Wittgenstein on mathematics

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The philosophy of mathematics was one of Wittgenstein's central concerns from the beginning of his philosophical career until close to the end: it is what he told Russell he wanted to work on when he came to Cambridge in the Autumn of 1911; it is one of the issues addressed in the *Tractatus*; it dominates his writings immediately after his return to philosophy, the *Philosophical Remarks* and *Philosophical Grammar*; it was a recurring theme of his Cambridge lecture courses throughout the 1930s; it was to have been the subject of the second half of the *Philosophical Investigations* as he originally conceived them; and it occupied much of his time during the Second World War. Yet Wittgenstein has had nothing like the influence on contemporary philosophy of mathematics as he has on the philosophy of mind or of language. Although all of Wittgenstein's philosophy no doubt presents difficulties to the interpreter, the secondary literature on his philosophy of mathematics is peculiarly inconclusive even when it is judged by these standards. Why is this? In what follows I shall be describing some of Wittgenstein's views in the hope that this may contribute to answering that question.

After a brief sketch of the central ideas of Wittgenstein's account of mathematics in the *Tractatus*, most of the chapter will be devoted to his middle period writings in the philosophy of mathematics, focusing on his rejection of the actual infinite and on his suggestion that the meaning of an arithmetical generalization is its proof. One aim will be to show how this led him to some central themes of his later writings, his attitude to formal inconsistency and the rule-following considerations. Then I will say something about Wittgenstein's later philosophy of mathematics, such as it is. I shall conclude by offering a tentative diagnosis of its evident incompleteness.

1 Mathematics in the *Tractatus*

Very little is known about the early development of Wittgenstein's views on mathematics, but perhaps an anecdote later reported by Geach gives us a clue.

> The last time I saw Frege, as we were waiting at the station for my train, I said to him, 'Don't you ever find *any* difficulty in your theory that numbers are objects?' He replied, 'Sometimes I *seem* to see a difficulty, but then again I *don't* see it.' (Anscombe & Geach 1961, p. 128)

The last time Wittgenstein saw Frege was probably around Christmas 1913. So it seems that by then Wittgenstein had already rejected Frege's view that numbers are objects. If so, he had pinpointed what is surely a weakness in Frege's *Grundlagen*: the book depends crucially on this view, and yet it contains hardly any argument to show that it is true. Or, to put the point in a more linguistic mode, although Frege notes correctly that there are two kinds of uses to which number-words are put—as nouns and as adjectives—he simply assumes that it is the substantival use that is primary, the adjectival use derivative.

It would be wrong to put too much weight on an anecdote: not everything Wittgenstein's friends from his later life reported him as saying has turned out to be accurate. And the date of this exchange is perhaps a little surprising: Wittgenstein's surviving writings from before the war display no direct engagement with the nature of arithmetic at all. However, many of Wittgenstein's other central philosophical beliefs seem to have come to him very early indeed, so on that ground at least it would not be incredible if he had already come to deny that numbers are objects by the end of 1913.

In that case, however, Wittgenstein must have come to this view very early indeed. For not only do his letters from Norway that Autumn show no sign of a concern with the nature of numbers, but this is not a topic that is mentioned in the *Notes on Logic*, the summary of his work that he prepared for Russell in October 1913. If, as I have argued elsewhere (Potter 2009), the *Notes* summarize all that Wittgenstein thought worth preserving of his work from perhaps February of that year onwards, it suggests that the view about numbers was already in place before that.

'The theory of classes,' Wittgenstein says in the *Tractatus*, 'is altogether superfluous in mathematics. This is connected with the fact that the generality which we need in mathematics is not the accidental one.' (1922, 6.031) At first sight, it is a little surprising to find Wittgenstein treating it as a strike *against* logicism that the generality of mathematics is not accidental. We might grant readily enough that it is worth distinguishing between generalizations that just happen to be true and those that are true necessarily. But if mathematics belongs on the necessary side of this distinction rather than the contingent one, that surely places it with logic rather than against it.

However, this is to misunderstand the kind of accident that Wittgenstein had in mind. In fact, there is an important argument against Russellian logicism encapsulated in 6.031. That argument is in two stages. First, we note that the notion of class that can be derived from logic is the accidental, not the essential kind. Russell's idea had been to reduce classes to propositional functions (the so-called 'no class' theory), and hence to allow us to talk about the 'accidental' class $\{x{:}\phi(x)\}$ (accidental because it has as its members just those objects which happen to satisfy ϕ). What Russell's reduction could not do was to legitimate talk about 'essential' classes such as $\{a,b\}$ (essential because what its members are does not depend on the properties those members happen to have). Second, Wittgenstein claimed that mathematics is not in this sense accidental and hence cannot be based on the accidental kind of class. Since these are all that Russellian logicism has to offer, it must fail.

