A Reply to Dummett on Wittgenstein

Although I have a generally high opinion of Michael Dummett's contributions to the philosophy of mathematics, and also to the philosophy of language, I find his criticism of Wittgenstein's views about mathematical proof (Encounter, March 1978) very unconvincing. It appears to be based upon a faulty picture of how mathematical arguments, and also philosophical ones, provide us with certain beliefs. Dummett chastises Wittgenstein for not paying enough attention to what mathematicians actually do or to the point of what they say and do. But it appears that, in attempting to make this charge stick, Dummett himself blurs the distinction between what mathematicians do and what they believe about what they do. Admittedly, the distinction is not always easy to maintain but it nevertheless behooves us to try.

The picture of pure mathematics that emerges from Dummett's remarks is that of an activity of proving mathematical assertions and also, occasionally, discovering a mistake in an argument that hitherto had been acknowledged to be a proof. If an argument is correct then the result it establishes is 'genuine' and there can be no counterexample to it. The reason we do sometimes find a counterexample to an assertion that had been acknowledged to be proved is that we sometimes make a mistake and think we have proved a theorem when in fact we have not.

Although it may seem difficult to find anything objectionable about such an account one can, I think, make considerable progress in that direction merely by trying to take seriously what it would be like to question whether some particular mathematical result is a 'genuine' one. The spurious notion of a 'genuine' mathematical result appears to inhabit the same ethereal domain as Popper's notion of an 'objective' truth and to be equally irrelevant for the description of science. Perhaps it is no coincidence that each is put foward under the heading of a 'guiding' or 'regulative' principle.

When Dummett talks in his article about mathematicians proving theorems, or discovering counterexamples, or finding mistakes in proofs, he is not being at all ethereal. On the contrary, it is plain that he is talking about the performance of certain cognitive acts - more precisely, about the performance of certain linguistic acts that are recognized as being of a particular type: a proof of a theorem, a discovery of a counterexample, etc.. Even when he asserts that "there subsequently turned out to be counter-examples" to theorems that for many years had been accepted as proved, he means only that we now have what we unquestioningly take to be counterexamples. And we do so because we recognize them as such. Therefore, when Dummett purports to explain how there can "turn out to be" counterexamples to theorems that have been accepted as proved, the phenomenon for which he is required to account is that of mathematicians finding what they take to be counterexamples to assertions they previously had taken to be proved.

A straightfoward explanation of the phenomenon just described is that, although the immediate experience of proving a theorem is completely convincing and leaves us with the belief that we can get back into such a state of conviction by rereading the proof, it nevertheless does not seem to transform us in such a way that we become constitutionally unable to have, later on, an experience of finding a counterexample - that is to say, an experience of what we take to be finding a counterexample. Although Dummett does not address this explanation directly, judging from what he says in his article it may be presumed that he would level the same charge against it that he does against Wittgenstein's - namely, that it is faulty because it fails to take into account that the objective reason there can be counterexamples to theorems that are accepted as proved is that people can make mistakes in proof and believe they have succeeded when in fact they have not. ~~(If a theorem really has not been proved then of course it is not too surprising if there turns out to be a counterexample.)~~

But how are we to take such a claim? Dummett presents it as a self-evident truth, which it surely is not. Possibly he does so because he anticipates that his readers will see it as such; however, Dummett is arguing against Wittgenstein, not against his readers. In an earlier part of his discussion, Dummett himself does an excellent job of showing the reader why Wittgenstein's objections to traditional views about mathematical proof cannot be disposed of so easily as is commonly believed. Why then does he believe that he can refute Wittgenstein on this very same subject merely by reaffirming one of the traditional beliefs against which Wittgenstein's critique is in fact most telling? The belief that whenever we find a counterexample we also can find a mistake in the proof is, if anything, even more vulnerable to Wittgenstein's criticism than the one discussed by Dummett in the earlier part of his article. Presumably Dummett insists on such a view because of his more fundamental belief that "we are in principle capable of recognizing a proof as correct or incorrect." But I know of no good reason for sharing this belief and Dummett does not offer any.

