2

What Mathematics Has Done to Some and Only Some Philosophers

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1. How is pure mathematics possible?

ACCORDING TO BERTRAND RUSSELL, 'the question which Kant put at the beginning of his philosophy, namely "How is pure mathematics possible?" is an interesting and difficult one, to which every philosophy which is not purely sceptical must find an answer'.[1]

Russell exaggerated. Many philosophies that are not purely sceptical have had no interest in Kant's question. It never even occurred to them, much less struck them as important. I shall not be invidious, but we can quickly think of canonical Western philosophers of all periods who have not troubled themselves with mathematics at all. Hence the 'some and only some' of my title. But the spirit of Russell's remark is right. A great many of the philosophers whom we still read have been deeply impressed by mathematics, and have gone so far as to tailor much of their philosophy to their vision of mathematical knowledge, mathematical reality, or, what I think is crucial, mathematical proof.

Why do so many philosophies try to answer Kant's question? And why, incidentally, do a great many not address it? We need not distinguish between Russell's talk of philosophies and individual philosophers, so long as we examine only a few famous philosophers each of whom has defined a philosophy. I shall employ the

[1] Bertrand Russell, *The Problems of Philosophy* (London: The Home University Library, 1946), p. 84. Kant's question is stated in *The Critique of Pure Reason*, translated by Norman Kemp Smith (London: Macmillan, 1929), p. 56 (B 20).

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very opposite of Myles Burnyeats's skilful scholarship, for I am less concerned with philosophical texts or doctrines than with the broader question of why a philosopher should have become obsessed by mathematics as a source of philosophical inspiration. I shall seem to be naïve. My quest is phenomenological: what is there about the immediate feel of this or that piece of mathematics that has fascinated this or that philosopher?

I say 'piece of mathematics', because we ought to look at mathematics in action, proofs more than theorems, vital understanding rather than quiescent truths, discovery as much as knowledge. Experiences connected with live mathematics have driven the philosophers who have built cornerstones out of it. This is true not only of a Descartes or a Leibniz, mathematician–philosophers, but also of astonished onlookers, a Plato or a Wittgenstein.

What struck the philosophers? We know what troubled Bertrand Russell in 1912. 'The apparent power of anticipating facts about things of which we have no experience is certainly surprising.'[2] Our name for the phenomenon that surprised Russell is 'a priori knowledge'. Both the 'necessary' that figures in the title of this symposium and the 'a priori' which occurs in the title of Russell's chapter are not so much descriptive adjectives as demonstrative ones, pointers that gesture at something that feels remarkable.

1.1 Philosophy and mathematics

When Professor Smiley arranged these Dawes Hicks Lectures, he suggested that my topic should be philosophy *and* mathematics. Not the philosophy *of* mathematics. I shall respect that suggestion. An answer to Kant's question, 'How is pure mathematics possible?' would be a contribution to the philosophy of mathematics. I shall not defend or even seriously examine any answer, old or new. Some philosophers have drawn quite extraordinary inferences from the possibility of mathematics. We should reflect in an immediate and almost childlike way on the elementary phenomena that fascinated them, in order to grasp that question, 'How is pure mathematics

[2] Russell, *Problems of Philosophy*, p. 85.

possible?' Too often we are pleased to fly off into subtlety or technicality without asking what worries us.

I shall refer to six different philosophies, and place them in two groups. The first group could be described as inflationary, the second as deflationary. Two of my inflationary philosophers, Plato and Leibniz, draw remarkable conclusions about, well, everything, from their experience of mathematics, while the third, John Stuart Mill, sees himself as doing mighty battle against such inflation. Inflation leads philosophers to make grotesque claims about everything. Deflation, on the other hand, leads philosophers to say things about mathematics itself that are widely regarded as absurd. Think of Descartes, who apparently held that God could make two plus two equal to five, or of some of the more curious of Wittgenstein's *Remarks on the Foundations of Mathematics.*

I am not here to say who is right and wrong, but I do think, paradoxically, that all my chosen figures are, in a sense, right. I do not mean that their philosophies are right — it would be impossible for so many contradictory doctrines all to be true at once. I mean that each was right to be astonished by mathematics, and to follow that astonishment as far as their extraordinary imaginations would take them. In each of my two groups, the inflationary and the deflationary, there is one philosopher who is more down to earth than the others, namely Mill in the first group and Lakatos in the second.

1.2 Infection of philosophy by mathematics

How has the felt need to answer Kant's question affected those parts of a philosopher's work that are not concerned with mathematics? How has philosophy been infected by mathematics? Infection has negative connotations of illness and disease, but the effects of taking Kant's question with high seriousness have indeed been bizarre. We tend to play down the exotic features of a philosophy influenced by mathematics. We do not want to take the florid turns of phrase very seriously. But only when we take them literally do we fully grasp the enormity of the conclusions the philosophers would foist on us on the basis of a relatively minor part of human culture.

To refer to mathematics as minor is not to demean it. Mathematical achievement may be among the half-dozen most profoundly human excellences, but it plays little role in the social or cultural life of even those communities, like our own, whose technologies and hence ways of life have been critically formed by mathematical reasoning. We know of only one major figure in the entire history of Western thought who imagined that mathematics ought to be culturally central, studied by aspiring statesmen for many of their mature years before they were allowed to engage in statecraft. When you realise, after reading Burnyeat (Chapter 1), that that was exactly what Plato meant, you realise how weird was the influence of mathematics on the whole of his philosophy, including his political science and his pedagogy.

1.3 One and many

But what is mathematical reasoning? Is there one definite type of human reasoning that counts as distinctively mathematical, and has been recognised as such for three millennia? We should be wary of anachronism, but not to the point of pedantry. Even if we disagree about the essence of mathematics (or whether it has an essence), we can agree about some of the examples that have impressed some philosophers. Even if a Euclid and a Hilbert might have defined mathematics differently, they could have agreed on some examples of what counted as mathematics.

Mathematics is characterised by unity and diversity. Without some unity, it would make little sense to discuss 'the' philosophy of mathematics. I insist on diversity because a great many philosophical questions about mathematics arise by emphasising a phenomenon that is felt very deeply for this or that example, and then by falsely generalising, making it into a characteristic of any mathematics. I shall emphasise difference, but first unity.

We seldom have difficulty in recognising what is and what is not a piece of mathematics. We recognise what sorts of problems might and which sorts of problems surely will not have mathematical solutions. New territory gets opened up over and over again — chaos theory, or the use of probabilities in that purest and most exact of sciences, number theory. We may debate the merits of intuitionism or constructive mathematics, but we know they are mathematics. At present it may take a year or more for experts to be sure that a new proof idea is sound, but we seldom hear the query, 'but is it mathematics?' We do encounter this question in the case of genuine territorial expansions. Computer-generated proofs furnish an example. They are completely beyond the human bounds of perspicuity, surveyability or Cartesian *intuitus*. 'We now know the solution to the four-colour problem — but is it mathematics?'

We recognise para-mathematics such as chess problems or computer programming. There are differences between combinatorial and spatial reasonings, attested by differences between the Greek and the Arabic heritage, and even by current muddy locutions such as the terms 'analogical' and 'digital' to distinguish ways in which wristwatches tell the time. Some people are better at one type of reasoning than the other, in ways that come out when performing tasks quite a long way from what we usually call mathematics — the use of maps, for example. Are these differences in taste, or in abilities? Maybe nature has more to do with these individual variations than nurture. Perhaps different parts of the brain are activated. The differences fade into matters of taste compared to our fairly uniform ability to recognise problems and methods of reasoning as mathematical.

Thus historians of mathematics can usually work their way into old proofs: contrast the plight of historians of laboratory science who sometimes simply cannot reproduce the effects carefully reported by esteemed past experimenters: Laplace's measurements on the velocity of sound, which confirmed his theory of caloric, come to mind. There is a school of the history of Greek mathematics, exemplified by the work of the late Wilbur Knorr, which dates parts of the existing corpus by arranging methods of proof in a sort of archaeological hierarchy; the more 'archaic' the proof method, the older the result is likely to be. This inferential procedure fits in pretty well with the scrappy temporal sequence of datable documentary reports that we possess.

The very integrity of mathematics makes it quite plausible to suppose that its mental resources might be located in a genetic inheritance, part of human nature. I have to leave such fascinating topics to the speculators. Our talk of mathematics, *tout court*, relies simply on our practices. Which comes first, institution and pedagogic organisation, or the intrinsic nature of the subject determined by uniquely mathematical cognitive potentials? It does not matter here. But as philosophers we pay a certain penalty for the felt integrity of mathematics. Some examples of mathematical reasoning and proof strike us very strongly, and some of us find it natural to take what is, in a common-sense way, true of those examples, and then think it is true of all mathematics. Finally we try to discover *the* nature of mathematics that explains what was true of the examples. Or, what amounts to much the same thing, one genre of philosophy of mathematics consists of finding something problematic in some examples, and then inventing a theory to explain that phenomenon, followed by, 'and that's how it is in all mathematics'. This is surely one reason that Wittgenstein wanted 'to give an account of the motley of mathematics'.[3] He thought that by doing so he could discourage quick generalisation of the sort that is so apparent in his own tantalisingly brief statements about mathematics in his *Tractatus Logico-Philosophicus*.

1.4 Pure mathematics

The notion of pure mathematics, which figures in Kant's question, is less firm than that of mathematics itself. Our distinctions between pure and applied were not present to Plato. They are not even the same as the distinctions made in Kant's time. It takes quite some work to understand the older distinction between pure and mixed mathematics. We count as empirical what Kant himself thought was a priori, for instance the whole of rational mechanics, stemming from Galileo and Newton and of which, in Kant's day, Lagrange was the master. Kant wanted to understand not only how pure mathematics is possible, but also how rational mechanics

[3] Ludwig Wittgenstein, *Remarks on the Foundations of Mathematics*, ed. G. H. von Wright, R. Rhees and G. E. M. Anscombe, tr. G. E. M. Anscombe (3rd and rev. edn, Oxford: Blackwell, 1978), III §48, p. 182. Cf. the text for n. 52 below.

is possible. Russell quietly ignored the fact that Kant put not one but two questions 'at the beginning of his philosophy', set out like this:

> How is pure mathematics possible?
> How is pure science of nature possible?[4]

His answer to the second question, given in the transcendental analytic, is different from, but formally analogous to, his answer for arithmetic and geometry, his paradigms of pure mathematics, given in the transcendental aesthetic.

Despite these qualifications, I shall follow Russell and suppose that a family of phenomena worth calling 'pure mathematics' has prompted a lot of speculation in Western philosophy from the pre-Socratics to the present. These phenomena have seemed perplexing. Theories have been advanced to explain them. Then we start debating the merits of the theories. Or even if we do not theorise, we generalise. We take a phenomenon that is apparent in one example, and imagine that it appears in a whole range of examples. Yet, on inspection, it does not. We have to look directly at the phenomena, and not at the imperial theories in which we clothe them. I am well aware that this is not strictly possible, for the theories have shaped our sensibilities. But we can try. Let us begin with three extreme reactions to mathematics — Mill, Plato, and Leibniz.

2. *John Stuart Mill*

Some philosophers who have answered Kant's question have *not* felt any deep-seated need to answer it. Take Mill. He certainly had an answer, namely, that statements of pure mathematics are high-level empirical generalisations founded upon induction. Frege jeered at him on this score, but the idea is not without merit. A version of it, subject to numerous cautions, has been put forward in our own time, although the explicit debt to Mill is, for reasons of

[4] *Critique of Pure Reason* B 20, Kemp Smith, p. 56.

tact, played down.[5] What we notice, however, is that Mill's philosophy of mathematics had no impact whatsoever on his philosophy. As far as the rest of *A System of Logic, Ratiocinative and Inductive* is concerned, let alone *Utilitarianism* or *On Liberty*, Mill's philosophy of mathematics could have been anything at all, or nothing at all, plain silence.

