Wittgenstein on Mathematical Proof

CRISPIN WRIGHT

To be asked to provide a short paper on Wittgenstein's views on mathematical proof is to be given a tall order (especially if little or no familiarity either with mathematics or with Wittgenstein's philosophy is to be presupposed!). Close to one half of Wittgenstein's writings after 1929 concerned mathematics, and the roots of his discussions, which contain a bewildering variety of underdeveloped and sometimes conflicting suggestions, go deep to some of the most basic and difficult ideas in his later philosophy. So my aims in what follows are forced to be modest. I shall sketch an intuitively attractive philosophy of mathematics and illustrate Wittgenstein's opposition to it. I shall explain why, contrary to what is often supposed, that opposition cannot be fully satisfactorily explained by tracing it back to the discussions of following a rule in the *Philosophical Investigations* and *Remarks on the Foundations of Mathematics*. Finally, I shall try to indicate very briefly something of the real motivation for Wittgenstein's more strikingly deflationary suggestions about mathematical proof, and canvass a reason why it may not in the end be possible to uphold them.

I

Euclid is credited with the first proof that, among the series of positive whole numbers, the occurrence of prime numbers is endless. His reasoning, as many readers doubtless recall, was based on what is often called the Fundamental Theorem of arithmetic—the lemma that every number has a unique prime factorization: i.e., can be represented as the product of a multiplication sum in which only prime numbers occur as factors. [Thus 28 is 7×2×2; 273 is 3×7×13; and so on.] The proof then proceeds by *reductio ad absurdum*. Suppose there were a last prime—call it N. And consider the corresponding N!+1—the number we get by cumulatively multiplying N by each of its predecessors in turn and adding 1 to the total. If N is the greatest prime, as we have assumed, then this number is not prime but composite. Hence, by the Fundamental Theorem, it is the product of a unique set of prime factors. But what are they? They cannot comprise any number *smaller than or equal to* N, for, given the way that N!+1 is constructed, none of those numbers divides into it without a remainder—they all leave remainder 1. So the prime factors of N!+1 must be *greater* than N—but then that contradicts our hypothesis, that N is the greatest prime, which is therefore refuted. If there were a greatest prime, then, by the Fundamental Theorem of arithmetic, there would have to be prime numbers greater than it; so there is no greatest prime number—the primes run on without end.

It is natural and attractive to view this pleasantly economical reasoning as constituting a *discovery*. The basic concepts of number theory—the branch of pure mathematics that deals with zero and the positive whole numbers that succeed it—are very accessible: they include the concept of zero itself, the idea of one number succeeding another, the idea of an endless array of such numbers getting larger and larger *ad infinitum*, and elementary operations upon them such as addition, multiplication, exponentiation and so on. In terms of this basic and easily intelligible set of notions, all the concepts and operations of pure number theory can be defined and all the statements which exercise the interest of the number theoretician can be formulated. And because of the accessibility of the basic concepts, and the straightforward character of many of the consequential definitions, some of these statements are extremely easy to understand. That the primes are infinite is one such; Goldbach's Conjecture, that every even number is the sum of two primes, and the alleged 'last theorem' of Fermat, that the equation $X^n+Y^n=Z^n$ has no solution among the natural numbers for values of n greater than or equal to 3, are two famous examples of readily intelligible statements which remain unresolved to this day.[1]

Confronted with such examples, our inclination is to think that they raise interesting questions which must have answers but to which we do not at present know the answers. It would be quite possible, before I knew anything of Euclid's proof, to wonder about Goldbach's Conjecture and then realize that, if true, it would require the infinity of the primes, and to proceed to wonder about that. Euclid's proof would then naturally present itself as a discovery that at least one necessary condition for the truth of the Goldbach Conjecture was met. And it would remain to wonder whether a similar but, no doubt, more complex feat of human ingenuity will some day disclose that the Conjecture itself is indeed true, or whether, rather, far out into the series of natural numbers, occurs an even number which is the sum of no two of its prime predecessors.

A striking aspect of this intuitively natural way of thinking is the separation it effects between the concepts of truth and proof in mathe-

[1] For n=2, of course, there are "Pythagorean" solutions—for instance, 3, 4 and 5; 5, 12 and 13; and so on.

matics. We wonder whether the Goldbach Conjecture will ever be proved or refuted. But the statement of the Conjecture is so easily grasped, and its meaning so apparently sharp, that we are not at all inclined to doubt that it must be, determinately, either true or false. For the series of natural numbers itself, we conceive, is a perfectly definite structure, in which the even numbers are a sharply defined sub-series. And of each particular even number it is, surely, a definite question with a definite and (in principle) ascertainable answer, whether it is the sum of two primes or not. And now, how can the question whether *all* items of a certain kind have a certain characteristic fail to have a determinate answer—even if we cannot know what the answer is—if the items in question are a sharply defined class and the characteristic in question is something which *each* of them determinately either possesses or not?

We are thus instinctively drawn, at least in the case of number theory, to *mathematical realism*. According to mathematical realism, the number-theoretician is a kind of explorer. His project arises because, whereas the natural numbers are infinite, the capacities and opportunities possessed by the human mind are finite. Perhaps a deity could somehow mechanically check each even number and determine whether it was the sum of two primes or not—and then remember whether, in the course of this infinite labour, any counter-examples to the Goldbach Conjecture had been turned up. But *we* can do no such thing. For us, the only way of determining the truth or falsity of such a statement is, as it were, indirectly, by cunning. So proof comes to be seen as merely a kind of cognitive auxiliary, a method of investigation which we are forced to use because, in dealing with infinite totalities, our finiteness leaves us with no other recourse.