It is perhaps worth noting parenthetically, however, that despite its central role in his rejection of logicism, Wittgenstein's claim that 'the generality which we need in mathematics is not the accidental one' receives no more justification in the *Tractatus* than does Frege's claim that numbers are objects in the *Grundlagen*. And although it is no doubt an initially appealing view, it does at any rate stand in need of justification. The contrary idea that mathematics, although in some sense necessary, might nonetheless depend somehow on the world is not confined to the writings of out-and-out empiricists. David Lewis, for instance, chose to define the null set as the fusion of all individuals, a definition which makes mathematics depend on the world. Indeed on his account mathematics would be vulnerable if there were no individuals at all. 'In that case,' he insouciantly noted, 'maybe we can let mathematics fall. Just how much security do we really need? '(1991, p. 13)

Wittgenstein's argument against Russellian logicism is unlikely to date from quite as early as his argument against Frege's conception of numbers as objects. After all, if Wittgenstein had already rejected logicism by 1913, it would surely be a little odd that that Summer Russell still thought of him as a suitable person to revise the first two parts of *Principia*. It seems more likely to me that the rejection came about in Norway the following year.

I shall not devote much space here to the positive account of arithmetic in the *Tractatus*, since I have discussed it at length elsewhere (2000, ch. 6). In essence, Wittgenstein's idea was that numerals are not names of objects but merely indices (labels) used to mark the number of iterations of what he called an operation—a process for deriving one proposition from another. What is important for our purposes about the account in the *Tractatus* is in any case not its exact details but the place it gave to arithmetical equations as attempts to encapsulate in symbolic form the tautologousness of various propositions. Thus the equation 2+2=4 encapsulates such things as the necessity that if there are 2 apples and 2 oranges in the bowl, then there are 4 pieces of fruit. Notice, though, that such necessities as this cannot be said in the language of the *Tractatus* but only shown. Arithmetical equations are therefore according to the *Tractatus* pseudo-propositions—failed attempts to say what cannot be said.

Perhaps it was only when he read the *Tractatus* in 1919 that Russell realized Wittgenstein was not the person to revise the early parts of *Principia*. Russell therefore set about doing the work himself, by writing a new Introduction for the second edition. But that Introduction does not take much account of the criticisms that Wittgenstein made in the *Tractatus*. To the extent that Russell addressed Wittgenstein's ideas, it was to a large extent the pre-war Wittgenstein that Russell was responding to.

It was left to Ramsey, therefore, to attempt a revision of the philosophical underpinnings of *Principia* in accordance with Tractarian principles. His paper on 'The foundations of mathematics', published in 1926, aims to show that the theorems of *Principia* are not pseudo-propositions at all but rather complicated tautologies. The central step consists in Ramsey's attempt to manufacture identity as a kind of propositional function, with the consequence that the essential class $\{a,b\}$, for instance, can then be derived in a no-class theory from the function $x=a \vee x=b$ (see Potter 2005).

It is relevant to note Wittgenstein's reaction to Ramsey's proposals. The part that commentators tend to focus on is Ramsey's argument for the adoption of a simple, rather than a ramified, theory of types. The surviving texts do not show any sign that Wittgenstein objected to this, however: instead Wittgenstein reserved his disapproval for the part of Ramsey's article in which he tried to derive an essential notion of class from logic. Wittgenstein objected to this notion not only in a letter he wrote to Ramsey, but he also returned to the point more than once subsequently, attempting various formulations of his objection in the *Philosophical Remarks* and the *Philosophical Grammar*. This lends further weight to the view that Wittgenstein regarded his objection to Russellian logicism as fundamental. What he evidently objected to in Ramsey was not so much the details of his account as the overall ambition of demonstrating that mathematics consists of tautologies.

3 Finitism

When Wittgenstein returned to philosophy and to Cambridge in 1929, it was the problem of the infinite that he started to consider–first of all in concert with Ramsey. In a sense, this problem was common ground between them since even if Ramsey's manufactured propositional functions were not susceptible to Wittgenstein's objections, there would remain the problem that much of mathematics depends on the assumption that there are infinitely many objects—an assumption which, Ramsey had to grant, is not a tautology even if it is true.

