Chapter 14*
Mathematical Knowledge

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§14*.1 Synthetic *A Priori*?

If mathematical knowledge is synthetic *a priori*, we owe an account of how it is possible. Some deny that. In her most recent book,[1] Professor Maddy argues that though extrinsic justifications of the axioms of set theory may be sought and found, they are not essential. Set theory is a going concern, and is quite all right as it is. The argument from use is weighty, in mathematics as in ordinary language, but, *pace* Wittgenstein and his followers, it is not conclusive. A mathematical practice could have to be abandoned, not only on account of an internal inconsistency, but because it presupposed propositions which were, on grounds quite external to mathematics, untenable. The very fact that many American philosophers of mathematics feel obliged to find an accommodation between mathematics and their naturalist metaphysics, witnesses to their not being totally disconnected.

Empiricists deny the possiblity of *a priori* knowledge absolutely, and seek to accommodate it in other ways. Professor Kitcher gives a causal account of a docile schoolgirl who believes that what her teachers tell her is warrantedly true.[2] As an explanation of how some people acquire mathematical abilities, it has much truth; the multiplication tables often are learned by rote. But we are seeking

[1] Penelope Maddy, *Naturalism in Mathematics*, Oxford, 1996.

[2] Philip Kitcher, *The Nature of Mathematical Knowledge*, New York, 1983, pp.91ff.

a justification, not a psychological explanation—even in the best-run schools some naughty boys ask why six sixes are thirty six, and are not prepared to accept the teacher's say-so as an adequate reason for believing it to be true. A hard-line empiricist may refer vaguely to the experience of previous generations, and encourage us to accept mathematics as the hard-won wisdom of the many and the wise, but once we have felt the force of Plato's *Meno* argument, we shall seek a more compelling account of cogency.

Fundamentally we feel the force of reason, and we cannot give a full formal account of reason. Unless we secure absolute validity at the price of absolute vacuity, we shall reach an end of arguing, where no more can be said, and we can only hope that the desire to know, and the ability to reason, will enable the person we are arguing with to abandon opinions held because they were his in favour of judgements held because they are true. But though that is the ultimate stage, there are many partial prescriptions and formalisations, which may enable him to see the error of his previous ways, and enable us also to detect flaws in our presentations, and see the whole issue in a more rational light.

The requirements of rationality afford adequate justification for adopting the Principle of Recursive Reasoning, Argument by Iterated Choice, and for there being infinite possibilities, and hence for various axioms of infinity; but there is an alternative view of the axioms, connected with meaning rather than rationality, which is also relevant to the completeness theorems. The axioms of different geometries are often seen as implicitly defining different concepts of point and line. The axiomatic extension of the natural numbers to include the negative integers, the rational numbers, the reals and complex numbers, can be viewed as articulating further our concept of number, as can the Axiom of Choice and other axioms of set theory, our concept of a set. This offers a different view of the valid but unprovable well-formed formulae of second-order logic. For there is *a* completeness theorem, due to Henkin.[3] But it involves secondary (*i.e.* non-principal) interpretations on unnatural models. If we extend the number of models taken into consideration, we reduce the number of well-formed formulae which will be true in all of them. Hence, if we admit weird models of second-order propositions, we have fewer well-formed formulae that are true in

[3] See Alonzo Church, *Introduction to Mathematical Logic*, Princeton, 1956, Metatheorem **546, p.314, and generally ch.5, §54, pp.307-315.

all of them, and it is easier for them all to be provable. The range of admissible models is thus of crucial importance in determining what shall count as a logical truth. With regard to models of Peano Arithmetic in first-order logic we were able to say which was the standard one and which were non-standard ones even though the difference could not be formulated in first-order terms. When we embed one theory in another, we restrict the range of available models. Some, non-Desarguian, models of projective plane geometry are not models of three-dimensional projective geometry. In rejecting non-Desarguian models of projective plane geometry we are discerning the shape of the models we had in mind when we sought axioms for projective plane geometry. But this is not a matter of arbitrary choice. Just as do have an idea of the intended model of the natural numbers, and reject non-standard ones, even if they satisfy a first-order specification of Peano's axioms, so the wider concept of *number*, of *dimensional space*, and—arguably—of *set*, are not constitutively defined by their definitions, but have a life of their own. On the strength of intuitions about what are the naturally intended models, I restrict the range of standard interpretations, and thus have a wider range of well-formed formulae that are true in all of them.

Second-order propositions on this showing *are* true in virtue of meanings, but not just the meanings of the terms involved, and thus not analytic in any derogatory sense. Rather, meanings in mathematics are not as fixed as the Formalists suppose, and may develop in a rationally coherent direction, much as they do in ordinary language. Meaning, in particular, is tied up with inference patterns, and hence with the validity of inferences.[4] Thus the Gödelian extensibility of valid inference beyond any pre-assigned formal limits, is paralleled by an extensibility of meanings in a way not licensed by antecedent rules, but rational none the less.

[4] See above, §3.3.

§14*.2 Not Seeing But Doing

Mathematical knowledge is very largely knowledge how to do things, rather than knowledge that such and such is the case. We learn how to do long division, solve quadratic equations and differential equations, how to do vectors and tensors and Fourier analysis. We differentiate and integrate and solve, and the whole theory of groups is naturally expressed in terms of transformations we carry out, rather than theorems we contemplate. In spite of the scorn poured by Plato and Aristotle on the operational language used by geometers,[5] group-theoretical considerations constituted a good argument in favour of Euclidean geometry; and we (and Euclid[6]) still talk of squaring and extracting square roots, and believe that in order to get hold of a concept and grasp what it really comes to, we need to have much practice in manipulating it. It is in our hands as well as our eyes that we humans excel. Besides the many visual metaphors we have for intelligent activity, we have some from the hands: *ὑπολαμβάνω* (*hupolambano*) 'grasp', 'comprehend'. The usage is not just a regrettable *façon de parler*, but an aspect of arguing which cannot be eliminated altogether, however much we formalise it. This was one reason why Formalism failed. It could not entirely eliminate *know-how* in favour of *know-that*, and required mathematicians to be able to exercise a minimal skill in recognising when a rule of inference applied, and on occasion a much more *recherché* one in assuring themselves that a particular formal system could be interpreted so as to apply to a particular model.[7]

Sometimes, admittedly, we can replace *know-how* by *know-that*, and advantageously so, when we want to formalise arguments in order the better to scrutinise them. Nevertheless, the real import of some axioms, such as Peano's Fifth Postulate or the Axiom of Choice, is more clearly discerned if they are seen as rules of inference, rather than assertions of recondite fact.[8] Moreover, Gödel's theorem shows that however far we go in articulating our inferences as instances of some specified rule of inference, there will

[5] See above, §2.7.