To conceive of the truth of number-theoretic statements in this way invites, of course, the question: what, when such a statement is true, *makes* it true? And it is no answer to say: the way things are with the natural numbers. What the questioner is requesting is advice about how, in general terms, the states of affairs which—perhaps quite independently of any possibility of human knowledge—confer truth on number-theoretic statements, should be conceived as constituted. A very ancient answer is that the world contains numbers and other kinds of mathematical objects much as it contains mountains and seas; that there is an abstract substance to the world as much as a physical one. Such a view is, remarkably, still a topic of ongoing professional debate.[2] But the view of most contemporary philosophers of mathematics would

[2] Kurt Gödel is widely regarded as endorsing the ancient answer in his (1947), pp. 483–4. Penelope Maddy is also a staunch champion of it; see her (1980).

be that it is no more than a metaphor for the kind of objectivity which, driven by the sort of intuitive realist thinking which I briefly sketched, we would like pure mathematics to have. What, I suspect, with our realist hats on, we really think about the constitutive question is something more anthropocentric. Kronecker said, famously, that whereas all the rest of pure mathematics was the work of man, the natural numbers were created by God. But no realist need think anything of that sort. It is enough if we are capable of creating, in thought, a sufficiently definite concept of the series of natural numbers to give substance to questions about its characteristics which we may not know how to answer. And is that so puzzling an idea? The rules of Noughts and Crosses, for instance, are perfectly definite, yet it is not totally trivial to show that the second player can always force a draw; and it is possible to understand the rules perfectly yet be unaware of the point. Is it not, nevertheless, a perfectly objective feature of the game which, when it finally dawns on one as a child, it is proper to think of oneself as *finding out*? And is not Noughts and Crosses a human invention for all that? As the small child with Noughts and Crosses, so the adult mathematician with number theory; the difference is only that there is, in the case of Noughts and Crosses, no analogue of the infinity of the number series to set up the possibility that truth and verifiability, even verifiability 'in principle', might come apart.

We now have on display almost all the ingredients in our intuitive thinking about pure mathematics against which Wittgenstein's later philosophy reacts. The conception of mathematics as a kind of investigative science; the notion that it explores a special domain of states of affairs, which are constituted by acts of human concept formation yet somehow acquire the autonomy to outstrip what is transparent, or even in principle accessible to the human subject; the view of proof as an exploratory tool, albeit a kind of cognitive prosthetic on which we are forced to rely because of finitude—each of these ideas is roundly criticized throughout Wittgenstein's later writings on mathematics. It remains only to include our sense of proof as somehow excluding all rational options but assent to its status as proof—what Wittgenstein famously characterized as the 'hardness' of the logical 'must'—and the associated idea, which even Descartes' scepticism did not prompt him seriously to call into question, that proof in mathematics is a source of an especially sure and certain genre of knowledge—add these and we have both a thumbnail sketch of the lay-philosophy of mathematics which we find most attractive and an inventory of the principal confusions to which Wittgenstein regarded our thought about these matters as prone.

For Wittgenstein, pure mathematics is not a project of exploration and discovery; mathematical proof is not an instrument whereby we find out things; conceptual structures cannot have the kind of autonomy to allow their characteristics to outstrip what can be ratified by human thought; there is no external compulsion upon us when we ratify proofs—we are driven, but not by cognition of an external, normative constraint; and in so far as there is a special sureness about at least some mathematical propositions, it does not amount to a superlative genre of knowledge—such propositions do not enjoy a *cognitive* certainty at all.

Here are some passages illustrative of each of these deflationary lines of thought. On mathematics as an investigative science in which we explore the characteristics of our own conceptual constructions and rules, Wittgenstein writes:

> What, then—does [mathematics] just twist and turn about within these rules?—It forms ever new rules: is always building new roads for traffic; by extending the network of the old ones.
>
> But then doesn't it need a sanction for this? Can it extend the network *arbitrarily*? Well, I could say: a mathematician is always inventing new forms of description. Some, stimulated by practical needs, others from aesthetic needs,—and yet others in a variety of ways. And here imagine a landscape gardener designing paths for the layout of a garden; it may well be that he draws them on a drawing board merely as ornamental strips without the slightest thought of someone sometime walking on them.
>
> The mathematician is an inventor, not a discoverer. (RFM, I, 165–7)

He speaks with suspicion of the idea of

> Arithmetic as the natural history [mineralogy] of numbers. But *who* talks like this about it? Our whole thinking is penetrated with this idea. (RFM, III, 11)

And in the Appendix on Cantor's Diagonal Argument he remarks:

> 'Fractions cannot be arranged in an order of magnitude'.—First and foremost, this sounds extremely interesting and remarkable.
>
> It sounds interesting in a quite different way from, say, a proposition of the differential calculus. The difference, I think, resides in the fact that *such* a proposition is easily associated with an application to physics, whereas *this* proposition belongs simply and solely to mathematics, seems to concern as it were the natural history of mathematical objects themselves.
>
> One would like to say of it e.g.: it introduces us to the mysteries of the mathematical world. *This* is the aspect against which I want to give a warning.
>
> When it looks as if . . ., we should look out. (RFM, II, 10)

Against the conception of proof as an instrument of conceptual discovery, Wittgenstein urges a quite different picture:

> I am trying to say something like this: Even if the mathematical proposition seems to point to a reality outside itself, still it only expresses acceptance of a new measure (of reality). . . . we have won through to a piece of knowledge in the proof? And the final proposition expresses this knowledge? And is this knowledge now independent of the proof (is the navel string cut)?—well, the proposition is now used by itself and without having the proof attached to it.
>
> Why should I not say: in the proof I have won through to a *decision*? . . .
>
> The proposition proved by means of the proof serves as a rule . . . (RFM, II, 27, 28)
>
> I go through the proof and say: 'yes, this is how it *has* to be; I must fix the use of my language in *this* way'.
>
> I want to say that the *must* corresponds to a track which I *lay down* [my emphasis, C.W.] in language.
>
> When I said that a proof introduces a new concept, I meant something like: the proof puts a new paradigm among the paradigms of the language . . . the proof changes the grammar of our language, changes our concepts. It makes new connections, and it creates the concept of those connections. (It does not establish that they are there; they do not exist until it makes them.) (RFM, II, 30, 31)