As far as one can judge, Wittgenstein and Ramsey seem to have moved together towards finitism, i.e. a rejection of the extensional view of generalization in the case of infinitely many propositions. Braithwaite, in his Introduction to the first posthumous edition of Ramsey's papers, (1931, p. xii) remarks that 'in 1929 [Ramsey] was converted to a finitist view which rejects the existence of any actual infinite aggregate'. This is the context of Ramsey's remark that 'What we can't say we can't say, and we can't whistle it either.' (1990, p. 146) Around this time Ramsey studied and made notes on several papers on intuitionism, while Wittgenstein also considered Skolem's quantifier-free arithmetic. (Wittgenstein 1975, 163)

The work in which Wittgenstein's emerging hostility to the infinite first gained expression was the *Philosophical Remarks* of 1930. What confront us there are various remarks intended to cast doubt on the idea that there are actual infinite sets. For instance:

> The infinite number series is only the infinite possibility of finite series of numbers. It is senseless to speak of the *whole* infinite number series, as if it, too, were an extension. (1975, p. 164)

But what was Wittgenstein's objection to infinite sets? Many of his remarks seem to be intended to clarify the distinction between the actual and the potential infinite. For instance:

> A searchlight sends out light into infinite space and so illuminates everything in its direction, but you can't say it illuminates infinity. (1975, p. 162)

However, this distinction was already available. (It goes back, after all, to Aristotle.) When we look for an *argument* against the existence of infinite sets, the nearest we find is perhaps the following.

> Let's imagine a man whose life goes back for an infinite time and who says to us: 'I'm just writing down the last digit of π, and it's a 2'. Every day of his life he has written down a digit, without ever having begun; he has just finished.
> This seems utter nonsense, and a *reduction ad absurdum* of the concept of an infinite *totality*. (1975, p. 166)

Yet as an argument against the notion of an infinite totality it is hard to know what to make of this. At any rate, Wittgenstein surely mis-stated what he meant. Since π is (as was known in the late 19th century) irrational, its decimal expansion does not terminate and hence has no 'last digit'. Presumably what Wittgenstein means us to imagine (or fail to imagine) is a man writing down the digits of π *backwards*, in which case the last digit would of course be a 3. But even if we agree with Wittgenstein that *this* is utter nonsense, it is far from clear that what is nonsensical about it is the fault of the infinite totality involved, rather than something inherently directional in our conception of a task performed in time. If so, then the right response might be to reject the kind of appeal to the intuition of time in grounding arithmetic that was advocated by Brouwer; it is not so clear why the coherence of the notion of an infinite set should thereby be threatened.

Nonetheless, in the *Philosophical Remarks* Wittgenstein sees his rejection of the actual infinite as causing a problem for quantification in arithmetic. Evidently if we deny the existence of infinite totalities, we no longer have available the Tractarian account of quantification over an infinite domain as an infinite logical product.

> If no finite product makes a proposition true, that means *no* product makes it true. And so it *isn't* a logical product. (1975, p. 149)

The difficulty this leads to, according to Wittgenstein, is that in that case it seems to me that we can't use generality—all, etc.–in mathematics at all. There's no such thing as 'all numbers', simply because there are infinitely many. (1975, p. 148)

In fact, the account in the *Tractatus* is probably in trouble even without the extra constraint that finitism brings (as Ramsey had pointed out in his critical notice on the *Tractatus* even before he had met Wittgenstein). What is important in the development of Wittgenstein's views is not so much the link he saw with finitism as his realization of the need to address the problem. In the *Tractatus* he had given the impression that all applications of mathematics could be funnelled through quantifier-free arithmetic. Only in 1929 does he seem to have begun to recognize how implausible this is. This was, of course, just before Gödel proved his incompleteness theorems, which demonstrated dramatically the gulf in complexity between the arithmetic of simple equations and full-blown quantified arithmetic. But if anyone might have had at least a vague sense of this gulf in advance of Gödel's proof, it was surely Ramsey: after all, his work on the decision problem for first-order logic spawned a whole subject in combinatorics now known as Ramsey theory. Perhaps, therefore, it was Ramsey who persuaded Wittgenstein that an account of the meaning of quantified propositions in arithmetic does not follow trivially from what he had said in the *Tractatus* about equations.

4 Meaning as proof

Wittgenstein's route to an account of quantified propositions seems to have started from the verification principle: the meaning of a proposition consists in its means of verification. Wittgenstein certainly espoused this view for several years. It was also, at around the same time, one of the central tenets of the logical positivists. (Whether it was Wittgenstein who initiated its adoption by the members of the Vienna Circle is less clear.) What is distinctive about mathematics, of course, is that its fundamental method is that of proof: the means by which we verify that a mathematical proposition is true is to prove it. And in the case of an arithmetical generalization, in particular, the proof proceeds by the use of mathematical induction. So if we apply the thesis of verificationism to an arithmetical generalization we obtain the conclusion that the meaning of the proposition consists in its inductive proof.