Mathematics did not, to repeat my ugly word, infect Mill's philosophy. He was immune. Why? Because at the lowest but perhaps also deepest level of description, Mill did not feel a need to answer Kant's question. He took up the philosophy of mathematics only because of what it did to other philosophers, and because of the subsequent harm to innocent bystanders. He says as much in the *Autobiography*. The long passage is so moving, and so little read by philosophers of mathematics, that I shall quote quite a lot of it. 'Whatever may be the practical value of a true philosophy' of logic and mathematics, said Mill,

> it is hardly possible to exaggerate the mischiefs of a false one. The notion that truths external to the mind may be known by intuition or consciousness, independently of observation and experience, is, I am persuaded, in these times, the great intellectual support of false doctrines and bad institutions. By the aid of this theory, every inveterate belief and every intense feeling, of which the origin is not remembered, is enabled to dispense with the obligation of justifying itself by reason, and is erected into its own all-sufficient voucher and justification. There never was such an instrument devised for consecrating all deep-seated prejudices. And the chief strength of this false philosophy in morals, politics, and religion, lies in the appeal which it is accustomed to make to the evidence of mathematics and of the cognate branches of physical science.[6]

Mill deeply opposed what he took to be the reactionary philosophies of Sir William Hamilton and William Whewell, the immediate target of his assault. The chief need that Mill felt, in connection with mathematics, was to denounce a philosophy of mathematics

[5] Philip Kitcher, *Mathematical Empiricism: The Nature of Mathematical Knowledge* (New York: Oxford University Press, 1983). When I emphasised the Mill connection in my piece on Kitcher's book in *The New York Review of Books*, 16 February 1984, Kitcher wrote agreeing with this take on his work.

[6] John Stuart Mill, *Autobiography* (London: Longman, 1873), pp. 225–6.

that produced a pernicious attitude to life itself. He continued his discussion in the *Autobiography* with the words:

> In attempting to clear up the real nature of the evidence of mathematical and physical truths, the 'System of Logic' met the intuitive philosophers on grounds on which they had previously been deemed unassailable; and gave its own explanation, from experience and association, of that peculiar character, of what are called necessary truths, which is adduced as proof that their evidence must come from a deeper source than experience.

Mill is my exemplar of a philosopher who did not feel that there was anything unusual about mathematics, except its sheer generality. He wrote chapters on the philosophy of mathematics because he thought that other philosophers made horrible extensions of their theories about mathematics. But since he himself felt nothing much about mathematics, he did not allow it to infect the rest of his philosophy.

3. Plato

Mathematical Platonism is perhaps the least interesting aspect of Plato's philosophy. This use of the very name 'Platonism' is apparently a neologism, commonly traced back to a lecture given in 1934 by the logician, set-theorist and mathematician, Paul Bernays.[7] Plato did hold that there is a domain of objects that is in some way intermediate between the domain of objects that we encounter with the senses, and the domain of Forms or Ideas. He held that mathematical propositions are true of objects in this intermediate domain. This idea is not so out of the ordinary as is usually made out. Every distinct style of reasoning introduces a new domain of objects and hence provokes an ontological debate — or so I contend elsewhere.[8] If that is correct, mathematical Platonism should be seen as a characteristic by-product of a way of thinking

[7] Paul Bernays, 'On Platonism in Mathematics', tr. C. D. Parsons, in P. Benacerraf and H. Putnam (eds), *Philosophy of Mathematics* (Englewood Cliffs, NJ: Prentice-Hall, 1964) pp. 274–86. (German original 1934.)

[8] Ian Hacking, '"Style" for Historians and Philosophers', *Studies in History and Philosophy*, 23 (1992), 1–20.

rather than as something unusual. Plato's other inferences about mathematics are more shocking.

Now I shall tread where I have no right to tread, except that every citizen must, sooner or later, step warily in Plato's footsteps. I shall refer to Plato's *Meno* not as a scholar but as a member of the reading public who has always found a part of the dialogue curiously exciting, less as philosophy than as parable. I mean of course the story of the slave boy and the Pythagorean problem of producing a square twice the area of a given square. Talk about infection: Socrates conjectures, on the basis of this demonstration, that the boy has an immortal soul and recollects how to double the area of a given square, knowledge that he had in a prior existence. That is astounding, whereas mathematical 'Platonism' in itself is rather drab.

3.1 Leading questions

Scholars have endlessly debated the extent to which Socrates cheated, implicitly leading the boy to the solution of the problem. That is of little moment. Yes, Socrates did pose leading questions. What is important is that both the boy, and we the readers, can be led to see that a certain solution is correct, and to understand why it is correct. Being told the solution, even being led to the solution, does not do the trick. The real stunner is that once you see how the proof goes, you understand why that square on the diagonal is twice the area of the given square. Socrates could have given plenty of examples of how it is possible to learn by being questioned, but which do not lead to this kind of understanding. Imagine this fragment:

> *Soc*: How long ago did Pericles deliver his famous funeral oration?
> *Boy*: A hundred years ago, I suppose.
> *Soc*: No, seriously now, how long ago was it, do you think?
> *Boy*: Forty years?
> *Soc*: Now think carefully. Did your mother ever tell you about that speech?
> *Boy*: Often. She was present, against all the rules, with her mistress too, though of course she was a mere child.
> *Soc*: And how old was she then?

Boy: Ten.
Soc: How old are you?
Boy: Oh, I'm a man now, 15.
Soc: And are you the oldest son of your mother?
Boy: Yes, but I have an older sister.
Soc: Was your mother already an old woman, then, when she gave birth to you?
Boy: No, she was 20, or so she tells me.
Soc: So how long ago did Pericles give his funeral speech?
Boy [grudgingly]: I see what you are driving at. She heard the speech ten years before I was born. So Pericles gave the greatest oration that the world shall ever know just 25 years ago, in all.

Such a fragment would be a fine discovery. We could infer that the dialogue *Meno* is presented as taking place in 405, the speech having been given in 430. But the fragment would not serve the philosophy of mathematics. Just as in *Meno*, my imagined Socrates does use leading questions to help the boy solve a problem. If Socrates were interested only in the Socratic method of eliciting what an audience 'already knows' (implicitly) by getting it to think things through, he could well have used something like my fragment to make the point. After the boy has been led to his conclusion, he knows when the speech was given, but not why that was a quarter-century earlier. He does not understand the fact he has uncovered. The most stunning element of *Meno* is absent.

That is the critical difference between my phoney fragment and Socrates' use of the proof of the Pythagorean theorem. Of course there are other differences. For example the dating in my fragment is not certain, in the way that the theorem is certain, for the mother may have lied to her son about her age. The boy's inference about Pericles rests upon empirical evidence. It is not a priori. The conclusion itself is not a necessary truth. The boy could conclude with the words, 'so the speech must have been given only 25 years ago'; but that 'must' is the 'must' of drawing a conclusive inference from data, not the 'must' of pure necessity. And so forth. Yes, there are many differences between *Meno*'s example and mine.

My example would nevertheless be a perfectly good example of Socrates' method of eliciting implicit knowledge — complete with its leading questions. But it could not engender a philosophy of mathematics or a conjecture about the immortality of the soul. That is why I do not follow a lot of scholarship, and examine the extent to which the slave boy is led by Socrates to his discovery. The point of the dialogue is different; it is a point about proof.

3.2 Perspicuity: grasping the proof

Notice that in the case of the theorem, Socrates makes plain that the boy has to think about it, to rehearse the argument before he fully understands it. He will not become really convinced until he grasps the argument as a whole. Socrates and the boy may look at pictures of squares and triangles drawn on sand. As an aside Socrates makes clear that minute observation and measurement of the drawings is immaterial — they could have been larger or smaller, the angles are not exact.

At this juncture we should be personal. I was bowled over by the argument (as I was by the related argument that, as we now conveniently express the Pythagorean doctrine of incommensurability, the square root of two is not rational). Not everyone is bowled over. Many people can struggle through *Meno* and be unimpressed. Oh yes, that is how you double a square. Who on earth wants to double squares anyway? This talent for square-doubling is worthless. I agree. The content of the theorem hardly matters. What impressed Plato, and what impresses me, is that by talk, gesticulation, and reflection, we can find something out, and see why what we have found out is true. It certainly impressed Kant: 'A new light flashed upon the mind of the first man (be he Thales or some other) who demonstrated the properties of the isosceles triangle.'[9]

The fact that we can see not only that the theorem is true, but also why it must be true, is one of the core phenomena of *some* proofs, the sheer feeling of having 'got it'. That feeling, we well know, can be illusory. Every would-be proof-inventor has had many a false 'Aha!' experience.[10] Plato was not ignorant of this.

[9] *Critique of Pure Reason* B xi, Kemp Smith, p. 19.

[10] I believe we owe this apt label to Martin Gardner, sometime columnist in the *Scientific American*: Martin Gardner, *Aha! Aha! Insight* (New York: Scientific American, 1978).

Firm reflection and an ability to recapitulate the argument insightfully were essential ingredients in grasping the proof.

Thus one feature of the proof in *Meno* is that it can be internalised. Descartes, it will be recalled, thought that his *Meditations* have just this character. That does not mean that an argument can be grasped all at once, in a flash, but that by rehearsal and reflection one can get it in the mind, and see it through all at once. I do not think that the *Meditations* have this characteristic, but we can see what Descartes was hoping for.

I find it natural to say that the proof in *Meno* is perspicuous, but this must be a consequence of reading Wittgenstein in translation. He returned often to the statement that 'a mathematical proof must be perspicuous' (*übersichtlich*) or 'surveyable' (*übersehbar*). He did not repeatedly state it, but from time to time held it up for examination, writing it down in quotation marks.[11]

3.3 Anticipating facts

Perspicuity is only one feature of the proof in *Meno*. I said that the content of the proof is unimportant, but that is not quite true. The proof is about squares, and before we have become involved in high philosophy, we take it that there are squares. Or at any rate that we can, for example, stake out a square plot of land. Here, then, is a square. I offer to give you this plot, within my holdings. 'No, I want twice as much.' To satisfy you, I construct the square plot on the diagonal and offer you that. 'That is inconvenient, for it cuts across the direction of the ploughing, which was parallel to the original square.' So let us construct another square plot, with a side as long as that diagonal; you can have that.

Here we seem to have an instance of what Russell called 'the

[11] Wittgenstein, *Remarks*, 3rd edn. For *übersichtlich*, III §1, p. 143; IV §41, p. 246; VII §20, p. 385; cf. I §154, p. 95. The identical sentence *Der Beweis muss übersehbar sein* is translated as 'Proof must be surveyable' at III §55, p. 187, but as 'Proof must be capable of being taken in' at III §21, p. 159, and III §39, p. 170. This is an example of a general problem about the translation of these words throughout the entire Wittgenstein corpus. See J. Baker and P. M. S. Hacker, *Wittgenstein: Understanding and Meaning* (Oxford: Blackwell, 1980), pp. 531–4.

apparent power of anticipating facts about things of which we have no experience'. That is, we anticipate a fact about the third plot we have constructed before we have measured it. The possibility of doing so, as Russell continued my quotation, 'is certainly surprising.'

Perspicuity and anticipation name two related phenomena that help to give us the idea of a priori knowledge. We have generalised on this and said that much other mathematics is also a priori, but the starting-point, and that which creates philosophical astonishment, is with an example like *Meno*. For the present I stay as close to the phenomena as possible. In Section 5 I turn to the generalised notion of a priori knowledge.

I have not yet mentioned necessary truth, logical necessity, and related concepts. They are perhaps already present in the idea of 'anticipating' facts. This square plot is twice the area of the first square plot, because the side of the larger plot is the length of the diagonal of the smaller one. Is it? Let us check and see. Perhaps Socrates calls in an Athenian land surveyor to check the 'anticipation'. Yes, we can confirm, verify our prediction by land surveying. What if the surveyor concludes that the second plot is not twice the area, despite the construction having been made exactly right? Then he *must* have made an error, or perhaps the land has shifted, as happens when you stake out plots on permafrost. No disconfirmation will be accepted. This is the 'must' of logical necessity: no empirical counter-example will be allowed.

This comes out most strongly when we pass to a closely related theorem. The diagonal of a square is incommensurable with its side. Let us lay down successive lengths of the diagonal alongside and parallel to successive lengths of the side. The two lengths have a common measure when m of the former exactly match n of the latter. Otherwise they are incommensurable. Legend attributes knowledge of incommensurability to Pythagoras. Today we are more familiar with the brief elegant proof that the square root of 2 is not a rational fraction, which we take to express the Pythagorean result. Can we say that this theorem 'anticipates' that if we lay down lengths of diagonal and side, end to end, never the twain shall match? Not at all! You will probably find that 141 sides match with 100 diagonals, end to end.

The theorem does not anticipate nature. But that is not what we say. We conclude that our measurement was inexact. The theorem shows us that. We know a priori that there must be an error of precision. This 'must' manifests our conviction that what the theorem states is not only true, but is also necessarily true. The proof of the theorem shows us that it cannot be otherwise.

3.4 The immortality of the soul

If ever we had a case of mathematics infecting philosophy, it is the argument that continues in *Meno* after the proof. The possibility of mathematical discovery provides an argument for the immortality of the soul. Not a definitive argument — Socrates adds appropriate marks of caution — but a powerful one. I spoke of bizarre inferences to be drawn from the possibility of mathematics. That of *Meno* is exhibit No. 1.