Proofs do not draw to our attention what must be the case, nor is it right to think of them as commanding our assent:

> What is the transition that I make from 'it will be like this' to 'it *must* be like this'? I form a different concept. One involving something that was not there before. When I say: 'if these derivations are the same, then it *must* be that . . .', I am making something into a criterion of identity. So I am recasting my concept of identity. . . .
>
> Can I say: the proof induces us to make a certain decision, namely that of accepting a particular concept formation?
>
> Do not look on a proof as a procedure which *compels* you, but as one which *guides* you.—And what it guides is your *conception* of a (particular) situation. (RFM, III, 29, 30)
>
> *What* is unshakably certain about what is proved?
>
> To accept a proposition as unshakably certain—I want to say—means to use it as a grammatical rule: this removes uncertainty from it. (RFM, II, 39)

And again, on mathematical (logical) compulsion:

> We say: 'If you really follow the rule in multiplying, you *must* all get the same results'. Now if this is only the somewhat hysterical way

> of putting things that you get in university talk, it need not interest us overmuch.
>
> It is however the expression of an attitude towards the technique of calculation, which comes out everywhere in our life. The emphasis of the *must* corresponds only to the inexorableness of this attitude both to the technique of calculating and to a host of related techniques.
>
> The mathematical Must is only another expression of the fact that mathematics forms concepts. (RFM, V, 46)

Finally, on a mathematical problem structurally similar to that posed by the Goldbach Conjecture—the question whether seven consecutive '7's occur in the decimal expansion of Π—Wittgenstein writes

> The question—I want to say—changes its status when it becomes decidable. For a connection is then made which formerly *was not there* . . .
>
> However queer it sounds, the further expansion of an irrational number is a further expansion of mathematics . . .
>
> I want to say: it looks as though the ground for the decision were already there; and it has yet to be invented. (RFM, IV, 9)

It does indeed look as though the 'ground for the decision were already there'. How could it not be? For is not the decimal expansion of Π determined by rule at every step? So how can there be any indeterminacy, before we get any mathematical result on the matter, about what is the correct answer to the question? Either, we want to say, seven consecutive '7's do occur—and necessarily so, since their occurrence, where they occur, is built into the identity of Π—or, again necessarily, they do not. The question could be indeterminate—'the ground for the decision' not yet exist—only if there were some indeterminacy in the proper expansion of Π. But there is, surely, none. Yet Wittgenstein challenges these intuitive and seemingly unassailable thoughts head-on.

> Might I not say: if you do a multiplication, in any case you do not find the mathematical fact, but you do find the mathematical proposition? For what you *find* is the non-mathematical fact, and in this way the mathematical proposition. . . . a mathematical proposition is the determination of a concept, following upon a discovery . . .
>
> The concept is altered so that this *had* to be the result. I find, not the result, but that I reach it. And it is not this route's beginning here and ending here that is an empirical fact, but my having gone this road, or some road to this end.
>
> But might it not be said that the *rules* lead this way, even if no one went it? For that is what one would like to say—and here we see the mathematical machine which, driven by the rules themselves, obeys

> only mathematical laws and not physical ones.
>
> I want to say: the working of the mathematical machine is only the *picture* of the working of a machine. The rule does not do work, for whatever happens according to the rule is an interpretation of the rule. (RFM, III, 47, 48)

What drives Wittgenstein to these implausible-seeming and unattractive views?

II

Ten years ago, in a systematic study of Wittgenstein's later philosophy of mathematics (Wright, 1980), I was, I think, the first to offer and develop in print the suggestion that the critical examination of the concept of following a rule, pursued both in the *Remarks on the Foundation of Mathematics* and the *Philosophical Investigations*, is central to the interpretation not just of Wittgenstein's later thought about mathematics but of his later philosophy as a whole. This perspective seems to have been an idea whose time had come. Subsequently Saul Kripke published his highly influential *Wittgenstein on Rules and Private Language* (Kripke, 1982) outlining a vivid 'Sceptical Paradox' which it presents as the heart of Wittgenstein's thought about rule-following, and whose resolution purportedly generates the argument against private language. Kripke's book concentrates almost exclusively on the Sceptical Paradox and the accommodation with it—the Sceptical Solution—which, in Kripke's view, underlies Wittgenstein's ideas about privacy and the self-ascription of sensation and other psychological states. But he anticipates (Kripke, 1982, 3–5) a perfectly direct application of his interpretation of the ideas about rules to the philosophy of mathematics, and does not hesitate to suggest that both the philosophy of mind of the *Investigations* and the philosophy of logic and mathematics expounded in the *Remarks on the Foundations of Mathematics* should be seen as driven by them.[3]

[3] For the record let me say that Kripke's ideas about these issues and mine seem to have developed in complete isolation from each other. Kripke's interpretation originated, as he recounts, in graduate seminars given in Princeton as early as the spring of 1965 and was subsequently developed through a series of conferences and colloquia from 1976 onwards. I first proposed such an interpretation of aspects of Wittgenstein's later thought on mathematics in my (1968); and the material that constitutes the first six chapters of *Wittgenstein on the Foundations of Mathematics* (Wright, 1980) was first written up for graduate seminars given in All Souls College, Oxford in the summer of 1974. Kripke and I were, indeed, colleagues for several months at All Souls in the academic year 1977–8, when he held a Visiting Fellowship there. But we never discussed the interpretation of Wittgenstein.