[6] See above, §2.2.

[7] See above, §3.6, and below, §14.3, §14.4.

[8] See above, §6.5, and §13.6.

still be further inferences, evidently valid, not covered by the specified rules.[9] Drawing valid inferences is a mathematical activity that cannot be fully formalised, and must be accounted one of the things a mathematician does, rather than truths he sees.

Once we see the mathematician as an active operator who does things he knows how to do rather than a passive percipient of eternal truths, mathematical knowledge appears much less puzzling.[10] Whereas claims to know that something is the case invite questions "How do you know?", which we are hard put to answer, claims to know how to do something are vindicated by actually doing it. Moreover, I know not only how to do things, but what I shall do,[11] and what I can do. As a doer I have freedom of choice. Choice not only gives sense to the quotifiers, allowing you to choose the worst case for my thesis, or me the best case, but offers us a way into the modal concepts of possibility, impossibility and necessity.

But mathematics is not just a collection of personal skills, and personal choices. The canons of success are interpersonal. We need to consider not just what I can do, but what in principle might be done; we have to move from "the actual operations of human agents" to "the ideal operations performed by ideal agents".[12] Kitcher claims, in imitation of Mill, that "arithmetic is a permanent possibility of manipulation",[13] or, more formally, that mathematics is "the constructive output of an ideal agent" (though, somewhat nervously, disclaiming any actual belief in God).[14] The God's-hand view is illuminating. St Augustine and Cantor were able to accept the actual infinite because God could actually survey it. Divine omnipotence is the right modality for the mathematician's 'can'. In our sublunary world we can aspire towards it by means of quotifiers and more human modal operators. Although poor mortal that I am, I cannot count all the natural numbers, it remains the case that, given any particular number, I can go on to the next; and if I could not, someone else could. I can not only report on my own actual abilities, but consider what someone might be able

[9] See above, §8.11.

[10] See C.Ormell, *The Peircean Applicability of Mathematics*, London, 1997.

[11] See above, §1.5.

[12] P.Kitcher, *Mathematical Knowledge*, New York, 1983, p.117.

[13] p.108.

[14] ch.6 generally, esp. pp.108-122 and n.18.

to do in hypothetical circumstances, and what it would be like if what could be done were done. We thus have epistemic access to a wide range of impersonal possibilities, which we can suppose to be actual. The puzzling feature of mathematical modality,

Ab Posse valet consequentia Esse,

begins to look less inexplicable, as also its corollary, that what is, is necessarily so.[15] Considerations of omnipotence are illuminating, but not to be presumed upon. Mathematicians, like political theorists, tend to be "egotheists", each seeing himself as God, and not having to take seriously the imperfections of himself and his fellow men.[16] My drawing an inference is not a guarantee of its being valid. Mathematical claims are contestable, and may be challenged and vindicated.

§14*.3 Pattern Recognition

The argument of the two previous sections shows that mathematical knowledge depends crucially on the correct discernment of patterns. The experience of mathematicians bears this out. Although we break down proofs into a number of steps, we do not comprehend them as that. To understand a proof we need to see it as a whole. It is a common experience to follow each individual step of a proof, but not to be able to see what it all adds up to, not to be able to grasp it as a whole. Often, after having worked at the details for a long time, comprehension comes suddenly in a flash, and the different bits fall into place, and it all comes together.[17] When that happens, we are moderately sure that we do understand the proof, and could reproduce it if necessary on another occasion, whereas until then we had not got hold of it, and though we might mug it up and memorise it for an examination, we did not really have it in our minds, and could not claim really to know it.

[15] See above, §1.6, §7.10, and below, §15.6.

[16] See above, §11.2.

[17] M.D.Resnik, "Mathematical Knowledge and Pattern-Cognition", *Canadian Journal of Philosophy*, **5**, 1975, p.32, reports how for many years he had studied Bernstein's theorem, and could reproduce the proof in teaching it to others, but it was only on reading Cohen's presentation that he really understood it, with all the disparate pieces falling into place. See also his *Mathematics as a Science of Patterns*, Oxford, 1997, esp. ch.11.

This experience is widespread, but in recent years has been discounted in our thinking about the epistemology of mathematics. In part this has been due to the teaching of Wittgenstein, who ridiculed the probative value of the "got it" experience. He argued that by itself an experience of having got it, was of no consequence: the proof of the pudding was in the eating; if a schoolboy could reproduce a proof in an exam, he would get full marks, irrespective of whether he had had the "got-it" experience, and if he could not reproduce it, he would get no marks, no matter how strongly he had felt that he had got it. Wittgenstein's argument is cogent against someone who maintains that first-personal experience is by itself conclusive evidence of mastery of some technique. But that is not what is claimed. If, as is generally observed to be the case, the "got-it" experience is regularly associated with a subsequent ability to produce and deploy the proof on demand, it gives a reliable indication of the nature of that ability. It shows that the ability is not a collection of disparate competences to take separate steps, but an integrated ability to take each step in the context of the others as part of a complete manoeuvre focused on an over-all goal. Many abilities are of this sort—swinging a golf-club so as to propel a golf ball to the next hole, for example—and there are various indications of this being so. In the case of an intellectual ability like that of understanding a mathematical proof, most of those indications are bound to be first-personal reports of introspective experience. But to discount those out of hand is behaviourist prejudice, and simply precludes our ever having an informed understanding of what it is to understand.