Dummett's own explanation, that there can be counterexamples because there can be mistakes in proof, would indeed be undisputable if it happened to be a feature of mathematical practice that no apparent counterexample to a theorem may be admitted to be correct until one has located a mistake in whatever argument, prior to the discovery of the counterexample, had been recognized as being a proof. I do not know whether Dummett actually believes this to be a feature of mathematical practice but he does assert in his article that if we should hit upon an apparent counterexample to a certain assertion that we now believe to be proved then "we should know that there must be a mistake either in the proof of the theorem or in the identification of the alleged counter-example, and should set about to discover which." Although it probably is true that in the great majority of cases we should do just what Dummett says, with regard to the question that is treated in his article the more important point is that there also are cases in which we should not and are not required to proceed in that way. What Dummett seems to overlook is that, for a counterexample to a theorem to be accepted as correct, all that is necessary is that the argument that formerly had been seen as a proof of the theorem no longer be found convincing. And that can happen without anyone finding a mistake in the argument, even without it being believed that 'in principle' one could find such a mistake. Imagine, for example, a case in which there is a very simple counterexample to a theorem the proof of which is no longer a part of anybody's working knowledge and moreover has a reputation of being exceedingly tricky and complicated. In such a case the mathematician has no obligation to go back over the argument that is on record 'in the archives' and locate some specific mistake in it; it is enough that the argument no longer is seen as counting for anything. Furthermore, even if the mathematician does go back over the argument, he may not see it as counting for anything even if he does not succeed in locating a mistake in it. I have in mind a case in which, for whatever the reason, a series of attempts to master the argument has produced nothing but confusion and a sense of disbelief that one did once master it well enough to find it convincing. Confronted with such a situation, what point can there be in continuing to insist that 'in principle' a mistake could be found?

It is true that, even in a case of the sort just described, if anyone should ask us how it can happen that we now admit there to be a counterexample to an assertion that once was seen by us as being proved, we should answer just as Dummett says: "We made a mistake." But if someone were to go further and ask how we know that we made a mistake, very likely our immediate response would be "Because we have found a counterexample." In other words, our answer would be more in accord with Wittgenstein's picture of mathematics than with Dummett's for we would, in effect, be adopting a new criterion as to what constitutes a demonstration that 'there is a mistake in the proof.'

Dummett concludes his discussion by accusing Wittgenstein of having "allowed himself to fall into the trap of offering a new explanation of this alleged certainty of mathematical results, instead of exposing it as spurious." But it seems rather that Dummett has fallen into the trap of mistaking an idealized picture of mathematics - an oversimplification that currently serves us well enough so long as we do not insist on maintaining it in cases in which it evidently does not apply - for a normative description of the discipline itself. At the present time, pure mathematics has an appearance of great stability. A very sizable body of belief has been built up and it is a matter of record that, for those theorems that have been 'put into the archives,' the discovery of a counterexample or a mistake in proof actually is a rather rare event. It also is a matter of record that, in those cases in which it has been deemed important, the discovery of a counterexample has indeed been followed by the location of a mistake in the argument that previously had been accepted as a proof. Moreover, during the contemporary period there do not appear to be any significant cases in which the discovery of the counterexample and the subsequent discovery of a mistake in the proof were themselves found, later on, to be incorrect. Under such circumstances there inevitably is a temptation to regard such good fortune as a proof that 'we must be doing something right' and to elevate this belief to the status of an official doctrine. However, in contemporary mathematics as elsewhere, it may be the better part of wisdom to enjoy such good fortune while it lasts and not to insist on making more out of it ~~than actually is there~~.

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by Stokely Carmichael and Peter Sedgwick, for not being political enough; but they may not appreciate where he stands. With fine rhetoric or trembling rage, Laing has tried to stare down a century of naturalisation into false consciousness. He may not succeed; he may, for all I know, have given up in bitter defeat. But the task was one for heroes.

Reckonings

Wittgenstein on Mathematics—By MICHAEL DUMMETT

IT WAS EARLY IN 1950 that philosophers of my generation in Oxford, those, that is to say, who were undergraduates or graduate students in the immediately post-War years, first saw any of Wittgenstein's work other than the *Tractatus* and "Some Remarks on Logical Form." In that year, three of Wittgenstein's later works suddenly arrived in Oxford in the form of typescripts: they were the Blue Book, the Brown Book and the Notes on the Mathematics Lectures, the latter in what has turned out to be Bosanquet's version, one of the four from which the present volume[1] has been edited. The first two had been dictated by Wittgenstein for circulation among a select group; the third was merely one person's record of a set of lectures Wittgenstein had given. All three circulated in Oxford with extreme rapidity: there being no xerox machines, the system was for everyone who obtained a set to find five other people who wanted one, engage a typist to type out the entire work with four carbons, and then sell them at cost price.