We can trace out this view in the *Philosophical Remarks* of 1930. Wittgenstein first observes that

> *generality* in arithmetic is indicated by an induction.
> An induction is the expression for arithmetical generality. (1975, p. 150)

He then makes the general remark that

> how a proposition is verified is what it says. The verification is not *one* token of the truth, it is *the* sense of the proposition. (1975, p. 200)

The moral he draws from this is that

> if we want to see what has been proved, we ought to look at nothing but the proof. (1975, p. 193)

He then deduces that the sense of an arithmetical generalization is its inductive proof.

> We are not saying that when $f(1)$ holds and when $f(c+1)$ follows from $f(c)$, the proposition $f(x)$ is *therefore* true of all cardinal numbers; but: "the proposition $f(x)$ holds for all cardinal numbers" *means* "it holds for x=1, and $f(c+1)$ follows from $f(c)$ ". (1974, II, VI, 32)

Although, as I have suggested, this account flows quite naturally from the verification principle, there is a series of very obvious difficulties with it. One of these is that the account does not explain why we feel entitled to infer from $(x)\phi(x)$ to $\phi(n)$ for any number *n*: if we try to give an argument for this, we simply find ourselves using induction again, but in a slightly more complicated case.

A related problem is that if we wish to prove $(x)\phi(x)$ by mathematical induction, we must prove two things: first we check that $\phi(0)$ holds; then we prove $(x)(\phi(x)\rightarrow \phi(x+1))$. If the meaning of $(x)\phi(x)$ is an induction, then in the same way we would expect the meaning of $(x)(\phi(x)\rightarrow \phi(x+1))$ to be an induction too. Moreover, the second of these expressions is logically *more* complex than the first. We are therefore in an infinite regress of more and more complex expressions, the meaning of each of which is explained by appeal to the sense of the next in the sequence. We might try to relieve this difficulty by making a distinction between generalizations which have inductive proofs and ones (such as $(x+y)^2=x^2+2xy+y^2$) which have free variable proofs; but the relief is only temporary. The problem is that in most systems these proofs depend in their turn on other inductive proofs.

As Wittgenstein himself noted, another difficulty for his doctrine arises with unsolved problems.

> My explanation mustn't wipe out the existence of mathematical problems. That is to say, it isn't as if it were only certain that a mathematical proposition made sense when it (or its opposite) had been proved. (1975, p. 170)

The trouble is that Wittgenstein's explanation *does* wipe out mathematical problems. If the meaning of an arithmetical generalization is given by its proof, then none of us understands Goldbach's conjecture, since no proof or refutation of it is currently known. And in that case how could anyone try to find one?

The key distinction for Wittgenstein at this point seems to be whether or not there is a decision procedure for the problem (cf. Saatela's paper). If not, then according to him we do not really understand the problem (despite appearances to the contrary).

> This boils down to saying: If I hear a proposition of, say, number theory, but don't know how to prove it, then I don't understand the proposition either. This sounds extremely paradoxical. It means, that is to say, that I don't understand the proposition that there are infinitely many primes, unless I know its so-called proof: when I learn the proof, I learn something *completely new*, and not just the way leading to a goal with which I'm already familiar. But in that case it's unintelligible that I should admit, when I've got the proof, that it's a proof of precisely *this* proposition, or of the induction meant by this proposition. (1975, p. 183)

> Only where there's a method of solution is there a problem (of course that doesn't mean 'Only when the solution has been found is there a problem').
> That is, where we can only expect the solution from some sort of revelation, there isn't even a problem. A revelation doesn't correspond to any question.
> It would be like wanting to ask about experiences belonging to a sense organ we don't yet possess. Our being given a new sense, I would call revelation. (1975, p. 172)

> Every legitimate mathematical proposition must put a ladder up against the problem it poses, in the way that 12×13=137 does—which I can then climb if I choose. This holds for propositions of any degree of generality. (N.B. there is no ladder with 'infinitely many' rungs.) (1975, p. 179)

Wittgenstein's view leads, then, to the conclusion that we do not really understand Goldbach's conjecture. He has nothing very convincing to say about what mathematicians are doing when they try to prove Goldbach's conjecture.

Moreover, if propositions for which no proof is known constitute a difficulty for Wittgenstein's account, a further difficulty is presented by the opposite case of those for which there is more than one proof. His account renders it unintelligible, that is to say, how two different proofs could be proofs of the same proposition. And yet this is a perfectly common situation in mathematics.

5 Contradiction

Wittgenstein's view concerning arithmetical generalizations applies, via the arithmetization of syntax, to consistency statements. The statement that a formal system is consistent will, when arithmetized, have the form of an arithmetical generalization. Moreover, for any reasonably elaborate formal system there is no decision procedure for finding inconsistencies. Hence on Wittgenstein's view there is no problem of inconsistency until actually we find an inconsistency.