3.5 Pedagogy

Plato used mathematical knowledge for a great many purposes. Myles Burnyeat has examined the reasons for making mathematics so central a part of Republican education. It has always seemed preposterous that a legislator could decree that future statesmen should spend their middle years studying geometry in preparation for their real responsibilities. Plato's state pedagogy was infected by his vision of mathematics. Exhibit No. 2.

3.6 Politics

There is also a direct application to politics, found in *Gorgias* (probably contemporary with *Meno*). It reflects Plato's live astonishment with mathematical proof. In an instructive caricature of the dialogue, Bruno Latour argues that Plato has made Socrates completely imitate the sophists, such as Callicles, who argue that might is right.[12] According to Latour, the sophists need might for

[12] Bruno Latour, *Pandora's Hope: Essays on the Reality of Science Studies* (Cambridge, Mass.: Harvard University Press, 1999), chap. 7, subtitled 'The settlement of Socrates and Callicles'.

Hobbesian reasons, to control demos, the rabble, the mob. Socrates needs the same control, and so he mimics Callicles, but the might he deploys is stronger even than a phalanx of armed guards. Socrates can deploy the inexorability of mathematical proof. This is the guise in which logical necessity enters Platonic thought. 'How many divisions has Pope Socrates?' Infinitely many. Following Latour I exaggerate, but the exaggeration reminds us of what Plato does, literally, argue. His political science has been infected by an obsession with the sheer experience of proof, with a priori knowledge, necessary truth. Compelling proof is enough, Plato fantasised, to compel the mob. Exhibit No. 3.

3.7 The feeling of proof

I continue to draw attention to the phenomena of experienced proof, or at any rate one of the phenomena. It goes hand in hand with the unreasoned conviction that a proof carries with it. It may seem odd to speak of proofs being unreasoned, because proofs seem the very paragon of reason. What I mean is that we seem almost dazzled by the proof. What is impressive about the proof in *Meno* is not that it is rigorous, but that it is compelling. It may be said that this is just a fact of psychology. Say if you will that the philosophy of mathematics is prompted by something about the psychology of at least some human beings. It is the psychological effect upon we who are fascinated by the proof — that is not everyone! — which has helped mathematics infect the philosophy of some philosophers. Unfortunately perfunctory references to psychology do not help much. We know far too little about human psychology, cognitive or emotional, and should not pretend otherwise.

Here then is my first suggestion. Some and only some philosophers have been bowled over by the feeling that we see something directly when we grasp a proof. We can come to understand why certain facts are facts, facts that apparently anticipate facts about ordinary material objects. Then, by great leaps of imagination, philosophers such as Plato have used their interpretation or explanation of the phenomenon in some central parts of their philosophy, parts that have nothing intrinsically to do with mathematics, but which are more important to philosophy than mathematics, by itself, could possibly be.

4. *Leibniz*

Most mathematics is not like the proof in *Meno*, and not just because most of it is not so capable of being grasped as a whole. Kant distinguished arithmetic from geometry. With his sublime love of symmetry he associated the one with time and the other with space. There is a lot more to mathematics than arithmetic and geometry, we protest, even in the time of Kant. But in terms of the felt phenomena of mathematics, Kant's distinction was not so bad. It can be paired, very loosely, with two distinguishable modes of human cognition mentioned earlier, the visual and the combinatorial. It is a happy accident that geometry and algebra took root in different places in different ways, among different peoples. Of course there was a Greek study of numbers, but geometry was the model of the mathematical, and numbers were to a large extent understood as measures, as lengths. Arithmetic and algebra are of Indian and Arabic devising. That most combinatorial of concepts, the algorithm, is named after a tenth-century mathematician, whose name, when Europeanised, is Al Gorismi.

4.1 *Image and logic*

These two modes of cognition were not fully synthesised before the time of Descartes. They remain distinct. We describe some people as visual, while others are combinatorial. In new sciences we often see a battle between these two types of talent. For a longish time high-energy physics employed two groups of technologies, of instruments, data, and methods of data analysis. One involved scintillators and their statistical analysis, literally the combination of countless data points each of which meant nothing by itself. The other used photographs of tracks in bubble chambers, tracks whose properties could be literally observed, one by one. That is one facet of Peter Galison's monumental study of high-energy physics, *Image* *and Logic*.[13] The title does not say it all, for that takes Galison almost a thousand pages, but the message in the title is loud and clear. There were two competing methodologies, two competing casts of characters in the high-energy physics community of people and instruments. One was a world of images, the other of logic and combinatorial reasoning.

Another example is so familiar that we trivialise it. There were two competing modes of presentation of the operating systems of our word processors, the Mac and the PC. The former presented operations in a visual way, while the competing DOS system did so in a combinatorial way. That does not have much to do with what the computer does. It is a matter of how information is presented and activities are understood: point to an icon on a screen (visual) or type symbols on a keyboard (combinatorial). Perhaps the Mac's visual mode has won, even if the corporation that pioneered it may in the end be defeated. The Windows system is a caving in to Apple presentations, and is supposed to be a synthesis. A very poor one, many consumers would say. The contingencies of commerce and the skills of marketing have had more to do with the upshot than any matter of intrinsic reason. I make only this point: in the early days of personal computing (and we are not out of them yet) two ways of thinking came to the fore, spontaneously, without anyone planning things that way. And, adds the critic, the resulting synthesis is a mess. The philosopher might continue in this spirit by observing that the synthesis of Vieta, Descartes & co. was in the first instance a bit of a mess too, but happily it was not built into any material technology.

4.2 Proof as combinatorial

In the presentation of computational activity and information, image (to use Galison's labels) has superficially triumphed over logic. In the case of proof, however, it is the other way about. Twenty-five years ago I opened a Dawes Hicks lecture to the British

[13] Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: Chicago University Press, 1997).

Academy with the words, 'Leibniz knew what a proof is. Descartes did not.'[14] I said that because Leibniz had a combinatorial notion of proof, while Descartes did not. Leibniz had the right notion! Today I am less dogmatic about that. But what is this combinatorial notion implied in the work of Leibniz, and which he himself called combinatorial? We now define a proof in deductive logic as a sequence of sentences each of which is either an axiom or follows from an earlier sentence by one application of a rule of inference. This notion has almost no connection with that core feeling of proof that prompted the idea of a priori knowledge. Proof cast into logical form is proof anaesthetised, so that proof produces no feelings. G. H. Hardy is often quoted as saying 'proofs are what Littlewood and I call *gas*'.[15] Nitrous oxide, perhaps.

4.3 The unpuzzling character of calculation

Logicians make proof into a kind of calculation, to the point of insisting that the proof concept must be recursive. So let us turn to calculation, ordinary calculation. There is nothing perspicuous about calculation. There is no feeling of compulsion. There is no sense of understanding. I do not understand that, or why, $5+7 = 12$ (Kant's example), because I was trained at any early age to utter those words. So let us reach beyond memory. Divide 471 by 9. I can do this in my head. Nine into 47 goes 5 times, 2 to carry, and into 21 it goes twice, 3 to carry, so 471 divided by 9 is 52 and 3/9, or $52\frac{1}{3}$. I quickly check to see that is correct, by multiplying $52\frac{1}{3}$ by 9 in my head; yes, that's right, 471.

That is the result: 471 divided by 9 equals $52\frac{1}{3}$. *Must* it be the result? Do I see why this is the result? No, it simply is what happens when I follow rules that I had learned before I was six years old, and have used ever since. Does it anticipate experiment? If I have 471 marbles, and put them into bags of 9, I will have 52 bags with 3

[14] Ian Hacking, 'Leibniz and Descartes: Proof and Eternal Truths', *Proceedings of the British Academy*, 59 (1973). In A. Kenny (ed.), *Rationalism, Empiricism and Idealism: British Academy Lectures on the History of Philosophy* (Oxford: Oxford University Press, 1986), pp. 47–60.

[15] G. H. Hardy, 'Mathematical Proof', *Mind*, 38 (1929), 18.

marbles left over. Yes, this does anticipate the result of an experiment. If the experiment does not pan out as anticipated, however, that does not at once show that something has gone wrong, that I have mislaid a marble. The most plausible first guess, in the event of a mismatch between calculation and experiment, is that I divided wrongly. I had better do my sums again.

To put the multiplication in its place, compare it to solving a simple problem about the London Underground. In real life, only the truly adventurous try to travel from Morden, at the southern extremity of the Northern Line, to Cockfosters, north end of the Piccadilly Line. But we who have the tube map quickly work out one way to do it: change at the Elephant and Castle on to the Bakerloo Line, and then at Piccadilly Circus. I look at the familiar diagram and work out, by the usual rules, how to follow the lines to get from one place to another. Once again I anticipate experience. If the Underground is working properly I will indeed make the journey as predicted. This is exactly analogous to 471 divided by 9. If I try it out, and do not end up at Cockfosters, a first move is to check the tube map to see if I did not read it wrongly. There are of course more empirical possibilities. Marbles tend to behave when sorted, but alas the Underground does not have such a good track record. Nevertheless the question, 'Which route will get you there?' is quite distinct from 'Which is the quickest route?' The map does not indicate times, and indeed tells distance in only a rough and ready way. I could get to Cockfosters with only one change, at King's Cross, which gets me there just as surely, but, I hazard, more slowly, because the Northern Line is notorious for its delays. When I am less able-bodied I shall certainly choose the King's Cross route, but such empirical aspects do not enter into the calculation of a route to get to Cockfosters.

We do not distinguish a logically necessary proposition, about routing from Morden to Cockfosters, from an experimental proposition, about actually going by the underground railway. In contrast there has been a big point, in analytic philosophy, of distinguishing the empirical proposition, what happens if you move the marbles around, from the arithmetical proposition, a necessary truth. Yet the situations are analogous. The Underground calculation would never prompt anyone to think of a priori knowledge or necessary truth. Nor would the arithmetical calculation either, if it were not embedded in a larger framework of thinking about mathematics.

4.4 Calculation and certainty

Calculation has, however, a virtue altogether different from the insight of the theorem imparted by Socrates' talk and his squiggles on the sand. In the case of a calculation you get practical certainty, it seems, because each individual step can be checked. A chain, they say, is only as strong as its weakest link, but if every step in the chain of deductions is correct, then the chain is as strong as the strongest link, the link that is automatically correct. Russell describes his own fascination with mathematics as trying to impose certainty, rigour, on the proofs that were provided slipshod by his tutor or textbooks of mathematics. As mathematicians slowly began to realise that even the most elegant and graspable of proofs could take us in and fool us, new demands for rigour were abroad. But that is not where the fascination with combinatorial proof begins.

Leibniz was a member of the first European generation that could imagine that proof was essentially combinatorial, a matter of checking the manipulation of signs. He had the optimistic conjecture that in logic we would be able to do away with axioms. The starting-points for proofs would be statements of identity, not real statements at all, not worth calling axioms (which have some content), but just identities. Proof would proceed by rules of inference for manipulating signs. Mathematics would live in syntax alone. This idea is amazing. It is counter-intuitive in two distinct senses. It runs counter to every expectation (or untutored intuition). It also discounts intuition itself, including the *intuitus* that was at the core of Cartesian theory of mathematical reasoning.

4.5 Proof as syntactical

Can anything be said for Leibniz's fascination with syntax? Anachronistically we can think of systems of natural deduction or Gentzen's sequent calculus as beginning only with statements of identity (*A* follows from *A*). One can even argue that there is a 'do-it-yourself semantics' for such calculi, that is, each rule of inference introducing a single logical operator determines unique semantics for that operator.[16] This may be worth the mention, if only as an aside, because Leibnizian approaches to proof are now often dismissed because they pay so little attention to semantics. In fact it was Leibniz who formulated what in retrospect we can see as the first completeness conjecture, that is, the first conjecture that states that a semantic notion, truth, can be captured by a syntactic notion, provability. For he thought that truth-in-all-possible-worlds would coincide with syntactic provability. Those, however, are matters of some nicety which take us away from the raw phenomena of proof.

Leibniz thought that through the syntactic idea of proof he could explain the idea of necessity, logical necessity. I remarked how in the simplest felt sense of 'logical' compulsion, one could get this idea even by reflecting on squares and incommensurables. But in the case of necessity theology takes historical precedent over mathematics. Are there some propositions such that even God could not create a world in which they were false? Unlike Descartes, Leibniz was of the majority party that maintained there are some propositions that even an omnipotent being cannot falsify. Hence there is a problem of reconciling necessity with omnipotence. Leibniz's theory of proof solved that problem, for him. Every logically necessary proposition is an implicit identity, and hence (hence?) constrains nothing, neither man nor God. That assertion is totally counter to the feel of things. This is no greatest prime number: how could one think that this statement is at heart an identity statement? Only a philosopher, a Leibniz, or a Wittgenstein writing the *Tractatus*, could cleave to such an idea.