It is difficult to convey the excitement of reading these works for the first time. Wittgenstein was a distant presence of which we were all intensely aware, but an utterly enigmatic presence. I came up to Oxford, out of the army, just one term after the celebrated meeting of the Jowett Society, in the summer of 1947, which Wittgenstein attended and at which he talked at great length, and so had never even seen him. We were all vividly conscious of his being there in Cambridge, giving classes to selected audiences, and conscious also of the atmosphere of secrecy that surrounded him; we believed that he was probably a great genius, revealing to those fortunate enough to be admitted to his lectures a dazzling and completely original treatment of philosophical questions: but we did not know what it was that he said. There were, indeed, disciples of his, like Wisdom, whose writings we read, and Miss Anscombe, whose lectures we heard; but we could not be sure how faithfully they represented their teacher. And then, suddenly, by what channel I never knew, these works arrived, smuggled into Oxford from that city that had been as closed to us as Lhasa.

On me, at least, their impact was tremendous. The force of the personality came through even the pages of blurred typescript: for weeks after I first read them, all that would come out, whenever I attempted to write any philosophy, was a ridiculous pastiche of Wittgenstein, the stylistic mannerisms if not the substance. Later, when, after Wittgenstein's death and the publication of the *Philosophical Investigations*, the secrecy had been dissolved, I remained extremely grateful, not only to whoever had circulated the Blue and Brown Books, but also to the person who had pirated the Notes on Mathematics; to the extent that, when it was published, I understood the *Remarks on the Foundations of Mathematics*, textually quite distinct from the Notes but frequently overlapping in philosophical content, I did so because I had previously read and re-read the Notes.

BOSANQUET'S NOTES, which were written in the third person, had a peculiar charm, due in part to that convention. They opened with the words,

> How is it that Wittgenstein, who is a philosopher and knows little about mathematics, has a right to talk about the foundations of mathematics?

In Miss Diamond's version, conflated from four sets of notes, this appears as:

> I am proposing to talk about the foundations of mathematics. An important problem arises from the subject itself: How can I—or anyone who is not a mathematician—talk about this? What right has a philosopher to talk about mathematics?

No doubt her version is much closer to the words actually uttered on 23rd January 1939; but I do not think it is just sentimental attachment to something that had a great effect on me that leads me to think that some of the charm has gone. It

[1] *Wittgenstein's Lectures on the Foundations of Mathematics, Cambridge, 1939.* Edited by CORA DIAMOND from the notes of R. G. BOSANQUET, NORMAN MALCOLM, RUSH RHEES and YORICK SMYTHIES. Cornell University Press, $18.50; Harvester Press, £12.50.

appears, too, that Bosanquet's version was incomplete: there were four further lectures. All the same, it finished marvellously:

> This is most important. It has puzzled Wittgenstein more than he can say, how there can be a short-cut through logic. For the moment he will leave us puzzled.

The end in Miss Diamond's version—the end of a lecture not included in Bosanquet's—sounds authentic, but is less delightful; an abrupt sentence reading:

> The seed I'm most likely to sow is a certain jargon.

Miss Diamond has cobbled together this version of Wittgenstein's lectures from four sets of notes, all of them incomplete, although it is amazing to me that anyone was able to take such copious notes as to make the exercise possible at all. There are some thirty-odd passages where she reports that the versions differed substantially and that she has therefore had to guess at the original form of the remarks. Some might protest that they should have been given the texts of the divergent notes in these cases, so as to estimate the original intention for themselves, but I think she was right not to inflate the length and price of the book in this way: perhaps, however, for the sake of the scholarly, transcripts of all the notes ought to be deposited in some university library.

On the whole, I should judge that Miss Diamond has done an excellent job. Nevertheless, we should not be deluded into thinking that we are reading Wittgenstein, not even, as Miss Diamond warns us, unrevised Wittgenstein. The process of transmission has been too indirect to justify any such idea. Rather, it is as if we were standing outside the closed door of a lecture room, occasionally unable to catch what is being said inside, most of the time hearing a muffled voice which we can largely follow, sometimes sure that we must have misheard, never able to be quite certain that we have heard aright.