Wittgenstein makes a distinction here between hidden and obvious inconsistencies. He does grant that we should check for obvious inconsistencies. But if there are none, we cannot legitimately worry about hidden inconsistencies because the question whether there are any is devoid of meaning.

> Something tells me that a contradiction in the axioms of a system can't really do any harm until it is revealed. We think of a hidden contradiction as like a hidden illness which does harm even though (and perhaps precisely because) it doesn't show itself in an obvious way. But two rules in a game which in a particular instance contradict each other are perfectly in order until the case turns up, and it's only then that it becomes necessary to make a decision between them by a further rule. (1974, p. 303)

Wittgenstein attributes the existence of hidden contradictions to ambiguity in the rules.

> If a contradiction is found later on, that means that hitherto the rules have not been clear and unambiguous. So the contradiction doesn't matter, because we can now get rid of it by enunciating a rule.
>
> In a system with a clearly set out grammar there are no hidden contradictions, because such a system must include the rule which makes the contradiction discernible. A contradiction can only be hidden in the sense that it is in the higgledy-piggledy one of the rules, in the unorganized part of the grammar; and there it doesn't matter since it can be removed by organizing the grammar. (1974, p. 305)

This last remark is very strange indeed. As a matter of fact, there is no decision procedure for settling whether a formal system for arithmetic is consistent. Wittgenstein seems to have imagined that if it is uncertain whether a system is consistent, that can only be because the system has not been set out with sufficient clarity. That is simply false.

Moreover, the first part of the last quotation, in which Wittgenstein ascribes contradictions to lack of clarity in the rules, invites a further question. Why should we be worried by an inconsistency even if we do find it? There is a brief mention of this concern in the *Philosophical Remarks*.

> It seems to me that the idea of the consistency of the axioms of mathematics, by which mathematicians are so haunted these days, rests on a misunderstanding. This is tied up with the fact that the axioms of mathematics are not seen for what they are, namely, propositions of syntax. (1975, p. 189)

This point is taken up again in *Philosophical Grammar*.

> Mathematicians nowadays make so much fuss about proof of the consistency of axioms. I have the feeling that if there were a contradiction in the axioms of a system it wouldn't be such a great misfortune. Nothing easier than to remove it. (1974, p. 302)

Lying behind these remarks is a distinction, which Wittgenstein drew in conversation with Schlick and Waismann, between a contradictory sentence and a contradictory rule:

> Axioms have two meanings, as Frege saw:
>
> 1. The rules *according to* which you play.
> 2. The opening positions of a game.
>
> If you take the axioms in the second meaning, I can attach no sense to the claim that they contradict each other. It would be very odd to say, This configuration of the pieces ('0≠0', for example, in Hilbert's game with formulas) is a contradiction. Two rules can contradict one another. What do we do in such a case? Very simple—we introduce a new rule and the conflict is resolved. (Waismann 1979, p. 119)

Two rules contradict each other if one says you are allowed to do something and the other says you aren't. This induces puzzlement when you notice the problem, but you can then sort it out with a stipulation, and the stipulation needn't invalidate anything you have done so far. For this sort of contradiction Wittgenstein is quite right that in most precisely formulated formal systems they could not be hidden, i.e. they could have come about only through carelessness. Notice, though, that this is merely a feature of formal systems as we usually formulate them. We could, for example, have a formal system with two rules:

1. You may write down any formula which it would be legitimate to write down in Peano Arithmetic.
2. You may not write down any formula which contradicts what you have already written down.

Does this set of rules lead to contradiction, i.e. to the sort of puzzlement Wittgenstein is referring to, where we simply don't know what to do? That depends entirely on whether Peano Arithmetic is consistent, and we do not have a mechanical means of settling *that* question.

The idea that a contradiction is not harmful *per se* is one Wittgenstein made repeatedly. In his discussion of Gödel's incompleteness theorems, for instance, he says:

> Is there harm in the contradiction that arises when someone says: "I am lying.—So I am not lying.—So I am lying.—etc."? I mean: does it make our language less usable if in this case, according to the ordinary rules, a proposition yields its contradictory, and vice versa? —the proposition itself is unusable, and these inferences equally. … Such a contradiction is of interest only because it has tormented people. (1978, p. 120)

Let us compare now what Wittgenstein said in one of his 1939 lectures in Cambridge.