4.6 Infinite proof and the nature of truth

Most philosophers tempted to Leibniz's way of thinking apply it only to logical necessity. Their general philosophy, if they have one,

[16] Ian Hacking, 'Do it yourself Semantics for Classical Sequent Calculi, including the Ramified Theory of Types', *Logic, Foundations of Mathematics and Computability Theory*, ed. R. E. Butts and J. Hintikka (Dordrecht & Boston, 1977), pp. 371–90.

is little infected by a theory about mathematics. Leibniz was something else. He was overwhelmed by his sense of proof as a sequence of steps each connected to its predecessor by a single combinatorial transformation of signs. He transferred his speculations about provability and logical truth to every truth whatsoever. 'In every true proposition,' he wrote, 'the predicate is contained in the subject, or I know not what truth is.'[17] The proofs of logically necessary propositions would be finite. Leibniz envisaged infinite proofs, proofs still springing out of identities, and still consisting of combinatorial steps, but with no finite number of steps, and converging on the proposition proved. His model was the theory of limits and convergent sequences to which he himself contributed so much.[18] His idea was that human beings can, with endurance, survey a finite series or a finite proof. Only God can survey an infinite proof. But surveyability is irrelevant to the nature of proof and truth; it is in the connection of sentences that the truth consists.

This is an amazing congeries of ideas. Once again the infection of philosophy by mathematics yields bizarre results. Leibniz's claim that in every true proposition the predicate is contained in the subject (together with its arcane gloss in terms of infinite proof) is the most absurd theory of truth that has ever been advanced.[19] It is fascinating to a certain type of mind — mine for example — but quite unthinkable, today, as a serious project.

5. *The terms of art*

I have drawn attention to three philosophers. The first was quite unmoved by mathematics, and discussed it only to refute a dangerous analogy fostered by other philosophers. The second and third allowed their view of mathematics to infect their philosophy. I have

[17] Leibniz writing to Arnauld, 14 July 1686, in G. Gerhardt (ed.), *Die Philosophische Schriften von G. W. Leibniz* (Berlin, 1890), II, p. 56.
[18] For an anachronistic model, see my 'Infinite Analysis', *Studia Leibniziana*, 6 (1974), 126–30.
[19] Those who have attempted to advance it may have earned the right to call it absurd. Ian Hacking, 'A Leibnizian Theory of Truth', *Philosophical Papers*, 12 (1975), 268–81.

wanted to strip back the layers of readings of Plato and Leibniz, in order to attend to what most impressed each of them about mathematics. I did so because similar things impress most philosophers who experience mathematics, although their accounts of what they experience are more sedate than those of Leibniz or Plato.

The experience of mathematics has given rise to several technical terms that in recent years have often been thoughtlessly run together. Each points to a different phenomenological aspect of mathematics. The terms of art are time-worn, but we cannot easily abandon them and their baggage. They will be waiting for us at the end of the line, no matter what we do. Philosophical history is interwoven with the distinctions a priori/a posteriori, necessary/contingent, and analytic/synthetic. A priori is often twinned with empirical rather than a posteriori. Two other pairs are less prominent in our day, but have often played centre stage: certain/not certain, and conceivable/inconceivable.

There is at most a loose consensus about the use of these many terms. Of the first three pairs, I attach most importance to 'a priori', and least to 'analytic'. This is because 'a priori' is the closest of the three to that astonishing phenomenon, that we can find out facts, apparently about the real world, just by reasoning. Thus 'a priori' recalls the experience of thinking mathematically. 'Necessary' points to the conviction that some propositions are true no matter what, and can be seen to be such. 'Analytic', in contrast, serves in explanatory theories about mathematics. Kant misleads us into thinking of 'synthetic' and 'a priori' as simply orthogonal terms that serve to identify a problematic class of propositions, those that are synthetic a priori. 'A priori' is phenomenological, while 'analytic' is speculative. 'A priori' and 'necessary' are names for two problems in the philosophy of mathematics and theory of knowledge. 'Analytic' is the name of a possible solution. Let us attend to each in more detail.

5.1 A priori

The original scholastic meaning of this expression has fallen almost entirely into desuetude. Reasoning a priori meant reasoning from cause to effect or from principle to consequence. Reasoning a posteriori meant reasoning from known consequences or effects to inferred causes or principles. There is a close parallel between the ideas of analysis and synthesis used in antiquity for describing methods of proof and inquiry. Analysis was a priori (in the scholastic sense of the word); both were top–down. Synthesis was a posteriori; both were bottom–up. The primary use of the distinction was to distinguish not knowledge but ways of reasoning.

Leibniz, who in most matters was both reactionary and visionary, characteristically marked the transition in usage. He employed the expressions in the scholastic way and in the modern one.[20] Here is Leibniz speaking in the old-fashioned way, even late in his life:

> A reason is a known truth whose connection with some less well-known truth leads us to give our assent to the latter. But it is called a 'reason', especially and *par excellence*, if it is the cause not only of our judgement but also of the truth itself — which makes it what is known as an '*a priori reason*'.[21]

Leibniz is modern when he uses 'a priori' as a direct contrary of 'known by experience'. He speaks, for example, of God knowing something about Alexander the Great, a fact that we can know only from an empirical history book, '*a priori* and not by experience'.[22]

Kant made this second usage definitive. Knowledge is a priori when it is independent of experience. We can (we feel) find out some things in the armchair, sitting and thinking, with pencil and paper, without recourse to looking at or experimenting on the objects of our inquiry. It is widely supposed that if a fact *can* be known a priori, then it can *only* be known a priori. That is a mistake. There are many trivial and a few interesting examples of a priori knowledge established by a posteriori reasoning. First a trivial example

[20] I here follow A. Lalande, *Vocabulaire technique et critique de la philosophie* (8th edn, Paris: Presses Unversitaires de France, 1960).
[21] *New Essays on Human Understanding* IV XVII 3; in the translation of Peter Remnant and Jonathan Bennett (Cambridge: Cambridge University Press, 1981), p. 476. Lalande cited this as an example of the scholastic usage.
[22] In the so-called *Discourse on Metaphysics* of 1686, §8; *Philosophische Schriften* VII, p. 433. Translated in L. Loemker, trans. and ed., *Leibniz: Philosophical Papers and Letters* (2nd edn, Dordrecht: Reidel, 1969).

adapted from C. D. Broad. Although we know a priori that p implies p, one could deduce facts of this form from observations. For example, after reading this paper and some other work, you conclude that if Hacking refers to Leibniz (L) he will also discuss mathematics (M). L implies M. You also conclude that if he discusses mathematics he will also refer to Leibniz. M implies L. From these two observations you deduce that L implies L.

More interestingly, the Belgian physicist J. A. F. Plateau (1801–83) was able to solve problems in the calculus of variations by experiment before anyone could solve them by mathematics. In fact some could not be solved in any generality until half a century after his death. What is the curve of least area bounded by a given curve in space? Answer: form a wire into the required shape, dip it into a soap solution and observe the film that forms. Plateau determined, experimentally, the solution to this problem for many canonical curves in space. This anecdote about proof and experiments gains zest from the fact that Plateau had blinded himself by observing the midday sun for twenty seconds, in an experiment intended to study after-images; his notable work on surface tension, capillary action, and the like contains the results of 'hundreds of novel experiments that he saw only with others' eyes'.[23]

There is a theoretical way of saving the doctrine that facts that can be known a priori can only be established a priori. One would argue that the facts that Plateau discovered are not identical with the facts subsequently demonstrated in the calculus of variations. He discovered only an empirical generalisation, expressed by the very same sentence that later came to express a demonstrable mathematical proposition. This fits well with the doctrine that was perhaps not perfectly articulated until a classic pair of papers by C. G. Hempel.[24] We have to distinguish (he said) the arithmetical proposition that $2+2=4$ from a proposition of applied arithmetic that applies to sets of things; likewise we must distin-

[23] *Encyclopaedia Britannica* (11th edn, 1911), vol. 21, 804b.
[24] C. G. Hempel, 'On the Nature of Mathematical Truth', in *Readings in Philosophical Analysis*, ed. H. Feigl and W. Sellars (New York: Appleton Century Croft, 1949), pp. 222–37. 'Geometry and Empirical Science', *ibid.*, pp. 238–49. (Originals in *American Mathematical Monthly*, 52 [1945].)

guish propositions in geometry (or the calculus of variations) from propositions about the material objects that as a matter of fact satisfy the axioms of this or that calculus. I do not object to Hempel's way of talking on principle, but he is making a distinction founded on theory, not on the basis of our actual pre-theoretical usage of the words with which we express mathematical thoughts. In fact I like Hempel's regimentation of language, for then it seems to follow that many strictly mathematical sentences, or sentences with strictly mathematical meanings, come into being only in the course of being proven. That seems totally contrary to common sense and experience, but I shall briefly return to it in Sections 7 and 8.

There are less delicate questions. Any brief (and perhaps any lengthy) definition of the required sense of a priori knowledge is open to the query, what is independent of experience? Is work with pencil and paper not 'experience'? There is an immense amount of trial and error, properly called experimentation, in trying out a mathematical conjecture. Despite the difficulties of giving an iron-clad definition or explanation that answers this query, we have no difficulty in recognising the intended meaning. I shall leave it at that, with no illusions that our meaning of the expression 'a priori' is either clear or distinct. Despite the term 'a priori' having a scholastic history with roots in the theory of causality, it can today serve to refer to some of the phenomena of some mathematical reasoning.

5.2 Necessary

Then there is the idea that some truths not only are true but must be true; they could not be otherwise. This assumed especial importance for Christian philosophers, who contemplated an omnipotent God. Where Plato wondered how on earth we could by pure reason find out facts that bear on how things are in the experienced world, the Christians wondered why an omnipotent God was unable to make a world in which a square had five sides or in which $2+2=5$. Few were as hardy as Descartes, who urged that God could do so, but has chosen not to.

This idea of necessity is also fairly close to the experience of some mathematical truths. There are evident connections with aprioricity. In his *Prolegomena* Kant even argued that 'all strictly mathematical judgements are *a priori*, and not empirical, because they carry with them necessity, which cannot be obtained from experience'.[25] One can certainly argue the reverse way. If the idea of a priori knowledge is accepted, then it appears that all propositions known a priori will be necessarily true. If a proposition can be known a priori, then no imaginable experience could confute it. Hence, goes this chain of thought, if the proposition can be known a priori, there is no possible state of affairs in which it could be false — and so it is necessarily true.

But, contrary to Kant, the converse does not seem to hold. First, there certainly seem to be necessary truths that we shall never in fact know to be true, and hence never know a priori. We need not think of difficult unsolved problems here. Write down three big numbers, *a*, *b*, *c*, and then tear up your piece of paper before you have memorised them or calculated with them. No one will ever in fact know what *a* raised to the power of *b*, raised to the power of *c*, is. Yet on a standard view of necessity, there is a necessarily true result of this exponentiation.

Secondly, there might be necessarily true propositions that are interesting, and that we do know, but know only a posteriori. This was the status of many of Plateau's discoveries in what we now call the calculus of variations. Some of his knowledge, obtained experimentally at the outset of the nineteenth century, was proven mathematically only in the mid-twentieth century. One would have to defend Kant's claim that necessity implies aprioricity, on the ground that Plateau did not *really* know what he found out by his experiments, but that seems to me a shabby defence of Kant's dictum. More recently, Saul Kripke's theory of rigid designation and identity implies that some identity statements, which express necessary identities, can in fact only be known a posteriori.

We owe our most graphic picture of necessity to Leibniz. He

[25] Kant, *Prolegomena to Any Future Metaphysics* [269]; tr. Lewis White Beck (New York: Liberal Arts Press, 1950), p. 16.

attached great weight to the scholastic notion of a possible world, a world that God was able to create. He took this quite literally; it was not, for him, a mere picture or way of talking. A necessary proposition is one that is true in all possible worlds. This is not strictly a definition, for possibility and necessity are equivalent notions. But it provides, for many modal logicians, an attractive way of thinking about necessity, and for presenting, in a quasi-visual way, the differences between different types of necessity distinguished in modal logic.