Much of mathematics is concerned with discovering and communicating intelligible *Gestälten* which will make proofs surveyable, and enable the mathematician to discern the outline of the wood, without being distracted and confused by irrelevant detail of the trees. Granted the decimal notation, we can see at once that

$$7034174 + 6594321 = 13628495.$$

The decimal notation has a dual aspect: it not only provides in a mechanical way a numeral for each natural number, but is also a polynomial in '10', so that we can reduce the otherwise unsurveyable calculation to a small number of much more manageable ones.[18] In a similar way, Goodstein's theorem, which at first sight seems highly counter-intuitive, becomes obviously true, once we replace finite numbers by transfinite ones.[19] We seek new ways of looking at problems, so as to be able to get a grip on them, and twist them round to clear all obstruction to their solution. The theory of groups, Galois theory, the exponential and trigonometrical series, give us new looks, enabling us to focus on the point at issue, and understand clearly and distinctly what the question is, and what its answer must be.

Philosophers of mathematics in recent years have had problems with pattern recognition. They feel that Platonic "seeing" is a metaphor, not an account. Nevertheless, we often do recognise patterns. We recognise faces, voices, tunes, smells and tastes. Beethoven's Fifth Symphony is a pattern. I can recognise instances of it, and in recognising particular instances recognise in them the generic type. We characteristically use visual metaphors, but it is not only visual patterns that we recognise, and it is useful therefore to stress non-visual instances of pattern recognition, and to think of the theory of groups as being exemplified in campanology, or in turning round a sheet of paper, or in the manipulations of a solid object. Besides tunes and voices, we can also recognise the style of composers, and more generally literary style. Sometimes even it is possible to recognise a mathematician's style of argument. Style is peculiarly elusive and highly personal, and the fact that we can often recognise the style of a particular person is reason for thinking that personality is something real. A Platonist is quite entitled to ground his claim that mathematics is real on our being able to recognise a pair, a triplet, a foursome, as instances of two, three, or four; to see squares as being square, and to talk about the square and its diagonal.

Just as a large part of learning ornithology or radiography is the development of the ability to form and recognise the relevant

[18] I owe this illuminating point to Mark Steiner, *Mathematical Knowledge*, Cornell University Press, Ithaca, NY, 1975, ch.1, pp.43-45. See above, §7.8, §7.9.

[19] For a simple account, see Roger Penrose, "On Understanding Understanding", *International Studies in the Philosophy of Science*, vol.11, no.1, March, 1997, pp.11-15. Like Penrose, I owe the example to Dan Isaacson, Fellow of Wolfson College, Oxford.

patterns, so in mathematics a large part of the skill lies in being able to do the same, and handle them with dexterity when needed.

Admittedly, it remains unclear exactly what this ability is. It could be that recognition really was re-cognition, as Plato argues in the *Meno*; or that we have an innate ability, as Chomsky holds with regard to linguistic patterns, to select in accordance with certain principles of classification rather than acquiring it entirely from experience. But it is just a fact of human intellectual powers that what we talk about, and communicate as reports of what we have perceived, goes beyond the stimuli operating on our sense organs. Those who, for extraneous metaphysical reasons, would deny to mathematicians the ability to discern patterns, should, in all consistency, disclaim the ability generally, and in particular disallow the Formalist's ability to recognise a formal inference pattern, and the sceptical philosopher's ability to use linguistic patterns to express his doubts.

Nevertheless, caution is needed. It is dangerously easy to make use of some perceptual metaphor. Although when we perceive, we characteristically construe our perceptual stimuli in terms of some pattern, we can also recognise one pattern within the framework of another, as when we see the diagonal of a square as the side of a larger square. The difference is crucial, and reveals itself in the logic of mathematical discourse. Suppose Hardy or Gödel or Cohen told me that they had perceived the falsity of the Continuum Hypothesis. They try and point it out (de-monstrate) it to me. I fail to see. Could I just accept their word for it? Only in a necessarily provisional way. Although I can take it on trust that Fermat's Last Theorem has been proved by Wiles,[20] the trust is not in his veracity, but that he will be vindicated by independent assessments of his proof. Mathematics is quite different from history or geography, where we can achieve knowledge only if we are willing to take it second-hand from authorities who were in the right place at the right time to observe.

More plausibly I attribute my failure not to my not being in the right situation, but my perceptual incapacity. Perhaps I suffer from "continuum blindness", analogous to colour blindness. Suppose that a minority—but only a minority—of competent mathematicians report that following the demonstration, they, too, can

[20] See above, §7.4.

see it to be true. Would we accept this, and say that the others were, like me perceptually defective? Or would we credit the minority with a special power of perceptual discrimination, in the way that a minority of human beings can taste phenyl-thio-urea?[21] No. Mathematical knowledge not only does not depend on the authority of the observer, but does not respect the authority of the wise in the way that literary critics, jurisconsults, historians and other scholars should. Although on occasion we will accept the say-so of a mathematical colleague that some proposition is proved, or some purported proof valid, we could not be content to leave it there. We want more—that any competent mathematician *who tries*, should be able to make out the pattern, and validate the proof, for himself. There must be an *independent* criterion of competence. Mathematics is different from histology. In histology it is almost, if not quite, a test of competence to be able to see, with the aid of a microscope, bodies in the cell. The tests of mathematical ability, however, are more various, and mathematical discernment is not tied to any particular sense, and is permeable to argument. It would not be enough if there were a stable minority of mathematicians who could discern the truth of the Continuum Hypothesis and could communicate about it among themselves. The mathematical community is necessarily in principle an open community, enshrining the principle that there is open access to mathematical truth, open to all committed truth-seekers.

We are left with a paradox. The claim of mathematics to offer open access to all comers is belied by the fact that only rather clever people can do mathematics well. The paradox is resolved when we see mathematics not as a monologue, but as a dialogue, in which the cleverness of the mathematician is balanced by the unselective choice of respondent, and the cogency of mathematical argument is shown by his being forced to concede the inferences the mathematician wishes to draw.