This is a natural thought which it is worth filling out briefly. Kripke's Wittgenstein holds, crudely, that there are no facts of the matter about what an expression means, how it is generally understood, what accords or fails to accord with a particular rule, what behaviour constitutes implementation of a particular intention, and so on. This is the Sceptical Paradox. We habitually talk as if there were normative realities, constituted by the contents of our sentences, rules, thoughts, intentions, and so on, but the truth is that there are none such. The whole conception of facts to do with meaning and its cognates is mythology.

This wild and absurd-seeming thesis is backed by an impressive argument. In the first instance, Kripke's Wittgenstein constructs a debate about a token claim concerning any past meaning of mine—say, the claim that by '+' I formerly meant addition. I am to defend the claim and the sceptic is to contest it. You might think that even if I were to lose the debate, no conclusions about the *reality* of meanings, rules, etc. would be in prospect—the only conclusion licensed would be that the *epistemology* of claims about meaning was no more straightforward than, under sceptical pressure, the epistemology of *the past* or *the material world* has turned out to be. But that would be wrong. Traditional forms of scepticism make much issue of what are taken to be intrinsic cognitive predicaments of ours—it is contended that we are, necessarily, screened from direct knowledge of others' mental states, the past, and the characteristics of matter, and are therefore restricted to inferences from behaviour, the present and our own experience. By contrast, the debate with Kripke's Wittgenstein's Sceptic proceeds under conditions of *cognitive idealization*: in my attempt to justify my claim that by '+' I formerly meant addition, I am presumed to have perfect recall of all aspects of my former behaviour and mental life. And the governing strategic thought is precisely that, *were* there a fact about what I formerly meant by '+', it would have somehow to be constituted in aspects of my former behaviour and mental life; and would therefore, under the idealization, be salient to me. Accordingly, if I still lose the debate with the Sceptic, even so idealized, it follows that there can indeed be no such fact. This conclusion is then easily developed to generate, successively, that there are no facts about what I presently mean, no facts about what anyone else presently means, nor, therefore, any facts about what any expression means or, correlatively, about what uses comply with it.

Thus, in briefest outline, the overriding strategy of the argument for the Sceptical Paradox. And now it might seem quite straightforward how these ideas, if sustained, would dislodge the realist conception of pure mathematics and support the opposed Wittgensteinian ideas which we rehearsed. The basic conclusion is that there are, necessarily, no facts about meaning. It follows that there can be no such thing as a reflective exploration within the domain of meanings, no such thing as creating a concept and then, by an analysis and proof, verifying characteristics of it which, unwittingly, we have put into our creation. There simply is no coherent conception to be had, if the Sceptical Paradox is accepted, of the subject matter to which such an investigation would be responsive. So pure mathematical proofs cannot be instruments of discovery concerning such a subject matter—there are no such discoveries to be made. *A fortiori*, they cannot be a source of a special, *cognitively earned* certainty; and any sense of constraint, or compulsion, which they inspire in us cannot properly be conceived as a by-product of recognition of our obligations, so to speak, to conceptual structures which we ourselves have erected.

On further reflection, however, the ability of the meaning-scepticism, developed by Kripke's Wittgenstein, to motivate the philosophy of mathematics propounded by the actual Wittgenstein comes to seem less clear. Wittgenstein, as the passages above quoted illustrate, did not merely repudiate the intuitive realist conception of mathematical proof and objectivity; he proposed, in addition, a suggestive alternative conception—the conception of the mathematician as the developer of 'new measures' of reality, the architect of 'new roads for traffic', new tracks for the use of language to follow. The proper exegesis of this positive direction in his thought would be a matter of detail which we cannot undertake here. But this much seems to be clear: the general drift of the proposal has to be *conservative* of our intuitive understanding of rules, and rule-governed practices. If the pure mathematician is to be seen, broadly, not as the explorer of a special domain but the inventor of new forms of description, new rules linking together concepts which we are already accustomed to apply in non-pure mathematical contexts, then there has to be such a thing as *changing and extending* the way a discourse is properly practised. And that is a notion of which we can make sense only under the aegis of the distinction between practices which conform with the rules as they were before, and practices which reflect a modification in those rules generated by some pure mathematical development. Unless, then, there is such a thing as practice which is in line with a rule, contrasting with practice which is not, there is simply no chance of a competitive construal of Wittgenstein's positive proposals. But that distinction, it would seem, is precisely what we have lost if what is driving the negative proposals is the meaning-scepticism propounded by Kripke's Wittgenstein.

Someone familiar with Kripke's text may want to protest that this is to ignore the role of the so-called Sceptical Solution. The Sceptical Solution attempts an accommodation with the Sceptical Paradox. A proponent of the Sceptical Solution grants that there are indeed no substantial facts about meaning, understanding, or any of the other cognate notions; but disputes that the propriety of ordinary discourse in which such notions are implicated has to be a casualty of that concession. The casualty is rather a certain conception of the kind of content which such discourse has—the conception, precisely, that it is a kind of content which may be explicated in terms of the idea of correspondence to fact. The role of a statement like

The rules of addition dictate that 29+13 is 42

is not to report a state of affairs, but, for instance, to express a condition compliance with which we treat as a criterion for competence in adding.[4] According to the Sceptical Solution, then, our right to continue with our ordinary talk of rules, and of what complies with them and breaches them, is not jeopardized by the Sceptical Paradox. What we lose is only a certain philosophical picture of what, when we engage in such discourse, underwrites the distinction between correct and incorrect assertions within it.

But it is doubtful whether this helps. Let us describe as *content-committed* all discourse which, in one way or another, deals with meaning or any of its cognate notions—in general, all discourse which falls within the scope of the Sceptical Solution if the Sceptical Paradox is accepted; and as *robustly truth-conditional* all putatively factual discourse whose status as such can survive the Paradox. It is a matter of controversy whether a proponent of the Sceptical Solution can provide in plausible detail the sort of semantic proposals for content-committed discourse which he owes. One, as it seems to me, very impressive reason for doubting so is that the scope of content-committed discourse threatens, given three natural assumptions, to become universal.