For all the intricate theories that it has prompted, the idea of necessity still serves to point to the surprising experience we have that certain things could not possibly be false. Of course we do not have that experience for what Locke felicitously named trifling propositions, manifest identities, and truths by definition, items that can scarcely be thought of as expressing real knowledge at all.[26] Those are what Mill named 'propositions merely verbal'.[27] The propositions that we feel to be necessary are those that are not obviously true but for which we have compelling proofs. Only after a long inculcation of the idea of necessity does one press on to the result of theoretical reflection, that anything true in virtue of definition, logic or mathematics is necessarily true.

5.3 Analytic

The history of ideas of analysis and synthesis in mathematics is one of the more vexing issues in the history of mathematical method. There was the idea that there was a Greek method, analysis, which had been lost, just like so many ancient texts. There was the idea that there are two kinds of proofs, or at any rate methods of proof discovery, analytic and synthetic. These thoughts surged throughout the period of the new learning and ended only at the time of Leibniz. He agreed that there are significant practical differences between different proof procedures, but that they are of little theoretical importance. If you are trying to discover something,

[26] John Locke, *An Essay Concerning Human Understanding*, Bk IV, chap. VII.
[27] Mill, *A System of Logic* I VI; *Collected Works* VII, pp. 109–17.

you may begin devising a proof from first principles, or you can work back from the result that you want to prove.[28] And as for pedagogy, a proof by analysis may more certainly be free of error, but a proof by synthesis may better explain to the pupil the intuitive connection of ideas.

Leibniz did not use the actual adjective 'analytic' in any modern sense.[29] Nevertheless Kant's use of the term 'analytic' is derived from Leibniz's theory of analysis. Leibniz well understood those old ideas of analysis and synthesis, in so far as they could be well understood, but once again he gave prominence to a new idea. He took the existence of necessary truths to be a phenomenon worthy of philosophical explanation. He had a theory that every necessary proposition can be proven in a finite number of steps from identities and definitions. Such a proof would be an analysis. Kant coined the word 'analytic' to refer to propositions susceptible of such proofs. For Leibniz this was a theoretical, or speculative, notion; Kant did not emphasise just how theoretical a notion it is.

There is a minimal way of developing Leibniz's idea. It says, before we add any qualifications, that provability in a finite number of steps coincides with truth in all possible worlds. That amounts to a conjecture about completeness. The conjecture is indeed true for the most important case. First-order logic is complete in exactly the sense that best fits Leibniz's own groping descriptions. So in that respect the 'analytic programme', as we may call it, was a quite extraordinary success. Gödel wrote that the explication of recursiveness (accompanied by Church's thesis that every calculable function is recursive) was the first really successful piece of epistemological analysis. I would offer Gödel's own proof of the completeness of first-order logic as a rival, fulfilling one strand in Leibniz's vision of necessity. This aspect of Leibniz's programme has tended to be overlooked because (as in all things to which he

[28] Leibniz, 'Synthesi et analysi universali seu arte inveniendi et judicare', *Philosophische Schriften* VII; pp. 292–98. Tr. L. Loemker, 'On Universal Synthesis and Analysis, or the Art of Discovery and Judgement', in *Leibniz: Philosophical Papers and Letters* (Chicago: Chicago University Press, 1950).

[29] Hide Ishiguro, *Leibniz's Philosophy of Logic and Language* (London: Duckworth, 1972), pp. 120 f.

turned his mind) he had far greater ambitions. He wanted to show that all the necessary truths of mathematics are analytic, i.e. provable from identities by substitution of definitions.

Kant denied that the necessary truths of geometry and arithmetic can be so proven. So he offered a rival theory about the possibility of necessary truth and a priori knowledge. On my understanding of Leibniz (allowing for the fact that he did not himself use the word 'analytic' in the sense later derived from his own ideas), the assertion that all necessary truths are analytic *solves* a problem, namely, what are necessary truths and why are they necessary? For Kant, the assertion that the truths of geometry and arithmetic are synthetic *poses* a problem, namely, how can they be a priori and yet not analytic? Kant's solution is given in the Transcendental Aesthetic.

The analytic programme was brilliantly revived by Frege, who thought that Kant was on the right course about geometry, but that Leibniz was on the right course about arithmetic. Leibniz argued that an analytic proof began with identities and allowed substitutions by definition. We modify what we are allowed to begin with, namely axioms and rules of first-order logic (which can in a very natural sense be construed as identities and definitional rules).[30] The analytic programme came to be called logicism, the project of reducing mathematics, but first of all arithmetic, to logic by means of definitions. Wittgenstein's *Tractatus* gave an exceptional twist to the idea, urging that the propositions of logic are tautologies and that the propositions of mathematics are assertions of identity. Hence both are content-free by-products of a notation, or, more ambitiously, are the resultant of the very possibility of certain types of language.

The analytic programme fell on hard times. There was never a satisfactory extension of Wittgenstein's 'by-product' idea of tautology even to the first-order quantifiers. Gödel's first incompleteness theorem showed that logicism could not work even for arithmetic, which had been Frege's first target. And Quine made his celebrated

[30] I elaborate this idea of logicism in my 'What Is Logic?', *Journal of Philosophy*, 86 (1979), 285–319.

onslaught on the analytic/synthetic distinction. It included, as part of a thorough attack, a denial that there was a clear definition of the logical constants, even though we do of course agree on which constants form a list.

The analytic programme had a brief but simplistic flourishing during the heyday of the Vienna Circle. This was largely due to a lavish generalisation of Wittgenstein's results about truth-functional tautologies. According to A. J. Ayer's wonderful vulgarisation published in 1936, an analytic proposition is one whose 'validity depends solely on the definitions of the symbols it contains'.[31] He defined 'synthetic' to be interchangeable with 'empirical', or, in his words, 'determined by the facts of experience'. Those ancient terms of art, analytic, necessary, a priori, were blended together to form a homogeneous syrup. The analytic programme had reached its delightfully innocent intellectual nadir when Ayer explained a priori knowledge in terms of his notion of analyticity. '[O]ur knowledge that every oculist is an eye-doctor depends on the fact that the symbol "eye-doctor" is synonymous with "oculist." And the same holds good for every other *a priori* truth.'[32]

Ayer had reduced the idea of analyticity to Locke's idea of trifling propositions, or what Mill called propositions merely verbal. To complete the confusion J. J. Katz begins an encyclopaedic dictionary entry for *analyticity* by saying that 'the true story of analyticity is surprising in many ways'.[33] He argues that the idea begins with Locke on trifling propositions and is finally fully clarified by Quine, Putnam, and Katz. He gives a false history which does not mention Leibniz, and says that Kant got it wrong. What he means is that the history of what Katz thinks is the right idea of an analytic proposition is what Mill would called a merely verbal one, and what Locke called trifling. There is nothing intrinsically wrong with Katz's usage: since 'analytic' is a theoretical term, one may define it according to a theory that one holds to be correct. It is more instructive, however, to recall the rich history

[31] A. J. Ayer, *Language, Truth and Logic* (2nd edn, London: Gollancz, 1946), p. 78.
[32] *Ibid.*, p. 85.
[33] J. J. Katz, 'Analytic,' in J. Dancy and E. Sosa (eds), *A Companion to Epistemology* (Oxford: Blackwell, 1992) pp. 11–17, on p. 11.

of the idea of analyticity in philosophical logic, one that tends to have been forgotten ever since the heyday of logical positivism.

5.4 Inconceivable

Inconceivability does not play much of a role in recent philosophy. Mill denounced the prejudice '*that what is inconceivable is false*'.[34] His target in the deductive portion of *A System of Logic* was Whewell. Whewell had argued that the evidence for axioms in mathematics and rational mechanics lies in the inconceivability of their opposite. Mill's discussion is one of the finest hatchet jobs in the entire history of philosophical analysis, but it did not convince everyone. Herbert Spencer most vigilantly defended the doctrine of inconceivability, prompting a long chapter of rebuttals in a later edition of Mill's *System*.[35] But the most extraordinary discussions of inconceivability occur later in the *System*, where we read: 'I am indeed disposed to think that the fallacy under consideration has been the cause of two-thirds of the bad philosophy, and especially of the bad metaphysics, which the human mind has never ceased to produce'.[36] Leibniz is deemed to be the worst offender, followed by Spinoza and Descartes.

There are two distinct issues. First: is inconceivability a reliable guide to falsehood and hence to truth? Second: is inconceivability a good name for a certain phenomenon associated with proof or certain types of truth? The answer to the first question is 'No'. Mill established that, as definitively as one can establish anything by philosophical analysis. As for the second question, inconceivability is more associated with perspicuous proof than with truth. Not-*p* is inconceivable only after we have understood a proof of *p*. And then it is a secondary name for a primary phenomenon, the experience of being compelled by a proof.

34 Mill, *System* V. III. 3; *Collected Works* VIII, p. 750. In a section heading, and thus original italics.

35 Herbert Spencer, *Principles of Psychology* (London: Longman, Brown, Green, and Longmans, 1855). Mill, *System* II. VII. i–iii. Spencer, *Principles* (2nd edn, London: Williams and Norgate, 1870). Mill, *System*, II. VIII. iv.

36 *System* V. III. 3; *Collected Works* VIII, p. 752.

5.5 Certainty

Certainty has had a far more pervasive career in philosophy than inconceivability, and very often mathematics has been seen either as the apogee, or as the model, for knowledge that is certain. Of course it is not mathematical truths that are certain, but proven theorems of mathematics. Whenever we reflect on these matters we are driven away from propositions to reasons, from truths to their proofs. Kant referred to 'the apodictic certainty of all geometrical propositions'.[37] If he had taken that word 'apodictic' seriously, we might have had a different Kantian philosophy, for 'apodictic' is derived from the Greek for 'demonstration', and means established from incontrovertible evidence. Geometrical propositions are apodictic only when established, proven. More on 'apodictic' below.

If certainty was once the model for knowledge, probability and fallibility have increasingly taken control of Western thought. They have been encroaching ever since the Napoleonic era, and now make it difficult, for many of us, even to understand the lure of certainty. Why would absolute certainty be desirable? Well, we want to be absolutely certain that genetically modified foods are not harmful to human life and well-being. Yes, but that means, beyond all (real) doubt, which is still short of mathematical certainty.

Philosophers in most ages have, however, been deeply impressed by the certainty that results from some mathematical reasoning. Some proofs really are experienced as certain, but most are not. Only dogma or theory has made people say that mathematics as a whole has a peculiar certainty. The dogma has made it possible for there to be 'foundations crises' in mathematics. When things go wrong with some central part of condensed-matter physics, a radical counter-example to theories of superconductivity, say, that is news. There will be exciting times ahead for a small but critical part of physics. But when things go wrong with mathematics, that is a crisis, because if a result undoes a previous certainty, mathematics itself seems threatened. In *The Structure of Scientific Revolutions* T. S. Kuhn picked up the word 'crisis' as in

[37] Kant, *Critique* A 24, Kemp Smith, p. 68.

'foundations crisis' and made it an essential part of his dialectic of scientific revolutions (normal science, anomaly, crisis, revolution, new normal science). But his crises were localised to subdisciplines, whereas a foundations crisis was supposed to be just that, a crisis in foundations that might collapse, leaving everything in ruins.

Perhaps the discovery of incommensurability produced, as folklore has it, a crisis in ancient Greek mathematics. There were early nineteenth-century crises in analysis, causing Cauchy to demand entirely new standards of rigour. We are better acquainted with the foundations crisis that Russell and others provoked at the start of the twentieth century. Such crises moved only a small proportion of working mathematicians, because problems in one area are seldom contagious. But for the logicians and philosophers, there is just one thing, mathematics. Mathematics must be certain, secure, and whole, and if there is a flaw anywhere, it is everywhere. And so there is a crisis.

5.6 Apodictic

The word 'apodictic' is not in general use. As noted above, it is derived from the Greek for demonstration. It also has a good English pedigree, as may be confirmed by consulting the entries for the word and its cognates in the *OED*. Most readers will know the word only from Kant, but it was current in philosophical logic in Kant's day. In British usage, Thomas Reid is cited in 1788, the year before the first *Critique* was published in German; he wrote that 'when premises are certain, and the conclusions drawn from them are in due form, the syllogism is called apodictical'. Despite the phrase 'apodictic certainty' quoted above, Kant preferred to speak of apodictic propositions, principles or judgements. I like to take his usage more literally than he intended. 'For geometrical propositions are one and all apodictic, that is, are bound up with a consciousness of their necessity.'[38] No: geometrical propositions

[38] '*Denn die geometrischen Sätze sind insgesamt apodicktisch*', *ibid.*, B 41, Kemp Smith, p. 70. 'Apodictic principles' (*apodiktischer Grundsätze*) A 31/B 47, Kemp Smith, p. 75. The general theory about the problematic, the assertoric, and the apodictic is introduced in a footnote to A 75/B 100, Kemp Smith, p. 110.

in general are not bound up with a consciousness of their necessity — at most when they are proven or taken as basic maxims. By etymology we ought to reserve apodictic for what is proven, perhaps perspicuously proven. Then we might well say that apodictic judgements are bound up with a consciousness of their necessity — in virtue of the fact that they *are* apodictic judgements, proven.