§14*.4 Lakatos

The history of mathematics is much messier than we are supposed to suppose. Most important theorems were false when first enunciated. It is only gradually, as counter-examples are discovered and guarded against, that definitions are tightened up, and the

[21] Jonathan Bennett, "Substance, Reality and Primary Qualities", *American Philosophical Quarterly*, **2**, 1956, pp.1-17, esp. pp.9-10; reprinted in C.B.Martin and D.B.Armstrong, eds., *Locke and Berkeley*, London, 1969; see also his *Locke, Berkeley, Hume: Central Themes*, Oxford, 1971, p.95.

theorems properly proved. A great mathematician has a deep insight, and puts forward a proof or a concept, which wins general acceptance. Later doubts arise. Sometimes the argument itself is suspect—some series has been summed to infinity illegitimately, or some denominator has gone to zero. At other times the method of proof applied elsewhere leads to contradictions,[22] or a counter-example emerges.[23] The proof is re-examined, and some hole noticed—an extra condition tacitly assumed, or some possibility overlooked. Often it is a simple matter to tighten up the definitions, or to add some extra conditions narrowing the range of the theorem's applicability. Sometimes there are two competing concepts—*e.g.* Leibniz' and Weierstrass' concepts of the continuum—which are refined and distinguished only as something that is true of the one is not proved to the satisfaction of those who are working with the other. The concepts are then distinguished, and we have a simple bifurcation of topic, as some mathematicians work with the one, and others with the other.

Two important, but contrasting, morals are to be drawn. The first is that the process of mathematical discovery is dialectical. It is not, as we commonly assume, a steady incremental process as theorem is added to theorem, and the stock of mathematical truth is monotonically increased, but a zigzag movement, in which claims are made, but sometimes countered, and needing to be withdrawn, at least temporarily, until objections can be met, and difficulties overcome. Over the course of history, the typical connective of mathematical argument has not been uniformly 'therefore' but often 'but'. But this is not the only moral. The other is that though controversies have raged, they have usually been settled in the end, and settled positively. Theorems have been disputed, narrowed, emended, but very few have been found fatally flawed. Temporary withdrawals there have been, but very little ground has been permanently lost. The proofs may have seemed defective to later critics, but the truths have nearly always been vindicated.

Mathematicians tend to despise the history of their subject. Like scientists, they do not read the works of their predecessors, but prefer modern textbooks. They reckon that the history of the subject is irrelevant to its content; and that the heuristic process

[22] As Cantor's proof of the Power Set theorem became Russell's proof of his paradox.

[23] As happens time and again in the argument of ch.10, §§10.3-10.8.

of discovery and the logic of justification are totally distinct. That view has been controverted, most notably by Professor Lakatos.[24] Lakatos maintains that the two cannot be sharply separated, and that we *can* learn the logic of justification from considering the history of discovery, in much the same way as Kuhn does in the philosophy of science. Lakatos' favourite example is Euler's theorem that in a simple polyhedron V, the number of vertices, added to F, the number of faces, is two more than E, the number of edges. In symbols: $V + F = E + 2$

Figure 14*.4.1 Euler's proof that $V + F = E + 2$, by removing one face ($ABHE$) cutting open the polyhedron (which increase the number of edged and vertices equally), laying the sides out flat, triangulating them, and removing them one triangle at a time.

He discusses this at great length, producing large numbers of apparent counter-examples, which can be ruled out only by various "exception-barring" clauses. After a very full discussion he produces a definitive proof in terms of "k-polytopes".[25] A k-polytope

[24] Imre Lakatos, *Proofs and Refutations*, Cambridge, 1976; and *Mathematics, Science and Epistemology*, Cambridge, 1978. See also Philip Kitcher, "Mathematical Rigor—Who Needs It?", *Noûs*, **15** 1981, pp.469-493, esp. p.482; and his *Mathematical Knowledge*, New York, 1983, chs.7-10; and M.D.Resnik, "Mathematical Knowledge and Pattern-Recognition", *Canadian Journal of Philosophy*, **5**, 1975, pp.25-39, esp. p.33.

[25] pp.109-116.

is a k-dimensional figure: a polyhedron is a 3-polytope; its faces are 2-polytopes; their edges are 1-polytopes; and their vertices are 0-polytopes. He defines k-chains, boundaries, closed k-chains, and circuits, in terms of which he can also characterize what it is for a 3-polytope to be simple. A k-chain is the sum of k-polytopes; the boundary of a k-polytope is the sum (*modulo* 2) of the $(k-1)$-polytopes which belong to it; a k-chain is a closed k-chain iff its boundary is zero; a closed k-chain is called a k-circuit; a k-circuit bounds iff it is the boundary of a $(k+1)$-chain; a polyhedron is simply connected iff all its 2-circuits and all its 1-circuits bound; a surface is simply connected iff all its 0-circuits bound.

Granted these definitions, the theorem becomes:

> All polyhedra, all of whose circuits bound, are Eulerian. And all polyhedra in which circuits and boundary circuits coincide, are Eulerian.

k-chains can be viewed as N_k-dimensional vector-spaces over the field of residue classes *modulo* 2. We can thus view the theorem as one about vector-spaces:

> If the circuit-spaces and boundary circuit spaces coincide, the number of dimensions of 0-chain space minus the number of dimensions of 1-chain space plus the number of dimensions of 2-chain space equals 2.

In that form it can be proved, with a certain amount of tedious algebra.

The sceptical reader may ask "How much has been achieved? Do not the definitions mirror the original proof, so that all we are really saying is that for those polyhedra for which Euler's theorem holds Euler's theorem holds?". Lakatos admits that all the work is in assuring oneself that the definitions really capture the concepts originally invoked. There is much claim and counter-claim in determining exactly what the definition of the concept should be. Once we have specified the concept exactly, the rest of the proof goes through easily.