The first assumption is the platitude that the truth-value of a statement, as used on a particular occasion, is a function of its content, so used, and the then obtaining state of the world in relevant respects. The second is that it is an *a priori* truth, for anyone who understands English, that the result of substituting any truth-apt sentence in English for 'P' in the following schema will generate a truth:

'P' is true if and only if P.

The third is that no true biconditional can have as its constituents a fact-stating sentence—one about whose content a robust, truth-conditional conception is appropriate—and a sentence which is not apt for truth

[4] It might be said, similarly, by an opponent of moral realism that the role of the sentence

'Lying is wrong'

is not to describe a moral fact, but to express a condition compliance with which is a necessary condition for avoiding moral disapprobation.

and falsity at all, since it is not in the business of depiction of states of affairs but has a quite different role.

With these premises in place, we may reason as follows. Since, by the first, platitudinous assumption, truth-value on an occasion is always a function, in part, of content, and since—by the Sceptical Paradox—what it is correct to think about the content of a sentence, as used on a particular occasion, is not a substantial question, it follows that the truth-value of the sentence, as used on that occasion, is not a substantial question either. So no matter *what* English sentence we substitute for 'P'. '"P" is true' is never a robustly truth-conditional claim. And since, by our second assumption, it is co-acceptable with the claim that P, it follows—by the third assumption—that the claim that P is not robustly truth-conditional either. So nothing is robustly truth-conditional (cf. Wright, 1984, 769).

This reasoning may, perhaps, be resisted. In his (1989) Paul Boghossian independently develops a somewhat different argument to similar effect. Reacting to this argument, Simon Blackburn believes himself to find in it a confusion between use and mention; that is, he believes it illicitly jumps a gap between establishing a result, in point of robust truth-conditionality, about the *metalinguistic* assertion that 'P' is true, and justifying a conclusion in that respect about the object-language assertion that P.[5]

Which, precisely, of the two assumptions—the second and third—which just now enabled us to argue for the validity of that transition, Blackburn would want to deny, is a matter of speculation. But prescinding from the details of that argument, and of Boghossian's, it is hard to see in general terms how the result about the metalinguistic assertion could fail to be transferable. How could the claim that P escape the fate of its metalinguistic counterpart if the latter's fate is sealed merely by the involvement of content? Language is not a mere clothing for thought. We have no wordless contact with the thought that P: if we are to assess it, it has to be given to us *linguistically*. And our assessment will then be a function of the content which we find in its linguistic mode of presentation and of what we take to be the state of the world in relevant respects. Knowing what claim a particular use of a sentence makes is not and could not be a matter of pairing the sentence with an item that was somehow identified non-linguistically. If your and my sole language is English, then, in order to assess my claim that the cat is on the mat, it will be no less necessary for you to form a judgment about the content of 'The cat is on the mat' than if I had said that 'The cat is on the mat' is true. We have no grip on the question of

[5] Simon Blackburn, 'Wittgenstein's Irrealism' (forthcoming).

the truth-status of a claim that does not make it into the question whether a tokening of a sentence is true.

If all claims are content-committed—depend, in this way, upon the facts about meaning—then a proponent of the Sceptical Solution faces the daunting task of providing not merely a reconstructive, non-robustly truth-conditional semantics for all our declarative discourse, but also of explaining—when there are, perforce, no examples now to draw upon—what exactly it is that is being repudiated; what exactly robust truth-conditionality should be supposed to come to. And there is in addition a specific concern about how the *cogency* of the argument to the Sceptical Paradox can survive a non-truth-conditional reconstruction of its premises, lemmas, and conclusion. These issues are all very discussable.[6] But at present there seems every reason to question whether Kripke's Wittgenstein has the materials for a coherent philosophy of language.

That the package of Sceptical Paradox and Sceptical Solution is, if it is, unstable, is not, of course, a conclusive reason for refusing to ascribe it to Wittgenstein or for denying that it could have provided a key motive for his distinctive ideas about mathematics. But prudence, and the urge to learn, would dictate that we look for something better. What, if not that there are no substantial facts about the content of rules and about what complies with or breaches them, should the principal conclusion of Wittgenstein's discussion to be taken to be? And is there any prospect of a spin-off which, as Kripke anticipated, makes a case for each of the key features of Wittgenstein's philosophy of mathematics?

Since, for reasons that may already be apparent, the answer to the second question must, I believe, be negative, there is no point in getting embroiled—on this occasion—in the first. But I cannot forbear to say a little. Wittgenstein's general point, as I read him, is that coming to understand a particular rule, and acquiring thereby the ability to follow it in new cases, is not a matter of learning to keep track of something whose direction is dictated, somehow or other, independently of the judgements on the matter of anyone who might be regarded as competent. Wittgenstein's discussion of rule-following is directed against the mythological sublimation of rules and content, crystallized in traditional platonism, which allows the course assumed by the proper application of a rule—for instance, the identity of the series generated by an arithmetical function—to be thought of as generated purely by the rule itself, and of our judgements, when we competently follow it, as responses to states of affairs which are constituted quite independently. Such a picture seems to provide simultaneously both a certain

[6] And are further discussed in Crispin Wright and Paul Boghossian, 'Meaning-irrealism and Global Irrealism', forthcoming.