FRANK RAMSEY (who was so enormously influenced by Wittgenstein's earlier work, the *Tractatus*) accused the intuitionists, Brouwer and Weyl, of introducing Bolshevism into mathematics; and he meant the accusation quite seriously—for him, their work was as subversive of cherished traditional practices as was Bolshevist propaganda to Western capitalists. Hilbert, similarly, in a famous cry of defiance to the intuitionists, declared, "No one is going to turn us out of the paradise Cantor has created." Wittgenstein's comment is characteristic:

> I wouldn't dream of trying to drive anyone out of this paradise. I would try to do something quite different: I would try to show you that it is not a paradise—so that you'll leave of your own accord.

He nevertheless feels it necessary to protest that *he* is not introducing Bolshevism into mathematics. Ramsey, no doubt, would have agreed with this; if Brouwer and Weyl were Bolsheviks, then Wittgenstein, in his later phase, was an anarchist.

Consider an arithmetical predicate like "is the sum of two primes." In the view both of a classical mathematician and of an intuitionist, for each natural number, such a predicate either applies to it or does not apply to it: for the classical mathematician, this holds simply because the predicate is well defined; for the intuitionist, it holds because the predicate is decidable—we have an effective means, in principle, for deciding whether or not it applies to any given number. There is disagreement between them, however, over the way we give meaning to a statement involving generalisation over infinitely many natural numbers—for instance Goldbach's conjecture that every even number greater than 2 is the sum of two primes.

The classical mathematician conceives of the matter essentially as follows. Suppose that we assign 1 to every number to which the predicate applies, and 0 to every number to which it does not. Then, if we multiply together all the numbers so assigned to even numbers greater than 2, we shall arrive either at the number 1, in which case Goldbach's conjecture will be true, or at the number 0, in which case it will be false; the conjecture is therefore determinately either true or false.

For the intuitionist, this conception embodies a barbaric mistake about the nature of an infinite process: we cannot specify a determinate number as the final outcome of an infinite process, because an infinite process is by definition one which can never be completed. Hence we can have no conception of the truth of Goldbach's conjecture save as consisting in the existence of a proof of it, a procedure for showing, of each even number greater than 2, that it satisfies the predicate, nor of its falsity save as consisting in a means of deriving a contradiction from the supposition that we could prove it. Since we have no guarantee that we can either prove or disprove the conjecture, we cannot assume that it is either true or false.

WITTGENSTEIN AGREES with the intuitionists both about infinity and about mathematical truth ("What is the criterion for its being so—if not the proof?"); but he has an independent reason for denying that Goldbach's conjecture is a proposition having a determinate truth-value independently of our having a proof or disproof of it. He supposes that, when we multiply very large numbers, we find that we simply cannot attain agreement over the result; and he argues that, in such a case, there simply would not be a right result, a result that God would know, even though we could never be sure of it, since "to say that something is the right result is to say that we acknowledge it. . . . There is nothing for a higher intelligence to know . . . we know as much as God does in mathematics."

Now this is not a mere hypothetical case, it is the actual case: there is a bound on the complexity of computations we can in practice carry out at all, and a bound on the complexity of those we carry out with certainty of reaching agreement. It follows that such a predicate as "is the sum of two primes" cannot be regarded as definitely applying or failing to apply to every number, however large. There will be numbers too large for us to be able to decide whether or not the predicate applies to them; and it is useless to say that the predicate is true of such a number provided that, if we *were* able to perform the computation, we *should* decide that it was, or to say that God must know what the correct outcome of such a computation would be. There is, according to Wittgenstein, nothing for God to know; there *is* no right answer.

From such a standpoint, there is an objection to the classical conception of generalisation over an infinite domain even more direct and telling than the intuitionists' considerations about infinite processes, namely that there is not really a definite truth-value attaching to each statement of the form "*n* is the sum of two primes." We have not really succeeded in associating either the number 1 or the number 0 to each number *n*, according as the predicate does or does not apply to it; and so, of course, it is nonsense to talk about the product of all the infinitely many numbers, each equal to 1 or to 0, which we have in this way associated with the even numbers greater than 2.

