false?". By Shanker's lights (see Section 4 above), Wittgenstein holds that conjectures have *no* sense unless the introduction of new

rules which lead to new proofs endows them with sense. To ask whether there are "verification-transcendent" statements (in Wright's terms, "investigation-independent") rests on a "logical fallacy". Asking this would only make sense if a *"nonsensical expression"* could be true, which naturally it could not. (This refers to an expression having essentially the same status as an unproved *conjecture*. Recall that the meanings of proved mathematical statements are internally related to their multiple proofs, and therefore such statements would not count as candidates for expressions having "investigation-independent" status.) If so, Wittgenstein sees platonists as being *logically* confused (cf. pp. 51 and 66).

Early in Chapter 2, we are told that Wittgenstein's 'verificationism' differs from that of the positivists. They hold that "the bounds of sense are . . . firmly tied to what we are capable of grasping: to our 'recognitional capacities'" (p. 43). Wittgenstein agrees that "a sentence can only be significant if it is 'graspable'"; but this signifies that a sentence "adheres to the logical syntax of a *Satzsystem*" where there are "*Satzsystem*-rules to determine its use"; "graspability" is thus a matter of "grammatical intelligibility", not of positivism's "quasi-epistemological considerations" (p. 43). Many philosophers agree in distinguishing Wittgenstein's semantics from that of the positivists. Note that this can be defended by observing his aversion to using the verification principle as a 'theory of meaning'. Shanker himself points out that for Wittgenstein appeal to verification conditions is "one way among others of getting clear about the use of a word or sentence" (quoted in [7], p. 54; cited on p. 43). "Verification conditions" may simply be taken to provide us with useful procedures for obtaining linguistic clarification when it is needed.

Shanker's above-mentioned remarks involve the stronger tenet that verification conditions are specifiable in a way that is unconnected with our epistemic abilities. Once again, a 'logical' account is being proposed, here of Wittgenstein's conception of verifiability. In my opinion, the account resembles the earlier logical account of surveyability in needing further development and richer analysis than Shanker's book affords. That aside, we refer the reader to some important material in the earlier parts of Chapter 2. There is, first, a sensible critique of Wright's thesis (in Shanker's words) that "Wittgenstein's adoption [in the 1930's] of verificationism [is] proof of his conversion from 'Realist' to 'Anti-realist' semantics" (p. 42). There also are plausible criticisms of the wider thesis that the 1930's interest in both verificationism and constructivism reveals Wittgenstein's shift to Anti-realism from a basically Realist framework in the *Tractatus*. In this context (pp. 44ff), as throughout much of the chapter, Shanker's target is Dummett's semantical conception of the traditional dispute between 'realists' and their opponents. I forego discussion of early and middle period issues, e.g., that of whether the *Tractatus* is a realist work (see "Wittgenstein's 'Conversion'", the second section of Chapter 2), in order to focus on the question of whether Dummett's semantic distinction, or certain distinctions Wright draws, has relevance to our understanding the later Wittgenstein.

Shanker (p. 47) cites Dummett's well-known claim that "no sense can attach, for a constructivist, to the notion of a statement's being true [in mathematics] if this is to mean any less than that we are actually in possession of a proof of it" ([6], p. 164). This description of constructivism, Shanker argues, fails to attend to the historical emphasis given by constructivists like Kronecker

on prohibiting proof by *reductio ad absurdum*. Dummett's description changes the dispute from one over the "nature" of proof to a dispute regarding the epistemic significance of the "possession" of proofs of mathematical statements (p. 47). This more "technical side" of the constructivist/platonist debate is not discussed in detail in Chapter 2. What Shanker most vigorously pursues is the general idea that the traditional debate revolves around a meaningful "logical" issue, whereas Dummett's 'semantic debate' is bereft of sense: "we cannot even speak of the existence of a mathematical proposition [statement] in the absence of a proof (cf. Chapter 3)" (p. 53; see Section 4 above).

The 'realists' in Dummett's debate say that mathematical statements can be meaningful independently of our possessing proofs of them. Shanker's Wittgenstein rejects the "very picture" here sketched, for he denies the *intelligibility* of the view that 'proof-independent meaning' may accrue to mathematical statements (see pp. 53, 58, 65, 72 and *passim*). At this stage Shanker's interpretation becomes puzzling. He has argued that Wittgenstein thinks "it is of the essence of a mathematical statement that it is asserted as the conclusion of a *proof*"; these are Dummett's words (in [4], p. 327), and Shanker grants that "this identifies Wittgenstein as a constructivist in Dummett's Anti-realist sense" (p. 53). How, then, can Shanker consistently: (1) deny that Wittgenstein is an Anti-realist in this sense? and (2) insist that *that* is so because Dummett's so-called dispute is incoherent?