cognitive dignity for our rule-governed practices and an explanation of *why* we are able, by and large, to agree about what constitutes proper performance within them. It is rather as if we all had hold of the same railings. But for Wittgenstein, on the contrary, the conception of the content of a rule, thus sublimated, is unintelligible, and the epistemology of rule-following for which it calls—the account demanded of how we are able to *recognize* the requirements of rules when they are so conceived—impossible. In consequence, no real explanation is provided of our ability to concur in our use of language and in other rule-governed practices. And it is, indeed, a philosophical error to think that such an explanation is needed. No ulterior cognitive accomplishment underwrites our disposition, on receipt of similar explanations, to proceed to follow rules in similar ways.[7]

A host of questions arise, of course. The position outlined can maintain its distinction from that of Kripke's Wittgenstein, for instance, only if there can be an *unsublimated* conception of the content of rules, which nevertheless conserves our right to think of the requirements of rules as somehow substantial, and questions about whether a particular move is in or out of line as factual. Moreover it is all very well to say that we should not think of the requirements of a rule on a particular occasion as constituted, as it were platonically, in all independence of our judgements, but must there not also be some positive story: some account of the precise form of the dependence which we are being urged to recognize? Wittgenstein, it seems, thought not. And if so, then that view too requires explanation.

Whatever the correct upshot of engaging these various questions would be, it is hard not to feel that Wittgenstein is in range, in his discussions of rule following, of a most profound insight, even if philosophers have yet to clinch it definitively. And I think it is right that the general direction of his remarks, as just briefly characterized and without further analysis, does suffice to explain *some* aspects of his philosophy of mathematics. If our ongoing judgements are somehow primitively involved in determining the content of the rules that compose the decimal expansion of Π, would that not suffice to explain remarks like:

> however queer it sounds, the further expansion of an irrational number is a further expansion of mathematics . . . I want to say, it looks as though the ground for a decision were already there, and it is yet to be invented. (RFM, IV, 9, quoted above)

It is *we* who compose the decimal expansion of Π by the judgements concerning its expansion which, step by step, we find, ultimately,

[7] For further discussion of these ideas, and of the perplexities they generate, see my (1989a, b).

agreeable; for in making these judgements we do not align ourselves with requirements that are somehow constituted independently of us, but apply concepts primitively, in the sense that the conformity of such applications to some externally constituted standard makes no sense.

The truth is that the ideas on rules can motivate much of what Wittgenstein says about platonism in the philosophy of mathematics, and about mathematical objectivity and logical compulsion—and, in general, can explain his opposition to ideas about mathematics that overlook what we might call the 'anthropological contribution'. What they cannot explain are his distinctive remarks about proof and the status, in point of certainty, of the conclusions of proof. And we have, in effect, already glimpsed the reason for saying so, in the train of thought, levelled against the Sceptical Solution, that led to the conclusion that all discourse is 'content committed'.

That train of thought was that, since the recognition of meaning is an inextricable ingredient in the appraisal of any statement whatever, any general thesis about the epistemology and objectivity of meaning will be liable to widen into a thesis about the epistemology and objectivity of discourse in general. If, as in Kripke's discussion, the thesis is taken to be that discourse concerning such matters is devoid of genuinely factual content, then as noted, we get the dubiously coherent conclusion that that is the situation of all declarative discourse. If on the other hand the claim is the, in intention at least, more modest one which we are now entertaining—not that meanings, rules, etc., have no reality but that the truth about them and their requirements is somehow constitutionally responsive to our ongoing judgements and reactions—then the conclusion will be that truth in general is constitutionally responsive in the same way. The argument appealed to the platitude that the truth-value of a sentence, as used on a particular occasion, will be a function of its content and the state of the world in relevant respects. If its content, as so used—or, what comes to the same thing, the condition on how the state of the world in relevant respects has to be in order for the sentence to express a truth—is, so to speak, unmade in advance of appropriate, primitive judgements from us, then so is the truth-value of the utterance of the sentence in question. And now it seems that, in whatever sense the ideas about rule following afford the consequence that the further expansion of Π is a mathematical novelty, *all* discoveries, in whatever area of inquiry, are likewise novelties. We cannot keep our thumbs out of the scales anywhere.

But it is unmistakable that Wittgenstein intended a *distinctive* thesis about proof. His claim was not that, in some hopelessly sublimated sense of 'discovery', exploded by the rule-following considerations, proofs are not instruments of discovery; it was that, even when the notion of discovery is viewed aright, and many ordinary things do stand as discoveries, mathematical proofs do not. If the foregoing thoughts are right, then the ideas about rules, whatever the best development of their general direction and detail, cannot substantiate this claim. So we should think again.[8]

III

How should the claim that something is, or is not, a technique of discovery be appraised? Well, whatever constitutes the *discovery* that a proposition is true had better be a process, or sequence of events, which leaves someone who fully understands that proposition with no justifiable option but to assent to it. But of course one may always justifiably refuse assent to the conclusion of a proof in mathematics if one may justifiably refuse assent to the claim that what is presented is indeed a proof.[9] It follows that proofs are properly regarded as instruments whereby we discover mathematical facts only if there is in general no justifiable way whereby the status of a proof may be disputed—at least a wide class of proofs must be such that one who fully understands the concepts involved and works through all the steps has no justifiable option but to assent to the claim that what is presented is indeed a proof.

If this is right, then an effective way of attacking the mathematical realist conception of proof is to argue that a proper understanding of each of the notions in play in a proof and a full empirical awareness of its detail never completely constrain our assent; that the status of something as a proof is left underdetermined by its strictly cognitive aspects. When we ratify a proof, we go beyond anything that is required of us purely by acknowledgment of features of the presented construction.

This is, as we have noted, consonant with the general direction of Wittgenstein's remarks. Proofs do indeed do less than compel our assent:

> Do not look at the proof as a procedure that *compels* you, but as one that *guides* you. (RFM, III, 30)

But what does Wittgenstein say to make it clear that this is a practical option? What would it be to *refuse* the 'guidance' offered by a particular (putative) proof? The whole point about good proofs is that they strike us as *cogent*. And Wittgenstein himself stresses that it is of the essence of proof that it produce complete conviction:

[8] This point was perhaps the most important factor determining the overall direction of the argument of my (1980).