The emphasis is on concepts. The proof is like a spanner. The first attempts at proof are like spanners which do not exactly fit, and so do not really turn the nut. As we try to engage them, they slip, and we then try to tighten the adjustable spanner, or find a fixed spanner, so as to obtain a closer fit.[26] But if we tighten it up

[26] To deal with the coincident hands of a clock, discussed in §3.6, we need an 11-sided spanner, not, as we first thought, a 12-sided one.

too much, it will not go on at all, and many trials may be needed before we find one that is snug without being too tight.

We develop our grasp of concepts in the same way as we articulate proofs. Again, it is a dialectical process, with initial definitions being criticized and faulted, and new ones refined to obviate objection. The infinitesimals of Newton and Leibniz were open to the objections of Berkeley:[27] Weierstrass circumvented those objections by giving an ε-δ account in terms of a universal and an existential quotifier. Robinson met them head-on, by using the fact that Peano's axioms are not monomorphic to produce a model containing weird numbers greater than infinity, whose reciprocals had just the property, of being neither finite nor zero, that infinitesimals had been required to have. Dirac's delta function was similarly introduced originally with a thoroughly unsatisfactory definition, which only later was tidied up and made rigorous.

It is natural to see these successive approximations to the "true" concept in issue as exercises in Platonic or Aristotelian definition. We have at first only a vague idea of what we are after, but by arguing about it, and trying to see what its implications are, we come to focus on it more closely, and discern more exactly what really is at stake.[28] We are being led back to epistemological Platonism again. We are being asked to recognise patterns. We have to see with the eye of the mind that a certain theorem about linear vector spaces is isomorphic with Euler's original theorem about polyhedra, in much the same way as we have to be able to tell that an uninterpreted calculus studied by the formalist applies to a particular model, or that the theory of groups applies to bells pealing, or to rotations

[27] George Berkeley, *The Principles of Human Knowledge*, Dublin, 1710, §§130ff., and *The Analyst*, London and Dublin, 1734; reprinted in A.A.Luce and T.E.Jessop, eds., *The Works of George Berkeley, Bishop of Cloyne*, London, 1951, iv, 62-102.

[28] Lakatos himself draws a different conclusion, and adopts a quasi-empirical view of mathematics, largely on the grounds that the axiomatic approach is inadequate, and in order to parallel Popper's philosophy of science. But the quasi-empirical account is inadequate too, and gives no account of the coerciveness of mathematical proof. See Zheng Yuxin, "From the Logic of Mathematical Discovery to the Methodology of Scientific Research Programmes", *British Journal for the Philosophy of Science*, **41**, 1990, pp. 377-399, esp. §§1.2, 1.3.

and reflections of a dihedron, or to twists of a Rubik's cube.[29] These are not great feats of pattern-recognition, like that called for by Hardy when he gazes at the distant range of mountains,[30] but they are instances of pattern-recognition nonetheless, and holistic, in that if we overlook some detail, some crucial exception or some instance of double-counting, we may be misled into accepting a fallacious proof, just as many mathematicians overlooked some crucial feature of what it was to be a polyhedron when they sought to prove Euler's theorem.

§14*.5 Cogency and Dialogue

Mathematical proofs must be cogent. Although their holistic aspect is important, they are not like moral or political arguments, or those that arise in disputed matters of literary criticism. They are not cumulative, and we cannot end by striking a balance between two sides of the case, both of them weighty. Nor can we agree to differ: any difference of opinion must be pursued until resolved. If there is any doubt whether the instance in question actually fits the pattern proposed, or a particular inference is valid, or any other objection, we must go into it, and settle it. Mathematical arguments are not mathematical arguments at all, unless they are effectively incontrovertible. Once we have understood a valid proof, we must recognise that the conclusion has to be true, and as we urge it on another, we are pressuring him to concede, however much he would like to resist, that we have made out our case beyond all question.

We secure incontrovertibility by *fiat*. Arguments are both expanded and contracted. They are filled out to make them deductively valid, and narrowed in scope, so as not to apply in controvertible cases. We lay out a proof so that all controvertible points must be granted in advance. If any detail is doubted, we specify it in the *data*. If any assumption can be contested, we articulate it as an axiom. Thus the Axiom of Choice was gradually recognised to be questionable, and so was stated as an explicit axiom, which must be postulated if certain inferences were to be allowed. We seek total explicitness in order to block every hole, and leave the respondent with no option except either to concede, or else discontinue the dialogue. Where we cannot cast an argument in incontrovertible form, we discontinue the dialogue, deeming it not

[29] See above, §3.6, §14.3.

[30] See above, §1.5.

to be a mathematical argument at all. Indeed, some philosophers would deny that controvertible arguments are valid arguments at all. For the most part, however, we are willing to recognise inductive arguments, historical arguments and moral arguments, as arguments, but reckon the questions they address to be outside the purview of mathematics.

Plato sought to secure that there *could* be no gainsaying a mathematical argument by having all mathematical arguments deductive.[31] In his development of the idea of dialectic, he came to think of the respondent as utterly bloody-minded, who would not be persuaded by anything less than the fear of inconsistency, since the one sanction capable of coercing every recalcitrant reasoner into conceding the validity of each step was self-contradiction. Modern mathematicians, likewise, have construed the respondent as a moron, who can follow algorithmic procedures, but not exercise any understanding on his own. An argument was not really cogent unless it worked even on morons and sceptics.

But that ideal of absolute cogency, which could compel the assent of even the most recalcitrant respondent, has proved unattainable. Even the most extreme Formalist has to be willing to recognise some patterns of inference, and is susceptible to informal arguments at the meta level leading to hitherto unformalised Gödelian arguments.[32] Though it accords with the conventions of contemporary mathematical discourse that you should treat me as a fool, I do not like being treated as too much of a fool: it gets boring, if you go on proving to me every time that $2+2=4$. We cut corners. We have to. Not only is time limited, and patience, but our ability to digest fool-talk is limited too. A fully formalised proof is incomprehensible. I cannot see the wood for the trees. I am not a computer, and cannot remember very quickly, or do lots of trivial operations in a nanosecond. Perhaps I think laterally, perhaps in parallel, certainly holistically, and need to be able to see where I am going and to keep track of the thread of the argument. So we moderate our fool-talk, and only itemise the steps that might reasonably be doubted. It is also unnecessary to assume that sure-fire sanctions are needed every time to force my consent. Mathematicians not only lack the patience, but also are free from the limitations, of morons. They are not Thrasymacheanly bloody-minded sceptics;

[31] §1.7, §1.8.