A sympathetic reading helps to resolve this dilemma. Shanker metaphorically says: "[For Wittgenstein] a mathematical proposition is not in some sense a *free-floating* expression, standing in need of a proof; rather, a mathematical proposition is internally tied to its proof . . ." (p. 53, italics mine). He acknowledges Wittgenstein's constructivistic (Dummettian) Anti-realism only insofar as Wittgenstein would not *deny* the constructivist thesis as formulated. But we cannot suppose that Wittgenstein *affirms* the thesis. To affirm that a mathematical proposition must be "asserted as the conclusion of a proof" is highly misleading. For, Shanker believes, Dummett's formulation of the traditional dispute completely ignores the Wittgensteinian point that the concept *mathematical statement* is unintelligible apart from the concept *mathematical proof*. This point (examined in Section 4) explains Shanker's misgivings about characterizing Wittgenstein as a "constructivist" in Dummett's sense.

Revealingly, Shanker informs us that Dummett's distinction incoherently implies that it makes sense to "speak of producing a proof as evidence of the truth of a mathematical proposition". But to the contrary, "the proof *determines the essence* of the mathematical proposition, and it is as unintelligible to suggest that the proof provides evidence for itself as it is to assert that rules stand in need of evidence" (p. 53, italics mine). Thus we cannot take mathematical statements and proofs to be logically separable in thought. To try to do this is to attempt the task — which is impossible — of construing proofs as a kind of *evidence*. Shanker repeatedly makes this point in Chapter 2, and the point connects closely with the major interpretative claims his book advances about objectivity.

Shanker says: "mathematical propositions are divorced from the evidential framework as it is empirically understood" (p. 52) and "The essence of Wittgenstein's account is that the meaning of a mathematical proposition is characterised by the fact that it is *not* tied directly to the production of evidence

in the way that is true of empirical propositions; that it makes no sense to speak of evidence in [this] context" (pp. 52-53). Wittgenstein "simply disputed the assumption that it is intelligible to speak *in the normal sense* about the objectivity of 'mathematical facts'" (p. 62). Moreover, we read, Wittgenstein "took from Frege the idea that mathematical propositions are meaningful [*contra* extreme formalism] while from Hilbert the idea that they are not true in the empirical sense of 'true'" (pp. 68-69).

Surely Shanker is correct that Wittgenstein thinks it important to bring out both similarities and crucial differences between empirical and mathematical propositions. In the first place, on Shanker's account the similarities explain why both are called "propositions". Second, Wittgenstein puts weight on exactly those differences that may help us to understand these two 'logical' distinctions: (a) descriptive propositions *versus* rules of syntax and (b) truth and objectivity within the empirical framework of evidence *versus* truth and objectivity within the "conventional", "grammatical" framework of mathematical activity. I do not accept these more specific points, but let us examine their force.

If it be said that "objectivity" consists in the possibility of presenting evidence for what a statement or proposition *describes*, Shanker's seemingly reasonable rebuttal is that "It is, of course, absurd to suggest that only a platonist can believe in the objectivity of mathematics" (p. 68). Wright says that "The root idea of objectivity is that truth is not constituted by but is somehow independent of human judgement" ([14], p. 199; cited on p. 68). Shanker retorts that this is, *contra* Wright, "not quite the same thing as" Wittgenstein's "crucial point". *His* point is that "'human judgement' has nothing whatsoever to do with what constitutes the truth or falsity of mathematical propositions. This is entirely a matter of [their] syntax . . . of their use as grammatical conventions" (p. 68). It is difficult to see offhand just how Wright's characterization of "objectivity" conflicts with the "point" Shanker has ascribed to Wittgenstein. Of course, Wittgenstein does deny that mathematical truth is "constituted by" "human judgement". But Shanker holds that Wright's distinction is misdrawn—it classifies as "Anti-objectivists" only those who, unlike Wittgenstein, are either willing to embrace subjectivism or go so far as to accept "idealism" in the traditional sense (pp. 60 and 61-62).

After rejecting the above characterization of objectivity which Wright had proposed, Shanker considers a different suggestion Wright makes. This is that the Anti-objectivist is one who refuses to "admit . . . a distinction between meeting the most refined criteria of mathematical acceptability and actually being mathematically true" ([14], p. 7; cited on p. 65). Shanker again disapproves of this characterization. He says that, if we use it, the issue must then become, as it is for Dummett, whether "we can confer meanings on our statements which render them determinately true or false independently of our knowledge" ([5], p. xxviii; cited on p. 65). And Shanker again insists that for Wittgenstein there is simply no coherent issue here. His concern—which is coherent—is "rather . . . [with] clarifying how we are to draw the boundary circumscribing the class of 'mathematical statements'" (p. 65).