> Is it hidden as long as it hasn't been *noticed*? Then as long as it's hidden, I say that it's as good as gold. And when it comes out in the open it can do no harm. (1976, p. 219)

But can't a hidden contradiction do damage without our realizing it? This is a worry that Turing put to Wittgenstein explicitly:

> *Wittgenstein*: You might get *p.~p* by means of Frege's system. If you can draw any conclusion you like from it, then that, as far as I can see, is all the trouble you can get into. And I would say, "Well then, just don't draw any conclusions from a contradiction."
> *Turing*: But that would not be enough. For if one made that rule, one could get round it and get any conclusion which one liked without actually going through the contradiction. (1976, p. 220)

What, if anything, Wittgenstein had in mind here is quite unclear. Turing's remark is surely correct. Wittgenstein seems to have been labouring under the misconception that we can repair a contradictory system simply by refusing to draw any conclusions from a contradiction. Of course, much work has been done subsequently on formal systems of logic which *can* tolerate contradictions in something like the way Wittgenstein envisaged, but it is hard to see that work as bearing out the insouciance towards contradictions which Wittgenstein recommended. Perhaps, then, one can sympathize with the response Wittgenstein is recorded as having made to Turing's objection: 'Well, we must continue this discussion next time.' (At the next lecture Wittgenstein continued to discuss contradictions, but it is hard to see any of what he said as really answering Turing's objection.)

Perhaps it is significant, given how famous his views on consistency proofs in mathematics have become, that relatively little space in Wittgenstein's published typescripts (which one might regard as his main works, as opposed to lectures and manuscripts) is devoted to the subject. There are a couple of pages on the subject in *Philosophical Remarks*, a couple more in *Philosophical Grammar*, and part of an appendix to the pre-war *Investigations*. Moreover, the remarks Wittgenstein makes in these places are, even by his standards, gnomic in the extreme. Much lengthier remarks on the subject are contained in Wittgenstein's conversations with Schlick and Waismann, in the 1939 lectures, and in the manuscript books for 1939–40 (1978, part III). But he never really developed this material into a stable account.

6 Rule-following

I have suggested that some of what Wittgenstein thought about contradictions could be seen merely as the application to the 'problem' of consistency of his general view about arithmetical generalizations, that their meaning is given by their proof and they are therefore literally meaningless if we do not yet have proofs of them. I want now to indicate briefly how this general view also leads to Wittgenstein's famous 'rule-following argument', i.e. to the idea that each time we apply a rule there is in some sense—in *some* sense—an indeterminacy to be taken as to what 'applying the rule' amounts to on this occasion.

When I talked about Wittgenstein's views on generalizations, I mentioned the difficulty that he could not account for the validity of the process by which we instantiate a universally quantified proposition. Of course, we want to say that the inductive proof entitles us to deduce any individual instance. The difficulty, though, is that only the complete proof does that. How can we elide the middle steps?

> There's no substitute for stepping on every rung, and whatever is equivalent to doing so must in its turn possess the same multiplicity as doing so. (1975, p. 171)

If we give a general proof that we can miss the steps out, then this general proof will be another induction, and its application will involve the same problem at one stage removed (at the meta-level, as we—but not Wittgenstein—might naturally be inclined to put it). We seem dangerously close to the conclusion that we must appeal to a form of intuition to license each instantiation. This is what Wittgenstein is gesturing towards when he says:

> Supposing there to be a certain general rule (therefore one containing a variable), I must recognize each time afresh that this rule may be applied *here*. No act of foresight can absolve me from this act of *insight*. (1975, p. 171)

And elsewhere in the *Philosophical Remarks* Wittgenstein observes:

> Neither can I prove that $a+(b+1)=(a+b)+1$ is a special case of $a+(b+c)=(a+b)+c$; I must see it. (No rule can help me here either, since I would still have to know what would be a special case of this general rule.)
>
> This is the unbridgeable gulf between rule and application, or law and special case. (1975, p. 164)

As early as 1930, therefore, Wittgenstein was explicit about the 'unbridgeable gulf between rule and application', which is the essence of the rule-following argument. However, this is at best a particular case of the general argument. In the case of the rule for instantiating a universal generalization, nothing in the rule forces me to apply it in the 'right' way. Moreover, we have the observation that 'no rule can help me here either, since I would still have to know what would be a special case of this general rule'. What we need now is to generalize that conclusion to any rule whatsoever.