There is no point in pining for lost opportunities of usage. 'Apodictic' is not current English, and restoring it would do little good. Moreover there can be much play, which I have not acknowledged, with the idea of being proven, for things may be incontrovertibly proven from incontrovertible but empirical facts of history. Take the charming *OED* citation of De Quincy (1832–34): 'There were no roasted potatoes in Spain at that date [1608], which can be apodictically proved, because in Spain there were no potatoes at all.'

5.7 Theory and phenomena

To sum up: for some time there arose, in analytic philosophy, a tendency to use terms such as 'a priori', 'necessary', and 'analytic' as almost interchangeable. Analytic philosophers are separators, not homogenisers. Hence it is unusual for them to treat different terms of art as if they were virtual synonyms. The reason for treating these three as equivalent was the temporary success of the analytic programme. Later, the very demise of the programme made all three seem equally empty.

A rough-and-ready equation was taken for granted, namely a priori = necessary = analytic. Thus in virtue of theory there appeared to be one distinction, rather than three. Quine's celebrated attack on the analytic/synthetic distinction was widely construed as demolishing this three-in-one all at once. The theory was demolished by a conjuncture of Gödel's mathematics, Quine's analysis, and numerous other events in logic and philosophy. But what about the terms that prompted the theory in the first place? 'A priori' and 'necessary' remain with us as indicators of two different sources of philosophical difficulty. They are abbreviations for questions. 'A priori' calls to mind, 'How come we can find out some things just by thinking?' 'Necessary' calls to mind, 'Why is it that some things must be true, no matter how the world is?'

Let us now return to the declared task, of reflecting on what mathematics has done to some philosophers. My first group has been of philosophers enormously impressed with proof, and with the correlative phenomena of necessary truth and a priori knowledge (although in the trio I include Mill, who thought his mission was to debunk the philosophies that result from taking such things seriously). I shall now pass to a second group of philosophers who have decidedly non-standard attitudes to proof: Descartes, Lakatos, and Wittgenstein. These too have been deeply moved by mathematics. My first group was inflationary, and I did no more than take their extraordinary statements at face value. There was no call for interpreting. My second group is deflationary, with the consequence that their statements about mathematics itself sound absurd to mathematicians. Hence in the case of my second trio I have to venture something more like interpretation, to suggest what the author may have been getting at, or what was driving him down the path to 'absurdity'.

6. *Descartes*

Descartes looks like a sort of divine constructivist about mathematics. He can be read as having held that only thanks to a divine decision does $2+2$ make 4, and not 5. God could have made things differently, five-sided squares perhaps. That aspect of Descartes's philosophy has been discussed a great deal (even in my own previous Dawes Hicks Lecture), and I shall leave it to one side. Instead let us briefly examine his views about reasoning rather than truth.

Stephen Gaukroger argues that the Cartesian doctrine of *intuitus* is a reaction not against Aristotelian thought but against some late scholastic logic,[39] in particular, against the ideas of Spanish Jesuit philosophers whose textbooks were used at La

[39] Stephen Gaukroger, *Descartes' Conception of Inference* (Oxford: Clarendon Press, 1989).

Flèche. They thought that reasoning is done by a mental faculty, one of several alongside the faculties of memory and of imagination. They were the cognitive scientists of the day, describing the modules by which the mind fulfils its functions. The power of Chomsky's *Cartesian Linguistics* made many people take for granted that Descartes was the beginning of cognitive science. Now of course one fact about truly great philosophers is that they can be read in many ways. I do not want to take issue with Chomsky's uses of certain texts, but only to redirect attention. According to Gaukroger's reading, Descartes was fiercely *opposed* to the Spanish scholars who really did anticipate cognitive science.

The Spanish scholastics held that logic was a normative theory about the right working of the reasoning module. In pedagogy, they brought logic and rhetoric together with this conception, and propounded rules for right reason, to which any reasoning must answer in order to be judged valid, and which students must use to verify their inferences. Descartes rebelled because he was a mathematician (and not a teacher). The logic teacher wrote manuals for their pupils. Descartes's *Rules* and his *Discourse on Method* are manuals addressed to the human mind, yours and mine, we who are not pupils but equals of Descartes. He fathered a canonical move characteristic of constructivism. *There are no normative standards to which reason can answer.* This is not to forbid any regress whatsoever. Often in reasoning we seek better, or earlier, reasons. But we must not demand a justification for reason itself. I call this a little-regress position (to call it 'no-regress' would be to suggest that once a reason is given, no further reason can be asked for). Reason is its own self-authenticating guarantor. But it is self-authenticating only if completely purified, completely stripped even of steps, so that you can see an entire proof at once — precisely what Socrates advises the slave-boy to do.

Readers of Descartes may protest that there is something else that authenticates reason, namely the good God who from His bounty would not trick us. An obsession with justification has made it hard for us to understand Descartes and his God. God is not there in the role of justifier, but in the role of absence of justification, as background within which reason makes sense at all.

The role of the Cartesian God corresponds, abstractly speaking, to those elements in Wittgenstein's *Remarks on the Foundations of Mathematics* about what is prior to justification, within which all discourse makes sense. This part of Cartesian theology plays exactly the role of the 'anthropology' or 'natural history of human beings'.

This will seem an odd comparison. It has been standard at least since Peirce to take Descartes as something that the modern mind, with its transition from mental discourse to public language, has overthrown. The most common contrast, well exemplified by Rorty's *Philosophy and the Mirror of Nature*, is between Wittgenstein and Descartes. We have the switch from private to public (the fundamental transition in Western philosophy, and which Chomsky himself tries to ignore). So of course Wittgenstein is no Descartes. That said, I find remarkable parallels not only in their philosophies of mathematics but also in philosophical psychology.[40] God would not deceive us about mathematics, for He is a good God, but He does not guarantee mathematics either. Mathematical reasoning stands on its own, ungrounded, unfounded; to engage in it carefully is to be assured of its correctness, and there is nothing else to resort to. There is no foundation for mathematics.

7. *Lakatos*

One of Imre Lakatos's triumphs was to show how proofs are historical events, constantly revised in the light of counter-examples. In 1956, when Lakatos arrived in England to begin his research on the foundations of mathematics, philosophical logicians and students of foundations wrote as if mathematical reasoning meant deduction, plus, stretching things a bit, the construction of models in terms of which one would provide completeness proofs. This was far from the attitude of mathematicians. Lakatos's Hungarian masters, A. Renyi and Georg Polya (of *How to Solve it* fame) cast mathematical inquiry in an entirely different light. *Proofs*

[40] Cf. my 'Wittgenstein as psychologist', *The New York Review of Books*, 1 April 1982.

and Refutations is a splendid searchlight on some aspects of real-life mathematical reasoning, mathematics in action, where deduction is only part of a much larger mosaic.

In Section 5.1 I recalled Hempel's distinction between mathematical sentences and their empirical correlates, between $2+2=4$, understood as an arithmetical statement, and understood as a claim about combining things. I went so far as to suggest that if we took this seriously, we might have to think of the mathematical sentences — not so much the sequences of words but the sentences with new meanings, the sentences meant as necessary truths —coming into being as they are proved. That suggestion will never be taken up, it is so contrary to our common sense of what sentences mean. So it is fascinating to see in the course of Lakatos's presentation how new mathematical sentences (words never strung together before) do come into being before your eyes.

His dialogue is a piece of theatre, the mathematical-anatomy theatre. We start with a sentence of something like empirical mathematics, about the relation between the numbers of edges, sides, and vertices of a polyhedron. You check that one out by counting, although in that strange way in which one counts the vertices etc. on an example when one is using the example as a picture or model of all objects of that type. Lakatos liked to use the expression 'quasi-empirical' which is quite a nice way to describe some initial thinking. We can use a matchbox (not a cube!) to count the number of edges of a cube, because it serves as a picture of a cube.

Lakatos's group in the classroom begins to try proving the conjecture. We start with fairly plain language but as reasoning proceeds we come to neologisms. 'All polyhedra, all of whose circuits are bounded, are Eulerian.' 'All polyhedra in which circuits and bounding circuits coincide . . .', and on to a sentence that includes the explication of 'Eulerian': 'the number of dimensions of the 0-chain space minus etc equals 2'.[41]

Among the speakers in the dialogue are those who say that

[41] Imre Lakatos, *Conjectures and Refutations* (Cambridge: Cambridge University Press, 1970), pp. 114–16.

conventions are being made and those who deny it. The conventionalists are making a claim not only about new words, but about new conceptual connections. But at the more superficial level one thing is plain. There are new sentences, sentences that did not exist, until a certain point in the history of the proof, sentences that were unthought and could not have been thought without the proof idea. But what about truth conditions? The theorem, the sentence at the end of the proof, is in a proper sense of the word, if not 'analytic', 'analytified'. The proof, at least for the time being, provides the criteria in terms of which the proposition is true. This is a completely new twist in the analytic programme. Or so it seems, until we see start drawing similar suggestions out of Wittgenstein's *Remarks on the Foundations of Mathematics* (a work for which Lakatos had complete contempt).

Lakatos was never clear about the 'status' of the theorem whose development he described. As I said, he would use words like 'quasi-empirical' which seem to me to suit the beginning of the dialectic better than its end. He knew that theorems do not correspond to truths about physical objects, and equally that theorems are not a matter of convention. He resisted the idea of self-authentication, of the mutually reinforcing character of the reasoning and theorem proved. By the end of the dialogue the achieved proof really does establish the revised theorem as certain, but only because the meanings of the term used to express the theorem have been modified to fit the lemmas that have evolved in the proof's history. The proof and the ideas expressed in the theorem are, during the dialogue, plastic resources that are mutually moulded to produce the final product. That is precisely the dialectical character of what I call self-authentication or even self-vindication.[42] The final, sanitised proof is right because it leads to the analytified truth; conversely, the theorem is true because it is proven.

Logical positivism held that every mathematical truth is true in virtue of the meaning of the words used to express it. By a sudden

[42] These words are explained in my 'Style', cf. n. 8 above, and papers referred to therein.

and unexpected twist Lakatos's philosophy suddenly makes sense of that implausible claim. After the dialectic of conjecture and refutation that culminates in a proven theorem, the meanings of the words in the theorem have been so refined that indeed the theorem is true in virtue of what the words mean (when properly understood).

It will be noticed that this is a no-foundation view of mathematics. I must emphasise that I am putting a strong interpretation upon what I am calling Lakatos's philosophy. Like his predecessor Karl Popper he denounced, in general, the search for foundations of knowledge. But he did not own up to a complete no-foundation, little-regress, view like that of Descartes, Wittgenstein or even the logical positivists. An *ad hominem* speculation arises here. Perhaps Lakatos's repeated denunciations of 'justificationism' as an extinct stage in the history of philosophical thought reflect the fact that Lakatos himself was still a bit of a closet justificationist. He had not taken what I suggest was the Cartesian leap past justification.

8. *Wittgenstein*

My project has not been to defend or criticise any individual's philosophy of mathematics. I have wanted to examine the impact of mathematical experience on some philosophers. Wittgenstein was one philosopher who took mathematics very seriously. His view of logic and mathematics stated in the *Tractatus* had definitive, but perhaps not salutary, effects on both Bertrand Russell and the members of the Vienna Circle. Here, however, I am concerned with his later work.

Given the nature of my project, I do not have to state here whether Wittgenstein had a body of doctrine worth calling a philosophy of mathematics. I do not have to consider whether his repeated turns to questions about mathematics, from the late 1930s until his death in 1951, represent the evolution of a single coherent group of ideas. I do not have to opine on which parts, if any, of his recorded thoughts were false, negative, retrograde, or confused. Obviously I do have to interpret his words to the extent of exhibiting some of the aspects of mathematical reasoning that festered in his mind and kept him returning to these topics. But I do not intend to argue for a systematic interpretation of his work.