I confess I do not understand why this is considered to be a different issue. Perhaps Shanker has not stated clearly enough in this context that his description of Wittgenstein's "concern" is intended to capture something nonepistemo-

logical. At best, the discussion is elusive. And the subsequent remarks confuse matters further: "If . . . what Wright and Dummett are driving at is that according to the platonist a proposition can be well formed yet not decidable, then this is precisely the idea that Wittgenstein hoped to expose as incoherent: to be well formed is by *definition* to be decidable, and the notion of 'undecidability' is itself meaningless (cf. Chapter 3)" (p. 65, italics mine). Now Chapter 3's discussion of 'nonsensical conjectures' can be 'stretched' only so far. The present claim that conjectures are nonsensical *by definition* can hardly be Wittgenstein's view, even if it were consistent, which it is not, with Chapter 3's account of the nature of conjectures. Indeed, why not throw caution to the wind and affirm that conjectures lack meaning because, *via* some mysteriously efficacious definitions, we have 'judged' that they do?! Naturally, Shanker wants no part of this.

Some more helpful remarks do emerge a few pages later in the discussion of objectivity. Shanker explains, as many interpreters would agree, that Wittgensteinian rules are not binding in that they cannot "compel" or "force" us to follow them. They *are* binding in the important sense that "in order to play *this* game, we must follow *these* rules"—if one fails to *follow* such-and-such a rule in game *G*, one is not playing *G*, for we are not "at liberty to say that anything constitutes following an established rule" (p. 67). (Shanker strenuously rejects Wright's and Kripke's "skeptical" interpretations of Wittgenstein on rules.) This sets the stage for these useful remarks: "Although the actual existence of a rule may, so to speak, be dependent on us, the 'truth' of the rule is dependent on the rule itself. Assuming that it is not too confusing to speak of 'dependency' at all here, this leaves the far more important confusion of speaking of the truth of rules in the same way that we speak of the truth of empirical propositions, where the concept of objectivity finds its proper bearings" (p. 67). And two pages hence, we find this critical supplement: "What matters here is not whether it is 'legitimate' to describe mathematical propositions *qua* rules of logical syntax as 'true', but rather, in what sense this 'truth' should be understood; i.e., how this differs from empirical contexts" (p. 69).

Unfortunately, how the alleged 'truth' of rules of logical syntax differs from 'empirical truth' is never made precise. Given that the concept of objectivity "finds its proper bearings" in the area of empirical discourse, we need to know how we should understand 'objectivity' in the area of mathematical discourse. I find that this likewise does not become clear. True, Shanker is fairly clearly committed to translating the phrase "objective mathematical propositions" as "binding mathematical rules (of syntax)", with the relevant sense of "binding" being the second one above. We cannot assert *anything* we please in mathematics, since if we are playing, say, the game of *addition*, then we must either follow the game's established rules or fail altogether to *play*. Now this gloss on "mathematical objectivity" supports a specific interpretation of Shanker, viz., that he is interpreting Wittgenstein's 'mathematical propositions' as not really being propositions, but fundamentally *rules*. We can hereby make sense of Shanker's repeated insistence that these 'propositions' lack descriptive content. It is radically unclear how something which failed to *describe* at all could be a *proposition*.

If we are now to assess Shanker's account of mathematical objectivity, our main question must be whether his reliance on the "binding" character of math-

ematical rules makes objectivity any clearer. So far as I can see, it does not. For what we need is an analysis of 'rule of logical syntax'. Specifically, we need a better understanding of the relationship between those 'rules' which ostensibly give *meaning* to mathematical 'propositions' *via* proofs and the propositions proved (see Section 4 above). I do not think Shanker provides the relevant analyses. He speaks confidently of the "relations" that "*constitute* an expression as a mathematical proposition"; these are "internal" to the proposition. It "cannot be divorced from" them because they are "rules" which some proof of the proposition "construct(ed) for its use" (p. 111). The status of these constructed rules and how they are related to the rule ('proposition') which the proof proves is unclear.

It sounds as though these rules — if the meaning is the use — are what give the proved 'proposition' meaning, what *establish* the proved rule. But what in turn establishes the constructed rules themselves? Or is the case rather that the proved rule *consists* of the cluster of rules constructed during formulation of what was proved? If so, we still have to ask what it is about *proofs* that enables them to confer *objectively certain* meaning on their results (see the use of the phrase "objectively certain" *circa* p. 72). One cannot genuinely explain objectivity by appeal to a special feature of rules created by proofs, while failing to explain how proofs can be objectively relied upon to endow things with that feature. Very similar criticisms apply to Shanker's analysis of Wittgenstein's concept of mathematical necessity in Chapter 8.

7 Summary   There is much in Shanker's book that I have been unable to cover. There is a wealth of material of interest for many kinds of readers. These will include, as I should mention, even the most mathematical of philosophers of mathematics. It also must be emphasized that my discussion does not come close to being exhaustive.