I mentioned earlier that on Wittgenstein's account the sense of a generalization is to be explained by reference to another generalization, namely the one embedded in the inductive proof. For the appeal to the inductive proof as the sense of the generalization not to involve an infinite regress there must be *some* arithmetical statements which have a different justification. These are the recursive definitions of the primitive recursive functions. But calling something a definition does not really solve the problem. Recall what Frege said about definitions:

> The definition of an object does not, as such, really assert anything about the object, but only lays down the meaning of a symbol. After this has been done, the definition transforms itself into a judgement, which does assert about the object; but now it no longer introduces the object, it is exactly on a level with other assertions made about it. (1953, p. 78)

If we are to apply this in Wittgenstein's case, we must ignore the talk of judgements as being 'about' objects. For part of what he wanted to deny was that it is helpful to think of arithmetical sentences as being about numbers in the way that 'Blackburn Rovers won the Premiership' is about Blackburn Rovers. But Frege's main point surely stands: once we have adopted a recursive definition it has to be regarded as a generalization like any other. Yet it cannot, since other generalizations have as their sense their inductive proofs, and a definition (being a mere stipulation) is not the kind of thing that can be proved.

The temptation, of course, is to look for another account of the sense of a recursive definition. But if the account of generalizations as logical products is unavailable, and if we retain Wittgenstein's verificationism, it is hard to see what other account there could be. If that is right, then we are driven to the conclusion that there just is no sense to a recursive definition: there is nothing in the rule which compels us to apply it in a particular way. We have thus arrived again at Wittgenstein's rule-following argument, but now in a form that applies to any recursive definition whatever.

7 Rejection?

Wittgenstein incorporated the rule-following argument into the pre-war version of the *Philosophical Investigations* which he compiled in 1937. He revised and re-arranged this material later, and it is the later re-arrangement that is published as part I of the *Remarks on the Foundations of Mathematics*. The most striking presentational change is that Wittgenstein now goes directly to the rule-following argument, instead of presenting it via the idea that the meaning of an arithmetical generalization is its proof. The case from which the rule-following argument was originally derived is now mentioned only as a special case.

> "But doesn't e.g. '*fa*' have to follow from '(*x*)*fx*' if '(*x*)*fx*' is meant in the way we mean it? "—And how does *the way* we mean it come out? Doesn't it come out in the constant practice of its use? and perhaps further in certain *gestures*—and similar things.—But it is as if there were also something attached to the word "all", when *we* say it; something with which a different use could not be combined; namely, the *meaning*. " 'All' surely means: *all*! " we should like to say, when we have to explain this meaning; and we make a particular gesture and face. (1978, I, §10)

Moreover, what I have presented as the motivating idea of Wittgenstein's middle period philosophy of mathematics, that the meaning of an arithmetical generalization is its proof, is not explicitly mentioned in the pre-war *Philosophical Investigations* at all. This raises the question whether he eventually gave up the doctrine. Perhaps he did. At any rate, there are various passages in his later writings where we can see him struggling with the doctrine and with its puzzling consequences (cf. again Saatela's paper). For instance, he worries about how there can be several different proofs of the same proposition.

> This proof is a mathematical entity that cannot be replaced by any other; one can say that it can convince us of something that nothing else can, and this can be given expression by our assigning to it a proposition that we do not assign to any other proof.
>
> But am I not making a crude mistake? For just this is essential to the propositions of arithmetic and to the propositions of the Russellian logic: various proofs lead to them. Even: infinitely many proofs lead to any one of them.
>
> Is it correct to say that every proof demonstrates something to us which it alone can demonstrate? Would not—so to speak—the proved proposition then be superfluous, and the proof itself also be the thing proved?
>
> Is it only the proved proposition that the proof convinces me of? (1978, III, §§59–60)

Note, though, that this quotation (from late 1939 or early 1940) does not seem to come from someone who has already clearly rejected the doctrine that the meaning of a mathematical proposition should be identified with its proof; instead, it reads as though Wittgenstein was only just beginning to be troubled by the view. He repeats a similar concern a little later (June 1941).

> Now how about this—ought I to say that the same sense can only have *one* proof? Or that when a proof is found the sense alters?
>
> Of course some people would oppose this and say: ‘Then the proof of a proposition cannot ever be found, for, if it has been found, it is no longer the proof of *this* proposition’. (1978, VII, §10)

Wittgenstein’s enigmatic response is that ‘to say this is so far to say nothing at all’. He then recalls his earlier view that ‘if you want to know what a mathematical proposition says, look at what its proof proves’, before going as far as to grant that there might be ‘both truth and falsehood’ in this view.

If Wittgenstein began around 1939 to doubt the doctrine that the meaning of an arithmetical generalization is its proof, I think we can come, albeit tentatively, to a diagnosis of the reason why the final version of the *Philosophical Investigations* did not include, as the pre-war version had done, a lengthy section on the philosophy of mathematics. The reason, I believe, is that Wittgenstein’s thinking about mathematics had been dominated by this doctrine of meaning as proof. When he came to realize that it is untenable (or at least suspect), the damage done to his account was too great to be easily repaired.