I should admit to four attitudes to Wittgenstein's texts that seem not to be very widely shared by my fellow philosophers. First, I find it natural to take Wittgenstein quite literally, although one must always pay attention to questions of voice — who is uttering which sentence in the course of his numbered paragraphs. Secondly, what he literally meant is in general the simplest construction to put on his actual words. (It may not always seem simple, because it may run counter to something we commonly take for granted.) Thirdly, I respect his repeated avowals that he wanted to describe and not to explain. Hence I seldom find it helpful to derive general and synthetic doctrines from his work, especially when he did not explicitly state them in so many words. A fourth attitude follows. Wittgenstein insisted that he did not want to change mathematics, although he did hope to remove confusion about what mathematicians and others said about mathematics. I adopt as a maxim, in what follows, that Wittgenstein never argued that any proposition deemed to be proved, was not proven, or that any mathematical proposition deemed to be true, was not true. He did certainly call in question what it meant to say that a proposition was true, or proven, but that is something else. Thus I take Wittgenstein not to be 'revisionist' about mathematics. But he may well encourage us to revise the ways in which we have traditionally talked about mathematics.

8.1 Not rule-following

I must begin by referring to an important topic that I am not able to discuss here. The *Remarks* were published in 1956. After 1975 one concern of Wittgenstein's was fastened upon above all others: following a rule. Several philosophers took it up relatively independently, but Saul Kripke is the one who left an indelible mark, starting with his lecture in London, Ontario, in 1976.[43] Earlier

[43] Saul Kripke, *Wittgenstein: On Rules and Private Language* (Cambridge, Mass.: Harvard University Press, 1982 [public lecture, 1976]).

readers — we might speak of the generation of 1956–75 — paid relatively little attention to rule-following when they examined Wittgenstein's reflections on mathematics. This is partly an artefact of editing. Only in the revised edition of the *Remarks* (1978) did the editors print material which they rightly say is 'perhaps the most satisfactory presentation of Wittgenstein's thoughts on the problem of following a rule'.[44] It is Part IV in the renumbering of the revised edition, which will be used consistently below.

This is not the place to discuss the debate that Kripke initiated. Quite aside from questions about the interpretation of Wittgenstein, the debate hinges on very general questions about language. They are not especially about mathematics, although Kripke began them with an arithmetical example. The interest in Kripke's reading is prompted by *Philosophical Investigations*, not by the *Remarks*, and not, in effect, by questions about mathematics. Some early readers of the *Remarks* who also read Nelson Goodman (admittedly not a large class) noticed that it looks as if you can make some closely related points using Goodman's observations about 'grue', which, of course, have nothing specific to do with mathematics.

The debate about following rules, subsequent to Kripke, has seldom hinged on any question peculiar to mathematics. The one exception is Crispin Wright's important book, published in 1980, written and motivated independently of Kripke's lecture.[45] Wright put 'the rule-following considerations' centre stage in his examination of Wittgenstein's *Remarks* (even before the new Part VI had been published). Wright's contribution, rather than Kripke's, might prompt me to examine rule-following here, but the secondary literature has grown so vast that it is best to try to pass it by for present purposes. One might, however, answer my question: 'What effect did mathematics have on Wittgenstein's philosophy?' in this way: it led him to reflect on following a rule, which then became pivotal in his entire philosophy.

[44] 'Editors' Preface to the Revised Edition', *Remarks*, p. 29. There were of course paragraphs about rule-following in the first edition, including the opening sections of Part I, which mimic the corresponding part of the *Philosophical Investigations*.
[45] Crispin Wright, *Wittgenstein on the Foundations of Mathematics* (Cambridge, Mass.: Harvard University Press, 1980).

P. M. S. Hacker, the leading exegete of Wittgenstein's texts, has strongly contested not only Kripke's reading of Wittgenstein, but also the philosophy advocated by Kripke (whether or not it be Wittgenstein's). David Bloor, author of what may be the earliest published 'sociological' approach to Wittgenstein (1973), calls the two main types of opinion about rule-following 'collectivist', represented by Kripke and Bloor himself, and 'individualist', represented by Colin McGinn.[46] Many collectivists appear to hold that the ultimate sanction for rules lies in social organisations and institutions, while some individualists appear to seek a foundation in the individual practices of speakers. I myself would prefer an approach to these issues that rejects any search for final sanctions. That would be a limited-regress position analogous to that of Descartes. If God were a deceiver, we would be in trouble with mathematics, but that does not make God the ultimate sanction for mathematical truth. If certain aspects of what Wittgenstein called the 'natural history of mankind' did not obtain, the entire notion of following rules (and also, much or all mathematics) would collapse, but that does not make natural history, anthropology, culture or institutions the ultimate sanctions.[47] A limited-regress philosopher like myself is not inclined to look for sanctions.

8.2 Not foundations

Very few of Wittgenstein's *Remarks* are directed at the foundations of mathematics, as that term has been generally understood in the

[46] David Bloor, *Wittgenstein: Rules and Institutions* (London: Routledge, 1997, p. xii); 'Wittgenstein and Mannheim on the Sociology of Mathematics', *Studies in the History and Philosophy of Science* vol. 4 (1973), pp. 173–91; and *Wittgenstein: A Social Theory of Knowledge* (London: Macmillan, 1983). Colin McGinn, *Wittgenstein on Meaning: An Interpretation and Evaluation* (Oxford: Blackwell, 1984).

[47] The relevant observations about *Naturgeschichte* ('our natural history', 'the natural history of mankind') in the *Remarks* are: I §63, p. 61 (in connection with proof); I §142, p. 92; VI §49, pp. 352–3 (in connection with 'the logical "must"'). There are also remarks about ethnography and anthropology. Then there are analogies of a *completely* different sort. Why not, e.g., think of arithmetic as like mineralogy, that is, as the natural history of numbers? No one says that, but 'Our whole thinking is penetrated with this idea' IV §11, p. 229.

twentieth century. When he does address what seem recognisably to be foundations, as generally understood, his comments have provoked hostility or scorn. He seems to have had iconoclastic opinions about Gödel's first incompleteness theorem. He upset many readers with his suggestion that it would not matter if there were inconsistencies at the heart of mathematics, so long as no one knew about them. In their first edition his editors by and large bracketed off these discussions from the rest of the material and I shall follow that example. Likewise I shall not refer to his occasional remarks on Cantor, Dedekind or intuitionism. I do not thereby imply that these remarks lie outside his overall approach. I suggest only that the aspects of mathematics that so spurred Wittgenstein to return over and over again to the subject were not specific results like the theory of transfinite numbers or Gödel's theorem. Hence those results are not highly relevant for answering my title question as it applies to Wittgenstein, 'What did mathematics do to Wittgenstein as philosopher?'

8.3 Logicism

There is no doubt that logicism, the analytic programme, and Wittgenstein's own earlier thinking, are strongly in the background of his *Remarks*. But it would not be right to address my title question by saying that logicism, and a withdrawal from it, deeply affected Wittgenstein's philosophy. This is because logicism is not a part of mathematics. It is a thesis about mathematics. If we are to ask what in mathematics affected Wittgenstein's philosophy, we have to go below logicism, to the features of mathematics that prompted logicism itself.

There are plenty of remarks about Russell (perhaps one should say a figure whom Wittgenstein calls 'Russell') interspersed throughout the text. The most common context seems to me fairly unproblematic. One of Russell's professed aims in working on *Principia Mathematica* was to make mathematics certain — to provide clear, firm, and consistent foundations in pure logic, which would suffice for proving any truth of mathematics. Once the proof of a truth had been cast in the standard form of a proof in *Principia*, then we could be certain of that truth.

Reading the *Remarks*, one comes to see at once that this project, understood in just that way, is a chimera. This is because the proof, written out in the form of an unabbreviated *Principia* proof, would be an uncheckable sequence of millions of symbols. Even if we used a mechanical checker to check the proof, we would need a non-*Principia* proof to be sure that the checker worked on sound principles. A theme that can be read here is this: You, 'Russell', think that you have augmented the certainty of mathematics. Not so, the certainty rests more on familiar features of ordinary proofs than on your vast construction.

Notice that this does not strictly deny that a long and effectively unintelligible proof in the formalism of *Principia Mathematica* is a proof. It says that this proof lacks the character of proofs that do convince us, that do produce certainty. Hence this proof does not produce the certainty that 'Russell' claimed. Here we have an example of my earlier assertion, that Wittgenstein did not claim that any proposition deemed to be proved, was not proven, or that any mathematical proposition deemed to be true, was not true. He does claim that the proof does not have the character that other proofs do have. And of course if it is understood as a 'proof of certainty', it is not that, but then that is not what was proved in the formalism of *Principia*.

8.4 First readings

Many people who came across the *Remarks* soon after it was published, even those who respected Wittgenstein or *Philosophical Investigations*, thought that the published fragments were pretty dreadful. For example Georg Kreisel, who had attended many of Wittgenstein's seminars, described the published *Remarks* as the disappointing products of a sparkling mind.[48] But other philosophers, starting with Alice Ambrose and Michael Dummett, publish-

[48] Georg Kreisel, 'Wittgenstein's *Remarks on the Foundations of Mathematics*', *British Journal for the Philosophy of Science*, 9 (1958), 136–9.

ing in 1959, took them very seriously indeed.[49] Readers of the *Remarks* before 1976 attended to what they often called a conventionalist strand in Wittgenstein's writing about mathematics — or to what was called his radical, or strict, finitism. There was the tantalising assertion that proofs somehow 'fixed concepts'. One noticed the importance of proofs being perspicuous or surveyable.

These early reactions and loci of interest were faithful to the published text. The most frequently occurring term in the first edition of the *Remarks* is not 'rule' but 'proof'. Aside from 'language-game', other key nouns are 'application' (*Anwendung* ranks very high in an analytical index), and 'calculation', 'experiment', 'inference', 'measure', and 'picture'. This is not to say that there are no 'rule-following considerations' in the first edition of the *Remarks*.[50] But although rules are often mentioned, it is seldom in that context. A more typical statement is: 'The effect of proof is, I believe, that we plunge into a new rule.'[51]

The ideas singled out for discussion in the pre-rule-following days were harrowing enough. Added to them were Wittgenstein's queries about whether contradiction in foundations would matter, and his apparent scepticism (or confusion) about Gödel's theorem. Wittgenstein was often cast as a veritable ogre, maliciously obscuring all that was clear about the nature of mathematics. Descartes and Wittgenstein appear as the great mavericks in the history of

[49] Alice Ambrose, 'Proof and Theorem Proved', *Mind*, 58 (1959). Hector-Neri Castaneda, 'On Mathematical Proofs and Meaning', *Mind*, 60 (1961). Charles Chihara, 'Wittgenstein and Logical Compulsion', *Analysis*, 20 (1960–61); 'Mathematical Discovery and Concept Formation', *Philosophical Review*, 68 (1963). Michael Dummett, 'Wittgenstein's Philosophy of Mathematics', *Philosophical Review*, 64 (1959). A. B. Levison, 'Wittgenstein and Logical Laws', *Philosophical Quarterly*, 14 (1964); 'Wittgenstein and Logical Necessity', *Inquiry*, 6 (1964). E. J. Nell, 'The Hardness of the Logical "Must"', *Analysis*, 20 (1960–61). Aaron Sloman, 'Explaining Logical Necessity', *Proceedings of the Aristotelian Society*, 68 (1968–69). Barry Stroud, 'Wittgenstein and Logical Necessity', *Philosophical Review*, 70 (1965). O. P. Wood, 'On Being Forced to a Conclusion', *Proceedings of the Aristotelian Society*, suppl. vol. (1961).

[50] In the numeration of the revised edition, we have I §§1–3, pp. 35 f; I §§113–18, pp. 79–82; IV §§8–9, pp. 227–9; VII §§39–40, pp. 405 f. I do not mean to imply that these passages do not have application elsewhere in the texts.

[51] IV §36, p. 244.

philosophising about mathematics. Where Plato and Leibniz were sadists, one could say, doing terrible harm to the whole of philosophy on the basis of their obsession with mathematics, Descartes and Wittgenstein were masochists, fascinated by mathematics but hurting our understanding of it in gruesome ways.

8.5 The motley of mathematics

One of the reasons for this negative judgement is the tremendous desire to regard mathematics as a seamless whole. As I said in Section 1.3 above, the idea that mathematics forms a unity has been an implicit assumption of philosophies of mathematics, perhaps a precondition for there being such a thing as the philosophy of mathematics. But now we must emphasise the wholesale diversity of the subject.

One of Wittgenstein's most emphatic remarks is that 'Mathematics is a MOTLEY of techniques of proof'. The capitalised English word 'MOTLEY' translates a pair of words that, speaking literally, may be given more emphasis than any other pair in the entire Wittgenstein corpus. He wrote in German that mathematics is a 'BUNTES *Gemisch* von Beweistechniken'. There we have a capitalised word followed by an underlined one.[52] *Bunt* primarily means parti- or many-coloured. In the Bible, it is the German word for Jacob's coat of many colours. It may suggest the English word 'bunting', which aside from ornithology is now almost solely used for the many-coloured flags that sailing ships put out.[53] The same German word, with its image of many-colouredness, features in the ode at the start of Nietzsche's *Thus Spoke Zarathustra*, which may in turn recall another poem in yet another key, Hopkins's *Pied Beauty*.