This is not to say that Wittgenstein draws the conclusion so boldly and assertively drawn by the intuitionists, that classical mathematicians reason incorrectly. He says from the outset that it is "most important not to interfere with the mathematicians"; he is very reluctant to say of anyone, "They reasoned wrongly." He is not going to drive anybody out of a paradise; he will simply point out that it is not a paradise, and then they will leave of their own accord. But it does not in practice seem to me to make a great deal of difference; a mathematician who really believed what Wittgenstein says could hardly continue to employ classical reasoning.

WITTGENSTEIN'S IDEA that God does not know any more mathematics than we do (except in so far as He can foresee what we are going to do—since, apart from that, there is nothing to be known; that is, since truth attaches to a mathematical statement only in virtue of our acknowledging it as true) is, for him, a consequence of his general reflections concerning rules. Those reflections figure prominently in the *Investigations*, and the present lectures start from them. The existence of a rule—in particular, a rule of computation or one governing the use of a word or symbol—rests ultimately upon the fact of agreement in practice, amongst human beings who have been taught the rule, over its application, a fact not susceptible of further explanation.

Our natural reaction to such a thesis is to protest that one who has genuinely mastered the rule grasps the general principle underlying its applications. Wittgenstein of course allows that, very often, the mastery of a rule is attained by getting to know a formula or general formulation, and, in other cases, is manifested by the ability to frame such a general formulation. But every formula, every general statement of the rule, could still be misapplied, and we should not say that someone able to give the formula knew the rule unless he applied it as we all do; and we cannot without circularity or an infinite regress suppose that, in each case, someone's knowledge of how to apply a formula consists in his knowing another formula.

Faced with this, we are inclined grudgingly to admit that, in the last resort, Wittgenstein is right, but to suppose that that concession does not make much difference. Wittgenstein's example of the large multiplication sums shows, however, that he took it as making an enormous difference: even multiplication is well defined only for numbers which we can in practice multiply with confidence, because only in that case is there the agreement in practice which is needed to supply an application for "correct" and "incorrect."

WHY ARE WE INCLINED to think that conceding Wittgenstein to be right in the last resort does not make much difference to anything? I think that this is probably because we are impressed by the fact that any computation is a sequence of operations, each of one of a restricted number of kinds. A multiplication sum, for instance, consists in repeated multiplications and additions of single-digit numbers. Suppose that we grant Wittgenstein's thesis for the rules governing the basic constituent operations of a computation-procedure: we concede that, for each instance of any such operation, we might have regarded something quite different as the analogue of what we have done in other cases, and that nothing makes it the true analogue save the fact that we in practice concur in so regarding it.

All the same, what entitles us to speak of a rule governing the operation is the fact, repeatedly emphasised by Wittgenstein, that we do not in practice disagree over how it is to be applied; and this holds good just as much for an instance of such an operation embedded in an enormous computation as for any other. Suppose that we are concerned with a vast multiplication sum, which it takes the most skilled human calculator a month to do by hand; and suppose that we find that, however many individuals perform the same calculation, and however many times each checks and re-checks his working, they just cannot reach agreement on the correct answer. It is the nub of Wittgenstein's position to infer that there is then no such thing as *the* correct answer (and hence no correct answer either to the question whether a number of the order of magnitude of the product is prime or composite).

But we are inclined to reason as follows. Consider any one attempt at the multiplication sum. It consists of an immensely long sequence of operations of a kind familiar to a child at primary school. If we isolated any one such operation, we should have no difficulty in obtaining agreement over whether it had been performed correctly or incorrectly; the difficulty arises only because of the length of the computation as a whole. It is therefore an objective matter whether or not each of the constituent operations has been performed correctly, even though the criterion for its correctness rests solely upon our propensity to acknowledge it as correct. But the sum as a whole is correct just in case each of its constituent operations is correct. Hence God must know, even though we cannot, whether the sum has been done correctly or not; God must know what the right answer is.