[32] See above, §§3.3, 3.6, 8.11, 14.2.

they share a common concern to know the truth, and therefore are willing to make assumptions for the sake of argument, and concede hopeless cases before being actually check-mated in an actual contradiction.

The argument of Chapter Three[33] that instead of the intelligent and cooperative young men of Plato's preference, opponents must be assumed to be stupid and obstinate, was too extreme. Deductive argument, which must be allowed on pain of inconsistency, is *not* the paradigm, but, rather, a limiting case, where dialogue collapses into monologue, since the claim of anyone who disputes it contradicts itself, and so is, to all intents and purposes, unsayable. Mathematicians, though critical, are not complete sceptics; our hypothetical respondent should be fashioned in their image, not that of the utterly disagreeable fool, contesting every step until driven into inconsistency. There are many cases where I can count on his agreement, not because he must want to be understood, but because he is a rational truth-seeker, who will not niggle needlessly.

§14*.6 On Behalf of the Fool

Fools provide the foil for cogency. It is they who enable the mathematician to distinguish his insights from those of the metaphysician, the moralist, or the poet. But they are not, we have seen, absolutely moronic and totally bloody-minded; nor are they just victims waiting to be compelled by cogent argument to concede the truth of theorems; they are essential collaborators, who will not niggle needlessly. But they are critical collaborators, and will, on occasion, not accept some line of argument, and fail to feel the force of some sanction. Different fools feel the force of different sanctions, acknowledge the cogency of different types of argument, and generate different philosophies of mathematics. Intelligence and willingness to cooperate are not the same. One man can be rational but unreasonable, another reasonable but not very clever. The Intuitionists assume a high standard of mathematical ability in the respondent, but no willingness to make moves for the sake of argument. Classical mathematicians impute to their hypothetical respondents much less ability to recognise a proof when they see one, but a great readiness to play the game in order to discover truth rather than simply to win. By engaging whole-heartedly in the dialogue, responding to challenges and being willing to risk refutation,

[33] In §3.3.

they enable the possibilities of the dialogue to be exploited more fully, giving rise to further cogent patterns of argument. They thus make available arguments by *reductio ad absurdum*, which the Intuitionists are bound, by reason of their unreasonableness, to forgo.

Intuitionist fools, though unreasonable, claim to be very clever, and always able to recognise a valid proof when they see one. If we were all agreed about what was a proof, we should not need to have any characterization of proofs, and could simply say that we recognised a proof when we saw one. The options then open to us would be either Intuitionism or Epistemological Platonism. We could be Intuitionists if we were inclined to be idealists (in the old, metaphysical sense), and consider things as they appeared to us rather than attach any sense to them as they were in themselves. Intuitionism is the mathematical analogue to phenomenalism. If, however, we were inclined to be realists generally, we should be Platonists, and regard our ability to recognise proofs as a quasi-visual ability—the exercise of the eye of the mind—by means of which we apprehended the real pattern of the universe.

On the score of intellectual ability Intuitionism and Epistemological Platonism lie at one extreme and Formalism and Finitism at the other. Intuitionists and Epistemological Platonists assume that we all share a complete common rationality, and that there is really no doubt as to what constitutes a mathematical argument, and we just recognise valid proofs in the same way as we just know what the natural numbers are. Formalists assume no common rationality beyond the minimum needed for communication, and this itself spelled out in explicit rules. As we assume less and less rationality and willingness to cooperate, we are naturally driven to deductive argument, which must be acknowledged as cogent if communication is to be possible and if we are to play the Formalists' game at all. Formalists, however, are prepared to respond to challenges, survey end-games as a whole, and reach meta-mathematical conclusions on the basis of what they can see to be possible, whereas Finitists are not prepared to make any moves on their own account, and will not concede defeat until an inconsistency is actually reached. Only *Sorites* Arithmetic is available to them, whereas Peano Arithmetic is available to those of wider vision, which leads them to avoid not merely inconsistency but ω-inconsistency as well.

Dialectical argument, with challenges as well as straightforward proofs, is essential if we are to extend cogent argument to the transfinite. In order that a proof should be incontrovertible it was required to be finite, so that we could demonstrate its validity in a finite number of steps.[34] Otherwise, it seemed, we should be left in the air, never being able to clinch the argument. If anyone disputed its validity, we needed to be able to take him through it, bit by bit, and leave him unable to gainsay any of the steps. But in a dialogue, if anyone disputes its validity, we can challenge him to show where it is wrong, and examine whatever inference he says is invalid, and rub his nose in the fact that it is not. We can, further, justify a claim about an infinite number of instances, by challenging the disputant to cite a single counter-instance, confident that if he did so, we could show him wrong. This strategy makes use of the fact that every descending sequence of ordinals is finite, enabling us, in a context of challenge and response, to work backwards, and pinpoint any fallacy or unreasonable doubt, if there is either, and otherwise concede the cogency of the claim across the whole infinite field. Instead of having to go on for ever, waving our hand implausibly over an infinite number of instances, we can get our teeth into a definite, finite case, and chew through it to incontrovertible refutation. A "dialectical" justification of the Principle of Recursive Reasoning enables us to overcome the inherent finitude of incontrovertibility, and shows that it is an admissible form of argument, though not one for which a justification could be extracted from a recalcitrant reasoner by appeal to the Law of Non-contradiction alone. The Principle of Infinite Arbitrary Choice can be defended in somewhat the same way. If a sceptic questions our making an infinite number of choices, we ask him where the line is to be drawn between legitimate finite choices and illegitimate infinite ones. We do not in this case, as we do when justifying the Principle of Recursive Reasoning, have already to hand a knock-down refutation of whatever answer he may give; but we can put the sceptic on the defensive, having to defend a distinction which he needs to draw, and for which he has no obvious justification.