8.6 The terms of art

The *Remarks* are a motley about the motley of mathematics. Many of them address phenomena that in their different and to some

[52] III §46, p. 176. Cf. the text for n. 3, where 'motley' translates *Buntheit*.
[53] This may be a false friend or a mixed marriage, with the 'bunting' first denoting a type of cloth.

extent isolable ways prompt philosophising about mathematics. Wittgenstein does not address the synthetic a priori character of mathematics as a whole. That is, he does not ask Kant's question, 'How is pure a priori knowledge possible?' with its explicit assumption that all pure mathematics is synthetic and a priori. But that is not to say he has no interest in Kant's question. Reading him, we may begin to ask what it is that makes Kant's question itself possible? He does give point to one thing that led philosophers to talk that way. 'The distribution of primes would be an ideal example of what could be called synthetic *a priori*, for one can say that it is at any rate not discoverable by an analysis of the concept of a prime number.'[54]

Likewise Wittgenstein does not give us chapters on the nature of logical necessity. He does address the experience that gives us the idea of necessity, often using the word 'must', although there are other words such as 'inexorable' in play. 'A proof leads me to say: this *must* be like this — Now I understand this in the case of a Euclidean propositions or the proof of "$25 \times 25 = 625$", but is it also like this in the case of a Russellian proof . . . ?'[55]

I know of no other celebrated author who on occasion wonders why we are so sure that this or that question, or piece of reasoning, or knowledge, counts as mathematical at all. 'Why then are we inclined to call this problem straight away a "mathematical" one?'[56] That is a question with which one might begin a treatise on the philosophy of mathematics, and not have finished answering it even at the end.

8.7 Conviction

Wittgenstein took up a lot of philosophical questions about mathematics, ancient, modern, and new. The book in our hands is pieced together from scripts composed at different times, but the traditional waterfront is covered remarkably well. Since I claim that the *Remarks* address established philosophical issues, I should sketch

[54] IV §42, p. 146.
[55] III §30, p. 165.
[56] IV §20, p. 384.

Wittgenstein's attitude to one traditional philosophical problem or doctrine. I shall choose necessity, aphorised as 'the hardness of the logical must'.[57] Some proofs, some propositions (even that in *Meno*), create a philosophical problematic of necessity. We see that there not only is no greatest prime number, but also that there cannot be one. We do not say that because of some theory of necessity; the experience of the proof produces a need for theory.

Philosophers nowadays are trained, as they were not in Wittgenstein's time, to ask whether there is any sharp contrast between the necessary and contingent. That, in my opinion, is of no account here. It does absolutely nothing to our conviction that there is no greatest prime to be told there is not in general a sharp distinction between the necessary and the contingent. For those who grasp and know the elegant proof, the lack of a universal distinction does not affect our wonder at the experience the proof gives us. Wittgenstein was directing our attention to the particular, not the general.

'Musts' come less from a direct sense of how things must be than from the experience of reasoning. This is not a peculiarly Wittgensteinian thought. The verbal version of this fact was a commonplace for Oxford linguistic philosophy, where it was urged that 'musts' modified inferences, and expressed the way in which a conclusion followed from premises. Only derivatively do we get the idea that some propositions 'must' be true.

Wittgenstein honours the experience of mathematical conviction. He usually does so without using the word 'necessity'. It is as if he believed that the old examples, examined using the old terms of art, have been made so trite that we cannot bear to sit still and look them in the face. So we have to take new and even more simple examples, and describe them in a way that is uncluttered by past verbiage. It is not Wittgenstein but we philosophical readers, like

[57] 'Die Härte des logischen Muss', I §121, p. 84. This is translated as 'The hardness of the logical *must*', but there is no emphasis in the printed German version. The word 'must' is often emphasised when it is used as a verb, for example 'Es *muss* stimmen' ('it *must* be right' I §137, p. 91. But often it is an interlocutor who emphasises: '"Aber *muss* es denn nicht so sein?"' ('"But doesn't it *have* to be like that, then?"', I §168, p. 100.

myself, resolutely determined to play the old game, who use that generalised label, 'necessity', as if it were some property that propositions might or might not possess.

8.8 Application

Wittgenstein leads us to ask what it is that makes us think of this or that conclusion as necessary. He usually invokes novel examples. How can we get at this ill-expressed feeling of logical compulsion, and of a priori knowledge? I have emphasised often enough the Janus-faced aspects that help form the notion of the a priori. First is the unreasoned conviction that results from having grasped a proof. Secondly, the power of what is proven, in Russell's word, to 'anticipate' facts of experience. We seem to look inward to our conviction and outward to the world. The conviction without the application leaves us without the anticipations of experience, and hence without a full sense of the a priori. 'The assertion that the proof convinces us of something leaves us cold, — since this expression is capable of the most various constructions.' 'What interests us is, not the mental state of conviction, but the applications attaching to this conviction.'[58]

One way (and only one of the ways on which Wittgenstein dwells) to attend to application and anticipation is to take seriously the idea that an experiment could not confute a proven proposition. What would it be to test a theorem by experiment? One experiment would precisely follow the procedures of the proof itself. In that sense a proof may be like a picture of an experiment. 'Thus I might say: The proof does not serve as an experiment; but it does serve as the picture of an experiment.'[59] In so far as we take something as a proof, we are showing how a concept must be applied. Hence the creation of a proof can be said to create new criteria for the application of terms in the theorem; they are new in the straightforward sense that we would not use them as criteria without the

[58] III §25, p. 161.
[59] I §36, p. 51.

proof.[60] But would not the connections have held anyway? Doubtless, but not 'of necessity'. Think of necessity as a something that a proposition can acquire.

8.9 New criteria

Most philosophers who are content with modal notions find this idea completely repugnant. If a proposition is necessarily true, that's it. C. I. Lewis's system of modal logic S4 captures this expectation with the axiom that 'necessarily *p*' strictly implies 'necessarily necessarily *p*'.

Perhaps one could make peace between this axiom and the doctrine that propositions acquire necessity only through proof. An unexpected team of shuttle-diplomats might be called in — C. G. Hempel and Imre Lakatos. In Section 5.1 I recalled Hempel's doctrine that the mathematical proposition expressed by $7+5=12$, or by a theorem of geometry, is distinct from the empirical one, even if expressed by the same sentence. In Section 7 I drew attention to Lakatos's examples of literally new sentences coming into being in the course of the dialectic of proof-development. These ideas can be made to agree surprisingly well with Wittgenstein's reiterated theme that proofs fix the sense (*Sinn*) of what they prove. 'The proof constructs a proposition; but the point is *how* it constructs it.'[61] This is of course not a claim from the approach to mathematics called constructivist; it is about the construction of a sentence with a sense. It is about the construction of this meaning as the statement of a truth. What truth? The truth of what is proven. And, in this way of speaking (it is no more than that) the sentence that now means the necessary proposition comes into being with that meaning in the course of proof.

[60] Wittgenstein provides many examples. For a different one, involving a ball inside a cylinder, see my 'Scepticism, Rules, Proof, Wittgenstein' in I. Hacking (ed.), *Exercises in Analysis by Students of Casimir Lewy* (Cambridge: Cambridge University Press, 1985), pp. 97–106.

[61] III §29, p. 164. The German word is *Satz*; here and in other quotations the word could well have been rendered 'theorem'.

8.10 An alternative way of speaking

Modal logics formalise ways of talking philosophically. 'Necessity' and its ilk are not words of mathematics proper, but words prompted by our experience of mathematics. One might make the following proposal after reading Wittgenstein. We could revise our usage of the laudatory 'necessarily true', so that a proposition is called necessarily true only when we have a proof that enables us to see that it must be true. Once one starts to revise a way of talking, there may be many options. For example, liberally adapting Hempel and Lakatos, we could say that a sentence expresses a necessary proposition only when it comes at the end of a perspicuous proof, and is put to further use. If you want to picture the necessary proposition as always existing as an abstract entity, which has only recently been accessed by human beings, by all means form that picture. It is a fine picture, which explains nothing.

8.11 Not 'strict finitism'

Truth is different from necessity. Does Wittgenstein want to claim that truths of mathematics are not true until they have been proven by a perspicuous proof? He never says so. Hence I suppose that he does not mean to say so. He does not assert the opposite either. He leaves for others non-mathematical statements like this: 'Since Fermat's last theorem has now been proven, we know that it was true the day that Fermat formed his conjecture — Fermat was right, even if he was almost certainly wrong in thinking he could prove it.' Wittgenstein might have thought that we are easily misled by this straightforward statement about Fermat, but he would not deny it.

In his 1959 essay on the *Remarks* Michael Dummett introduced the expression 'strict finitism', and proposed that Wittgenstein's reasoning invited a doctrine that could be named strict finitism. The philosophical arguments for strict finitism that have been examined by Dummett and, for example, Wright are of great interest. But I am not inclined to attribute them to Wittgenstein (this in no way implies that the arguments are not sound).

First recall what Wittgenstein said about finitism. This is traditionally understood as the denial that there are sets with infinitely many members. It is also the name for the programme of reproducing as much classical mathematics as possible, without assuming the existence of any infinite set. 'Finitism and behaviourism are quite similar trends. Both say, but surely, all we have here is . . . . Both deny the existence of something, both with a view to escaping from a confusion.'[62] Wittgenstein seems to have thought that there were real confusions, from which behaviourism and finitism tried to escape, but that these two -isms were themselves confused types of escape.

Finitists have no trouble with sequences that can in principle be completed in a finite number of steps, with sets that can in principle be enumerated, or with proofs such that the number of applications of rules of inference leading from axioms to theorem is finite. What is strict finitism? It would be the doctrine that the only sequences that exist now are those that have actually been completed, and can be seen to have been completed. The only truths of mathematics are those that have actually been proven by perspicuous proof.

In so far as strict finitism is a positive doctrine, I think the Wittgenstein-style judgement has to be that strict finitism denies the existence or truth of something with a view to escaping from a confusion; to assert the positive doctrine is to commit another confusion. An argument to this effect would be as long and subtle as the examinations of strict finitism by Dummett and Wright. Here I content myself with the more modest observation that the opinions discussed in Sections 8.6–8.9 above do not involve strict finitism.

But do not the ideas sketched above amount to strict finitism? No, they say only that the problematic character of mathematical theorems, for example the sense that they are necessary, arises only in the context of their proof. If we are forced to say what there is or is not, then say, if you must, that there is no such property of necessity which propositions simply have or lack. Moreover, do not think that when a proposition has been proved, it has now acquired, in the abstract, some character of necessity. That would be analogous to some versions of intuitionist and constructivist thinking:

[62] II §61, p. 142. There is also a more jocular reference to a finitist, V §37.

that once a proposition has been proven to the satisfaction of an intuitionist or constructivist, then it has passed from one status, a limbo of not-(yet)-true-or-false, to a new status, true. Such an idea is totally alien to anything found in Wittgenstein. The characteristic that, following tradition, I have been calling necessity is something that attaches to propositions in use, not in the abstract realm that Brouwer adapted from Kant.

8.12 Explanation and description

To return to my title question, what did mathematics do to Wittgenstein? Like Plato, he was fascinated by perspicuous proof. Like Leibniz, he was entranced by combinatorial argument. Both those philosophers wanted explanations of the phenomena that captivated them. I suggest that one of the most powerful effects of mathematics on Wittgenstein is a realisation that classical explanations are wrong not in detail but in ambition. I have been emphasising Wittgenstein's particularity *ad nauseam*. But here we arrive at a general maxim. With some consistency Wittgenstein enjoined it on himself. Describe; do not try to explain. In the context of mathematics that means look very carefully at the simplest phenomena that set us to philosophising about mathematics. Do not try to explain them by any general feature of all mathematics. See instead that it is just not true that all mathematics has these features.

A second effect of mathematics on Wittgenstein was to enforce a second general maxim. In the course of description one should resist all attempts to generalise. He was just as struck by certain examples as were Plato and Leibniz, but he chose to describe them in all their individuality. He thereby cut short the philosophical explanations, the shocking uses that philosophers have made of mathematics.

These two maxims are not just lessons for philosophising about mathematics. They are lessons for philosophy. And here, paradoxically, Wittgenstein was just like Leibniz and Plato. He allowed a pair of maxims so wonderfully suited to his reflection on mathematics to become watchwords of his entire philosophy. In short, we have another case of the infection of philosophy by mathematics.