WITTGENSTEIN'S CONSTANT ADMONITION appears to be, "Do not etherealise; do not hypothesise general principles to which we are striving to conform, or imagine an external reality which, by various indirect means, we are trying to uncover: but look at what actually happens, at what we actually do." No one should doubt that this is often good advice to give; the question is whether it is not sometimes the wrong advice. I did not cite the foregoing objection to Wittgenstein's thesis about multiplication as a knock-down argument, but only as a diagnosis of our inclination to suppose that we can concede his fundamental point concerning rules and then carry on much as before. We are disposed to think that there may be some basic rules, out of which all other rules are compounded, and to which Wittgenstein's thesis may apply, but that, given those basic rules, what we normally believe about the complex rules compounded out of them remains unaffected.

I do not think that this simple way of protecting ourselves from the anarchy that Wittgenstein's views appear to threaten is satisfactory; for of course the rules governing, e.g., multiplication do not reduce without residue to those governing the basic operations of multiplying and adding single-digit numbers, but inescapably involve also those determining the order in which these basic operations are performed. It would be the same for any procedure which we should be unable to carry out accurately, or at all—counting a football crowd, for example. It is possible to break the procedure down into sub-operations so simple that no doubt could arise over the execution of any one of them; but the complexity then reappears in the rules determining which operation has to be performed at any point.

TO SAY THAT Wittgenstein's arguments cannot be swiftly dismissed or restricted in application to a harmless range of basic operations is not to grant that his conclusions carry conviction. He is, of course, well aware that, when simple methods cease to be reliable, more sophisticated ones are invoked. No one can stand on a football ground and count the crowd in the stadium, but it can be counted mechanically by turnstiles at the exits; even the population of London can be counted in a census. Multiplications that we cannot perform can be carried out, if required, by an electronic calculator or computer. We are not inhibited from measuring the distance from the Earth to the Sun by the fact that we cannot place footrules between them.

The question is not the practical one of whether we can find ways of giving answers, but the theoretical one of whether the primitive methods that we use to give answers in simple cases already determine what constitutes the correct answer in every case. Our overwhelming inclination is to believe that they do: that anyone who knows how to count already understands what would be meant by using a 20-digit number to give the number of stars in the universe; that anyone who knows how to show that 103 is prime already knows what would be meant by saying that that 20-digit number is prime; that distance is distance, whether it is a micron, a mile or a million light-years.

We think this, not merely because of the empirical fact that different methods give the same answers in overlapping areas of applicability, but because we see them as governed by the same underlying principles. Whether a human being counts or the turnstiles click, a one-to-one correlation is being established with an initial segment of the numerals; the computer simulates our own procedures of calculation. Wittgenstein is unimpressed by the conception of guiding principles. For him, what gives sense to the question is that it has a correct answer, and what makes an answer correct is that we are able to agree in acknowledging it as correct. Since any method of obtaining an answer is applicable only over a certain range, the meaning of the question can never be fully determined for every possible case.

I DO NOT BELIEVE that Wittgenstein's arguments can be easily rebutted; but I also do not believe that he does justice to the strength of the objections to them. This is odd, because, in other connections, he frequently insists on the necessity of attending, not merely to what is said or done, but to the *point* of what is said or done; he might, therefore, be expected to sympathise with the idea of a guiding principle to the extent of distinguishing between the details of a procedure and its point, which it could share with other procedures differing in detail. I shall not here further attempt to reconcile what is sound in Wittgenstein's arguments with what is sound in the objections to them, and indeed, I am unsure how to do so; but such a reconciliation is required if this matter is to be resolved. At present, philosophers largely ignore this cluster of Wittgenstein's ideas, because they perceive that they cannot be wholly correct, and because they run counter to the prejudices that philosophers have in common with everyone else; but, even if, as I think, there is something wrong with the conclusions Wittgenstein drew from those ideas, justice should be done to them.

MORE GENERALLY, it is plain that Wittgenstein did not handle satisfactorily the notion of a mistake in applying a procedure or a rule. In the early lectures, much use is made, as an example, of Gauss's proof of the impossibility of constructing the regular heptagon with ruler and compass. Wittgenstein says that this proof does not show that one might not, in practice, by fiddling about with a ruler and compass, produce a regular heptagon. The mathematician A. M. Turing, who was present at the lectures, and whose comments are about five times as numerous as those of any other member of the audience, remarks correctly that what it shows is that we cannot give instructions for constructing a regular heptagon. But Wittgenstein denies this too: "I might give instructions to someone and he might go on constructing heptagon after heptagon." What the proof does is to persuade us to exclude the expression "construction of the regular heptagon" from our notation: it does not assure us, of some conceivable event, that it will never occur; it imposes a certain restriction on how we shall be prepared to describe whatever occurs.