Although fools can be distinguished, they are not always distinct. I am not a single fool of utter stupidity, but a whole host of fools, of different degrees of foolishness on different occasions. Sometimes I am quite intelligent, and can take many small steps in my stride, so that the mathematician, by integrating a number of small steps into one block, and dealing with only a few such blocks, secures the surveyability of the proof, but always with the proviso that if I am dubious about any particular point, he can

[34] See above, §3.3.

address me in my new-found stupidity, and argue that particular point until I am satisfied. Furthermore, whichever of my foolish selves the mathematician chooses to address on a particular occasion, he has to address me in the knowledge that there are other fools around, who may interject objections if his inferences are not, by their standards, cogent. I may not be Thrasymachus myself, but he is listening to our conversation, ready to interrupt, if he thinks I am being too soft, and agreeing too readily to the claims the mathematician is putting forward.

Fools perform further useful functions. They force us to distinguish our various rules of inference, and separate what can be established only with their aid, from what can be proved by other means. They also may be themselves forced to articulate their doubts, and thereby belie their dubiousness. The Finitists cannot say what 'finite' means without the aid of second-order logic, itself deeply infinitistic. Boundaries are difficult to draw without being able to overstep them. An intelligent and helpful fool will stumble when a new sort of inferential leap is asked of him, but in the dialogue in which he articulates his difficulty, bridges may emerge that will enable the mathematician to lead him across to new fields of enquiry. As the Formalist explains his ideal of a formal system, he engages in informal reasoning about it, and thus is led to see the cogency of Gödel's argument, and so to realise that no complete and exhaustive formal characterization of a mathematical proof is possible.[35]

The fool is neither an utter fool, nor possessed of an immediate intuitive grasp of all mathematical truths, but occupies some middling position, which itself is under tension, since on the one hand there are always further proofs, of a type not previously envisaged, which he can be brought to acknowledge as cogent, while on the other there is always a certain pressure towards greater articulateness, so as to make the proof accessible to those with a smaller basis of shared insights. The questions the fool asks are real questions that the mathematican needs to answer: but the very fact that he asks them—the terms in which he phrases them—shows that they are not the unanswerable questions of the sceptic, but ones that can be answered from the conceptual resources involved in the very asking of them.

[35] See above, §8.11; see also John Myhill, "Some Remarks on the Notion of Proof", *Journal of Philosophy*, LVII, 1969, pp.461-471.

§14*.7 Hilbert

The account of mathematical knowledge thus far given has failed to quiet the doubts of some philosophers, or set at rest the fears felt by some mathematicians that philosophers were going to destroy the intellectual respectability of their subject. Some philosophers respond by avoiding the challenge of trying to account for mathematical knowledge, and make out that mathematics is not true at all, but only a useful pretend. "Mythological platonism", however, is implausible: we do not put maths books on the fiction shelves. Hilbert can be seen as a mathematician who tried to keep mathematics for mathematicians, safe from philosophical niggles about knowledge and truth.

Hilbert put forward his programme in the shadow of the paradoxes, and sought also to ward off Intuitionist attacks.[36] Radical in his methods, he was conservative in his interests, and hoped to combine the security of austere Formalism with the richness of the new fields of study. He came from geometry. In Euclidean geometry we do not always have it that two lines intersect in a point; they do unless they are parallel, but if they are parallel they have no point of intersection. This is awkward, and in homogeneous geometry we introduce points at infinity, and a line at infinity, which is "where" parallel lines meet. The introduction of points at infinity is a safe extension of Euclidean geometry to obtain greater uniformity; it does no harm, and makes arguments easier to articulate. But can we be sure that it does no harm? Not every extension of an axiomatic system is harmless. If I extend a system by a new symbol *tonk* with the rules of inference that it follows from $A \vee B$ and yields $A \wedge B$, we shall have made our system inconsistent.[37] We need to make sure our new system is consistent, that is, that our extension is a *consistent* extension of the original system. If only we could show transfinite set theory to be consistent, it would be freed from suspicion of paradox and lurking inconsistency. Once we have a consistency proof for homogeneous geometry, we can continue to operate it with a clear conscience that in the process of

[36] See further above, §§12.2, 12.3, 7.4-7.7.

[37] See A.N.Prior, "The runabout inference ticket", *Analysis*, **21**, 1960, pp. 39-39; reprinted in P.F.Strawson, ed., *Philosophical Logic*, Oxford, 1967, pp.129-131.

making geometry smoother, easier to work with, it has not exposed the whole enterprise to self-contradiction.[38]

But doubts could still arise. Although the cardinal sin of inconsistency was out of the question, other inferences might have been legitimised that, though not disastrous, were none the less invalid. To forestall any such imputation, Hilbert required that the new system not only be consistent, but, so far as it overlapped with the old system, it should introduce nothing new; clearly the system of homogeneous geometry will have some new well-formed formulae, namely those containing the new terms, 'point at infinity' and 'line at infinity', and some of these may be theorems of the new system: but if there are no well-formed formulae that can be expressed solely in terms of the old system that are theorems of the new system but not the old, then the new system is a "conservative" extension of the old. In that case it is *very* safe. It does not involve us in anything new, but merely puts the old in simpler and more uniform form. It would be hard to cavil at such an extension. It is, one might suggest, a mere notational device. Any substantial objection to the extended system must be an objection to the old, and if the old is above suspicion, then the new must be also.