[9] Provided, of course, that no other proof of the same conclusion is known to one.

> A proof shows what *ought* to come out.—And since every reproduction of the proof must demonstrate the same thing, while on the one hand it must reproduce the result automatically, on the other hand it must also reproduce the *compulsion* to get it. (RFM, II, 55)

How can these remarks be made to cohere? How can it be of the essense of proof that it produce complete conviction and, at the same time, of the nature of proof that it merely guides towards the conception of things embodied in its conclusion?

Take a simple example: suppose I calculate that 26×23=598. The calculation constitutes a proof only so long as it secures my conviction that no other outcome than 598 is possible if I correctly multiply 26 by 23. For if I have any doubt about that, I will not consider that I have a proof. Yet—according to Wittgenstein—I am simultaneously supposed to be able to see the proof as providing guidance, rather than compulsion, as constituting merely, as it were, an advocate of the complex set of convictions involved in accepting its conclusion—the conviction, for instance, that if 598 two-inch square tiles do not suffice to cover a rectangular surface, which I have measured as 46 inches by 52, then I must have mismeasured. So I am simultaneously *both* to see no alternative to accepting the proof which the calculation accomplishes, and the ways of looking at things dictated by its conclusion—otherwise I will not be persuaded that I have a proof at all—*and*, following Wittgenstein's advice, to view the proof not as *teaching* that its conclusion is correct, but merely as guiding me towards a decision, as it were, to count it so. How is this schizophrenic feat to be carried off?

The tension is resolved when we take the remarks about compulsion to pertain to the *phenomenology* of proof, and the talk of guidance to relate to its *cognitive status*. Consider an analogy. No-one but a theoretically committed philosopher would think that the sense of humour is, literally, a cognitive sense—something which enables us to detect real comic values, out there in the world. Finding a situation funny involves, no doubt, cognition of many features of it; but the comic response itself is contributed by the affective, rather than cognitive, side of our nature. Nevertheless, we should not count as finding a situation funny if our reaction to it were somehow a matter of choice. It is of the essence of comic responses that they seem to the responder to be elicited from without—you can simulate finding something funny, but you cannot bring it about by will that you do.

So also, in Wittgenstein's view, with the intellectual response—the conviction—generated by a proof. If a calculation is to impress us as proving its result, it has to convince us that no other outcome is possible if the sequence of operations which it contains are performed correctly. But it is one thing to say that we register that conviction in judging something to be a proof; another to say that the conviction is a purely cognitive accomplishment.

The question remains: given that the phenomenology of proof is essentially compulsive, wherein consists the cognitive freedom which Wittgenstein thinks we nevertheless possess? To get a sense of the kind of thought which leads him here, we need to look at his repeated discussions of the relations between proof, especially calculation, and experiment.[10]

Typical proofs, and all calculations, involve a *process* we start at a particular point, run through a series of prescribed operations—correctly, to the best of our ability—and get a certain result. So too with, say, a laboratory experiment: we set up an apparatus in a certain initial state, carry out certain operations upon it, and a certain result ensues. And both a proof and an experiment which, when repeated, consistently gives the same result, may prompt our assent to a conditional statement along the following lines:

> If, starting on such-and-such a basis, such-and-such procedures are properly carried out, then such-and-such results.[11]

This statement need not, in the case of proofs in general, be that which we regard the proof as primarily proving—though it always is precisely that in the case of a calculation. But one thing is clear: our willingness to acknowledge a proof as a proof—what marks it off for us from an experiment—depends upon our willingness to accept that this conditional statement holds of *necessity*: our willingness to accept that there is, in the case of the process in question, an *internal* connection between basis, steps and outcome.[12]

You can begin to get a sense of Wittgenstein's concern if, bearing in mind the very analogy between proof and an experiment, you now ask: what is it about a calculation, say—about the routine of carefully running through a calculation—which puts us in position to make a *necessitated* assertion? What do we recognize about the calculation that

[10] A selection of relevant passages from RFM (second edition) would include part I, sections 36–57, 75–103 and 156–64; part II, sections 55 and 65–76; part III, sections 46–53; and part V, sections 6, 14–15, 17 and 40. Germane material published for the first time in the third, revised, edition includes Appendix II, part VI, sections 1–10, 15–16 and 36; and part VII, sections 25–6. Cora Diamond's edition of the (1939) *Lectures on the Foundations of Mathematics* (LFM) touches on the issues in lectures iii (36–9), vii (71 and following), x and xi *passim,* and xiii (128–30).

[11] What I have elsewhere called the *corresponding descriptive conditional*. See my (1980), p. 452; also (1986c), pp. 203–4; and (1989c), pp. 231–2.

[12] This is what Wittgenstein means when he says—RFM, III, 41—that *causality* must play no part in a proof.

justifies the claim that the particular result *must* result from that particular process on that particular starting point? Why is our entitlement not exhausted by the claim, merely, that this is what resulted when, so far as we can tell, the calculation was done correctly—and probably always will result when we are so convinced?

The point to be explained is not that we are supremely certain of the truth of the corresponding conditional in the case of a calculation and merely, say, highly confident in the case of an experiment. I may well be more confident about the repeatability of an experiment than about the conclusion of a complex calculation, but still prepared to assert both the relevant conditionals. The crux is rather that, in the case of a calculation, we make a claim of a quite different kind. The 'must' is an expression, not of certainty, but of the conviction that the conditional, if true, is sustained by factors quite other than those which sustain its experimental counterpart. Yet the confirmatory processes seem similar; in each case we have, surely, only ordinary empirical grounds for identifying the starting point and conclusion, and only ordinary empirical certainty that appropriate controls have been properly applied on the intervening steps.