This is in line with the whole thrust of much of Wittgenstein's discussion: he wants to persuade us to adopt a view of mathematics, not as demonstrating substantial results about what is and is not possible, but as creating paradigms in terms of which we describe things, an ever more systematic framework of description. In spite of his several disclaimers ("I have no right to want you to say anything"; "I have no point"), it is plain that, in this, Wittgenstein had a vision of what he took mathematics to be, a vision that he tries again and again to communicate to his audience.

But, in the heptagon example, it is clear that Wittgenstein does not succeed in making out his case. It is true that the existence of the proof of non-constructibility does not render it inconceivable that we should hit on a set of instructions apparently enabling us to construct regular heptagons: but that is because there might be a mistake in the proof. It has happened that theorems have for many years been accepted to which there subsequently turned out to be counter-examples; we can seldom rule out the possibility that there is a mistake in what has been acknowledged as a proof. If such a set of instructions were to be devised, we should have an apparent counter-example to Gauss's theorem about which regular polygons are constructible; we should know that there must be a mistake either in the proof of the theorem or in the identification of the alleged counter-example, and should set about to discover which.

It thus seems that Wittgenstein confuses the *a priori* character of mathematics—the necessity of its results whenever they are genuine—with certainty, which is not a general characteristic of it; because we are in principle capable of recognising a proof as correct or incorrect, he assumes that we never accept a proof unless we are not merely assured but *certain* of its validity, and hence are prepared to "put it in the archives" and maintain it against all counter-evidence. Turing rightly insists on the possibility of error, but Wittgenstein fails to take his point: on one occasion, when the immediate topic was the multiplication of three-digit numbers (in which it is easy to make mistakes, though also easy to spot them), he responds to Turing's observation by shifting the example and asking what it would be to find that we had always been mistaken in saying that $12 \times 12 = 144$.

IT HAS BEEN COMMONPLACE to express the conclusive character of mathematical proof by saying that only in mathematics do we attain certainty; and it seems that Wittgenstein allowed himself to fall into the trap of offering a new explanation of this alleged certainty of mathematical results, instead of exposing it as spurious. We cannot but contrast unfavourably his imaginary accounts of the cavalier manner in which mathematicians would treat hypothetical apparent counter-examples with the careful descriptions by Imre Lakatos in his *Proofs and Refutations*[2] of how they have in fact treated actual ones. I am not recommending Lakatos's philosophical conclusions any more than Wittgenstein's; but they have the merit of really having been based on seeing what we actually do, as, despite his advocacy of that way of proceeding, Wittgenstein's do not.

Wittgenstein's vision of mathematics cannot, I believe, be sustained; it was a radically faulty vision. Nevertheless, although this book is necessarily an imperfect reconstruction of Wittgenstein's words, and although those words were impromptu and unrevised, they were words spoken by a profound philosopher who devoted a great part of his time throughout his life to thinking about mathematics. It therefore contains much more gold than I have been able here to indicate, gold which it will take much intellectual labour fully to extract. The English into which Miss Diamond has rendered the four sets of notes she was using is undistinguished and surely does not bear the stamp of Wittgenstein's personality, as all his writing does; but at least we are spared split infinitives and similar inappropriate Americanisms, save for the inescapable "I would/will" for "I should/shall", a solecism which, for all I know, Wittgenstein himself may have committed. There are many irritating features, not least Wittgenstein's inability to acknowledge that anyone else is right; it happens several times to Casimir Lewy, in particular, to say things that Wittgenstein himself is going to say a page or two later, only to have as Wittgenstein's immediate reaction a change of subject (see, *e.g.* p. 41, or pp. 45 and 47). But, with all these defects, the book is one that must absorb anyone interested in the philosophy of mathematics, particularly since the expression of Wittgenstein's thought is far more direct and unguarded than in the *Remarks*; and we owe much gratitude to Miss Diamond for her labour in producing it.

[2] Imre Lakatos, *Proofs and Refutations: The logic of Mathematical Discovery* (Cambridge University Press, 1976).