8 A late philosophy?

Nonetheless, Wittgenstein continued to work on the philosophy of mathematics intermittently until at least 1944. Is there perhaps room, therefore, for the possibility that there might be a distinct view, or cluster of views, that could be described as Wittgenstein’s late philosophy of mathematics—a philosophy which he developed after he had rejected the erroneous central doctrine of his middle period?

Since Wittgenstein published nothing after 1929 (except for a letter to the editor of *Mind* complaining that Braithwaite had misrepresented his views) it is a matter for debate which parts of his *Nachlass* we ought to take seriously. It is clear at one extreme that he intended the *Philosophical Investigations* (in their post-war version) for publication. And it is equally clear at the other that some of the material in his pocket notebooks is what one would expect to find in notebooks—ideas that obviously do not work and which he would never have had any temptation to publish. The point to remember here is that Wittgenstein’s working method remained broadly constant throughout his life. He wrote his ideas in journals from day to day, then extracted the best of them to put into further notebooks or typescripts, then cut up the typescripts and re-arranged them (sometimes almost endlessly). We should not regard what we find in the notebooks as having the same status as the worked-over typescripts.

It is therefore worth drawing attention to the status of the *Remarks on the Foundations of Mathematics*, written between 1937 and 1944, on which so much of the secondary literature on Wittgenstein’s late philosophy of mathematics relies. As the editors’ preface explains, only part I of this books is based on a complete typescript. Of the others, only part VI is a reasonably complete manuscript. The rest are selections (and in some cases arrangements) by the editors from much more extensive notebooks. What this book is not is a complete work by Wittgenstein on the philosophy of mathematics. It must surely be treated with care if it is not to do considerable harm to our understanding of Wittgenstein’s later philosophy of mathematics.

Although we find in the *Remarks on the Foundations of Mathematics* explorations (dating from the period up to 1944) of various themes in the philosophy of mathematics that are not to be found in what I have been calling Wittgenstein's middle period writings, we do not have typescripts from this period that he had worked over for publication (even one's with which he later became dissatisfied), as we do in the case of his middle period. This is part of the reason why it is so hard to state anything that deserves the title of a late-period philosophy of mathematics. The *Remarks on the Foundations of Mathematics* contain many thought-provoking observations and ideas, and anyone interested in the philosophy of mathematics ought to read them, but they are of variable quality and can hardly be said to present something that amounts to a coherent view.

It is often noted that one of the characteristic features of Wittgenstein's later philosophy generally is its lack of *theses*. In his later writings Wittgenstein's technique was neither to make claims nor to present tightly constructed arguments against the view of his opponents. So it is no surprise to find that the *Remarks on the Foundations of Mathematics* conform to this pattern by refraining from making claims. But the point I am making here goes beyond that general observation. Wittgenstein's late writings on mathematics are fragmentary in a way that his writings on philosophy of mind, for instance, are not. His later writings are no doubt always in a certain sense inconclusive and open-ended—allusive rather than straightforwardly persuasive. But if we put this open-endedness to one side, we usually find that there is a stance we can recognize Wittgenstein as adopting. There are views which we can ascribe to him, even while accepting, perhaps, that by presenting them as views which we can summarize, we inevitably misrepresent them somewhat. In the case of mathematics, though, not even this seems to be true. We know what he was against: platonism, logicism, intuitionism and Hilbertian formalism all at various times come in for his criticism. But what, by contrast, was he for? We can be fairly sure, I think, that he never gave up the two views that were central to his early philosophy of mathematics, that numbers are not objects, and that arithmetical equations are not tautologies. But if it is the first task of any philosopher of mathematics who holds these two views to give an account of arithmetical generalizations, and if Wittgenstein eventually gave up his account according to which their meanings are given by their proofs, then it is hard to see what he had to offer instead. Indeed, one struggles to present, even in outline, a positive account of mathematics that can reasonably be called Wittgensteinian. Too often Wittgenstein seems more concerned with offering meta-level advice about how to go about finding a correct account, rather than with developing the account itself.

Perhaps, though, Wittgenstein's failure to make significant progress was inevitable. He was trying to reconcile his radically anti-realist conception of the subject matter of mathematics with an anti-revisionary conception of mathematical practice without collapsing into formalism; and that is surely a very tall order.

Wittgenstein's remarks on Gödel's incompleteness theorems are notoriously controversial: it is a matter of debate whether Wittgenstein succeeds in enunciating a position from which the validity, or the standard interpretation, of Gödel's theorem can coherently be questioned. But he was right to try. For what Gödel's theorem demonstrates, on its standard, interpretation, is that conventional mathematics has a richness which the radical anti-realist cannot explain.

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