This is the problem that runs through much of Wittgenstein's discussion. A common thought, which will see no problem here, will want to credit us with a special intuitive faculty—a necessity-detector, as it were—which is summoned into action in 'reading' a proof. At the purely empirical level there is, it will be granted, no material difference between a proof and an experiment. And that just shows that, in order to detect the difference—to distinguish processes in which basis, steps and outcome are all internally related from processes in which they are not—more than merely empirical faculties are demanded.

But this response is at once obscure and *ad hoc*. And now we may feel the attraction of a simple opposing strategy of response: make out that the contrast between the two kinds of process, so far from needing explanation in terms of special cognitive faculties, is not properly speaking, a *cognitive* contrast at all. Consider a favourite example of Wittgenstein's: a rule of conversion between units of measurement in distinct systems—say, 'One inch equals 2.54 centimetres'. If we were at a point in history when there had been no interaction between users of the Imperial and Metric systems respectively, we would have to set about appraising that statement empirically: measuring objects using instruments calibrated in each unit, and determining statistically the limit of the ratios in which the results stood. Yet the proposition, if true, is surely *no contingency*—it expresses an internal relation between the concepts of correctly measuring in centimetres and correctly measuring in inches. So a similar problem arises: how could we possibly verify that an internal relation obtained by (broadly) statistical empirical methods?

The answer is that we do not. Rather, we *already* have the idea that there must be an internal relation here, and what we identify empirically is the *best candidate* for what the internal relation is. And so too, in Wittgenstein's view, with a calculation: the calculation excites no special intuitive faculty—rather we are already 'in the market' for an internal relation between basis, process and outcome, and doing the calculation, perhaps with repeated checks, makes an ordinary empirical case for what the internal relation in question is.

The crucial issue accordingly becomes: what puts us 'in the market'? Do we *recognize*, by dint of who knows what cognitive faculties, that the rules of multiplication *can* only ever generate one result when correctly applied to a particular set of factors? Or is it rather that here 'correctness' precludes variation in the outcome as a matter of (something akin to) *convention*?

The contrast, thus expressed, is crudely drawn. But many commentators have found in Wittgenstein a view of the second broad sort—albeit one in which the notion of convention is softened by considerations concerning our natures and unreflective practices. We do not, as it were, explicitly lay it down that the rules of multiplication do not count as correctly applied in cases where variation in output is unaccompanied by variation in input. It is merely that

> *We do not accept* e.g. a multiplication's not yielding the same result every time. (RFM, III, 52)

—our *practice* is not to tolerate such variation. And whereas, for the cognitivist, this practice is backed by a *perception*—an insight into what the rules of multiplication make possible—for Wittgenstein it may be accepted simply as primitive and groundless:

> This is use and custom among us, or a fact of our natural history. (RFM, I, 63)

What are the options if our inclination is to try to underwrite the 'must' which we incorporate into the conclusion of a calculation, with a substantial epistemology? Only the two canvassed, it might seem: *either* the details of a specific calculation somehow excite an intuitional recognition that what results must result, *or* there is only an empirical routine, but a routine which is informed by the general *a priori* insight that proper implementation of these rules cannot generate variable outcome. But either line precariously offers hostages for redemption by the theory of knowledge.

Wittgenstein's alternative, in contrast, promises what may seem the clear advantage of handling the mathematical 'must' within an empiricist epistemology. In Wittgenstein's view, the internal relations which articulate the interconnections within a proof are acknowledged by way of institution or custom, following on empirical findings. And this background of unreflective custom is not something in which our participation is imposed by purely cognitive considerations. So a faultless understanding of each of the notions in play in a proof and a full empirical awareness of its detail underdetermine assent to it; one needs, in addition, to be party to the relevant practices. And if that is right, then—by the criterion I offered at the start of this section—proofs are not instruments of discovery.

It remains to record the advertised cause for dissatisfaction. This is that the two alternatives we canvassed for the cognitivist—construction-triggered intuitions, or background *a priori* insights—do not exhaust the field. The reason is that the simplest ingredient steps in a calculation, or in formal proofs in general, are typically certified by operations which are primitively given as *functions*: operations which are *identified*, in part, by the characteristic that only variation in input can generate variation in output.[13] Wittgenstein's view has it that a culture might deploy the same arithmetical concepts as we do, but without the institutional setting that leads us to dignify the products of calculation as necessary. That requires that there be fully adequate modes of explanation of arithmetical operations that are, so to speak, neutral on the question whether their output may vary for fixed input. And that is most implausible. There is no hiatus between an understanding of arithmetical addition, for instance, and the knowledge that it is a function. Rather, it has to be an explicit part of any adequate explanation of the concept of addition that no pair of numbers has more than one sum. Not to know that is not to know what adding is. There is no stripped-down concept which is somehow neutral on the matter.

The crucial Wittgensteinian claim is that participation in the 'institutional setting' is no part of the conditions for *understanding* arithmetical operations. If that is wrong, then someone who fully understands those operations and properly assesses the empirical features of a correct calculation will have no rational option but to assent to its finding. And that will restore a scaled-down cognitivism, independent of intuitions and *a priori* insights: recognizing the 'must' that underlies a calculation will simply be a matter of applying concepts as one was taught them to an empirically given construction. These matters need much fuller discussion beyond the scope of this paper. But perhaps the preceding will convey at least something of the geography of the issues raised by Wittgenstein's mature thought about mathematical proof.[14]

[13] I am here by-passing complications to do with rules of inference, like vel-intro., which permit more than one conclusion from given premises. For further discussion, see my (1989c), pp. 234–5.

[14] For a somewhat fuller account see my (1989c), section IV.