My negative conclusions generally have concerned Shanker's occasional lack of precision, tendency to overstatement, and to my mind, failure to develop adequately some of his most original ideas. Hence, I cannot endorse the book as a persuasive *general* defense of the grammatical approach to Wittgenstein. Nonetheless, as I hope is clear, Shanker has shown the approach has promise, and more, perhaps, than recent Wittgenstein scholarship reflects. In this vein I have made special mention of his interpretation of the concept of surveyability (Chapter 4) and his discussion of Wittgenstein's view of consistency proofs (Chapter 6). The treatment of the relationship between mathematical propositions and their proofs is also quite inventive (Chapter 3).

As the book is long, it is a relief to find very few typographical errors. The index is passable. The bibliography, while not especially detailed, is adequate and contains useful references to some writings by mathematicians.

NOTES

1. Unless otherwise indicated, all page references are to Shanker.

2. Turing introduces the example of a bridge collapsing at the end of Lecture XXI in [12].

3. Shanker returns again and again to the "anti-revisionist" theme, that is, to the idea that Wittgenstein opposes any attempt on the part of nonmathematicians to evaluate whether a mathematical claim, or a mathematical proof, is correct.

4. An odd exception among insightful readers of Wittgenstein is Ziff (see [15], Section 28). Clearly, though, much more exegesis than what Ziff provides is required to settle the issue in his favor.

5. Contrary to Shanker, this is not terribly clear in, for instance, [13]. First, Wittgenstein rarely asks the question. He does ([13], V 1-4) raise the question, "Does a calculating machine *calculate*?" (V 2). But no unequivocal answer is given. Indeed, as the ensuing passages reveal, the point of the question is to raise a rather more basic one, viz. the question whether actions may count as "mathematical", e.g., as *calculations*, in cases where the action's 'agent'—human *or* nonhuman—lacks understanding of *applications*. The issue is not whether something nonhuman can perform calculations, but whether it is possible to 'do mathematics' without grasping any relation between what one does to things not involved in what one does. In one sense, Wittgenstein is exploring the relationship between mathematical operations, formalistically conceived, and the interpretation of mathematics.

REFERENCES

[1] Chihara, C., "Wittgenstein's analysis of the paradoxes in his *Lectures on the Foundations of Mathematics,*" *Philosophical Review*, vol. 86 (1977), pp. 365-381.

[2] Chihara, C., "The Wright-wing defense of Wittgenstein's philosophy of logic," *Philosophical Review*, vol. 91 (1982), pp. 99-108.

[3] Detlefsen, M. and M. Luker, "The four-color theorem and mathematical proof," *The Journal of Philosophy*, vol. 77 (1980), pp. 803-820.

[4] Dummett, M., "Wittgenstein's philosophy of mathematics," *Philosophical Review*, vol. 68 (1959), pp. 324-348.

[5] Dummett, M., *Truth and Other Enigmas*, Duckworth, 1978.

[6] Dummett, M., "Realism," pp. 145-165 in *Truth and Other Enigmas*.

[7] Gasking, D. A. T. and A. C. Jackson, "Wittgenstein as a teacher," pp. 49-55 in *Ludwig Wittgenstein: The Man and His Philosophy*, edited by K. T. Fann, Harvester Press, New York, 1967.

[8] McGuinness, B. F. (editor). McGuinness, B. F. and J. Schulte (translators), *Ludwig Wittgenstein and the Vienna Circle: Conversations Recorded by Friedrich Waismann*, Blackwell, Oxford, 1979.

[9] Wittgenstein, L., *Tractatus Logico-Philosophicus*, translated by D. F. Pears and B. F. McGuinness, Routledge & Kegan Paul, London, 1961.

[10] Wittgenstein, L., *Philosophical Grammar*, edited by R. Rhees, translated by A. Kenny, Blackwell, Oxford, 1974.

[11] Wittgenstein, L., *Philosophical Remarks*, edited by R. Rhees, translated by R. Hargreaves and R. White, Second edition, Blackwell, Oxford, 1975.

[12] Wittgenstein, L., *Lectures on the Foundations of Mathematics: 1939*, edited by C. Diamond, Harvester Press, 1976.

[13] Wittgenstein, L., *Remarks on the Foundations of Mathematics*, edited by G. H. von Wright, R. Rhees, and G. E. M. Anscombe, translated by G. E. M. Anscombe, Third Edition, Blackwell, Oxford, 1978.

[14] Wright, C., *Wittgenstein on the Foundations of Mathematics*, Duckworth, 1980.

[15] Ziff, P., "Remarks on Wittgenstein's *Remarks on the Foundations of Mathematics*," *Synthese*, vol. 56 (1983), pp. 351–361.

Shelley Stillwell
Department of Philosophy
Illinois State University
Normal, Illinois 61761-6901