Hilbert hoped to treat the arithmetical infinite like the geometrical points at infinity. Instead of *Sorites* Arithmetic, in which I take 515 steps to prove $F(257)$ from $F(0)$ and $F(n) \rightarrow F(n')$, I apply P5 to prove $(Ax)F(x)$ and then use Universal Instantiation, in three lines. Peano Arithmetic does not prove anything more about the natural numbers than *Sorites* Arithmetic does, but it does it much more neatly. In the ordinary arithmetic of the whole numbers, we have the symbols $0, 1, 2, 3, 4, 5, 6, \ldots N \ldots$ to which we should like to add the symbol ∞ so as to have $0, 1, 2, 3, 4, 5, 6, \ldots N \ldots \infty$. The extra symbol is just a notational addition which enables us to express concisely operations like Σ_1^∞, which can be explained in terms of only finite numbers, but are much more perspicuously written with the aid of the symbol for infinity. If we can formalise our rules for the use of ∞ and can show that these rules constitute a consistent extension of the axioms for the arithmetic of just the finite numbers, then it is safe to extend our arithmetic of the finite numbers to include ∞ as well.

Part of Hilbert's programme is, then, to formalise systems and show them to be consistent. Instead of considering homogeneous

[38] Michael Detlefsen, *Hilbert's Program*, Dordrecht, 1986, p.8.

geometry or infinite number theory as being *about* anything, we consider these systems only syntactically, simply as formal systems, without any interpretation in mind. Such formal investigations may reveal that they are syntactically consistent. If so, no inconsistency can arise from using them. But what is their status? Are they only, as the hard-line Formalists aver, games played with meaningless signs on paper? No: for they are conservative extensions of systems assumed to be all right; they therefore have an intended and standard interpretation for all except the "ideal" elements. Except for the points and line at infinity, the symbols of formal homogeneous geometry can be interpreted in the same way as in ordinary Euclidean geometry, that is as points and lines and circles, *etc.* So they are all right. Even if a philosopher has some doubts about interpretation, they are in no worse way than they were before the introduction of ideal elements. As for the points and line at infinity themselves, they are "carried" by the rest of the interpretation. It is much the same as with the account of theoretical entities in physical theories given by the Logical Positivists. They held that only sense-data really existed, but that it was all right to talk about atoms, electrons, and protons, because theories expressed in terms of them could yield predictions that were subject to empirical test: although they did not really exist, yet it was useful to talk about them as if they did; they were fictional entities, but useful ones in as much as they played a part in discourse which did yield statements about what really did exist. For Hilbert finite calculations with natural numbers have the same basic status that sense-data had for the Logical Positivists. They are "contentful", *inhältlich*: they confer reality on the whole system that is, in part, interpreted in terms of them. The symbol ∞ is carried by the finite natural numbers that keep company with it, and are themselves contentful.

We can now see the shape of Hilbert's programme. Hilbert combines caution with generosity. He is cautious in that he will not accept any form of reasoning just because it has been used by mathematicians or yields delightful results, but insists that any system be scrutinised and not accepted until it has been certified as safe from inconsistency and unwarranted innovation. In this he is as stringent as Descartes was in rejecting current modes of thought until they had been re-assessed. But he is much more relaxed than Descartes in the stringency of the test applied. Descartes would not admit any inference if it was logically possible for it to be gainsaid:

only if it was self-contradictory to contradict the conclusion, having conceded the premises, was an inference sound, so that if it were not contradictory to deny that I had a body, having allowed that I did exist, then I could not be said necessarily to have a body. Hilbert is much more relaxed. I can extend a system, so as to legitimise new inferences, in any way I please, so long as I know I shall not thereby run into inconsistency, and shall not say anything new about old-timers. He achieves this freedom of manoeuvre by severing the semantic links between the ideal elements and any supposed referents: 'points at infinity' do not refer to any points on, or just beyond, the Euclidean plane; ∞ is not another number lurking over the horizon of the natural numbers. These terms have no reference at all. They are just symbols, whose only significance derives from the extended systems in which they play a part, which extended systems do have a meaning because of the reference of other-than-ideal elements. Hilbert allows us to re-enter Cantor's paradise, but only on the strict understanding that it is a fairy tale, which has verisimilitude, because sometimes it mentions real people, and says demonstrably true things about them, but is not to be taken literally *tout court*.

Much of Hilbert's programme, therefore, was concerned with providing consistency proofs. On this score he was puritan, rather than liberal. Only finitary reasoning was to be employed in establishing the consistency of some system. Although once the consistency of a system employing infinity was established, infinity could be used with a clear conscience, the consistency proof itself must be finitary. Only so would absolute security (*Sicherheit*) be secured. But is there absolute security? We can agree that some inferences are less open to doubt than others, and that in the face of the paradoxes we do well to justify those inferences that have been impugned by means of others less open to objection. But the assumption that there is some class of inferences which are absolutely above suspicion has not been made out. Hilbert assumes that "finitary" inferences are, but it is not clear exactly what these inferences are. We cannot define 'finite' in first-order logic. Only if we already have the concept of infinity can we specify what the inferences are that do not rely on it. There is not a clear finitary definition of the finitary inferences that are alleged to constitute the class of absolutely safe inferences. We cannot in finitary terms know when we are finitarily safe.

In any case it is not true that in practice long chains of finitary inferences are safe. We are much more likely to make an error in a long calculation involving only finitary inferences than in a short one using the principle of mathematical induction. Maybe this is not what Hilbert had in mind, but it is enough to cast doubt on the concept of security as a clear and adequate guide to the inferences a mathematician can take for granted in seeking to justify others. Instead of an absolute class of real, safe calculations, in terms of which all others ought to be justified if they can, we have a gradation of inferences, some of which are more open to question than others, but none evidently suspect and none guaranteed to be above all suspicion.[39]

It is generally reckoned that Gödel's second theorem put paid to Hilbert's programme, but some philosophers contend that, far from constituting a threat to Hilbert's programme, Gödel's theorems are a protective shield, which protect it from diagonalized embarrassment and refutation.[40] Much turns on how Hilbert's programme is construed. He was himself a classical mathematician, trying to shore up classical mathematics against sceptical attacks by availing himself of unimpugnable methods of argument, but sensitive to other arguments too. From that standpoint Gödel's theorems are damaging. A classical mathematician can feel their force, and be rationally persuaded that absolute *Sicherheit* is unattainable. But the conclusion that absolute *Sicherheit* is unattainable may itself be unattainable by "safe" methods. A sufficiently determined sceptic may be able to go on not seeing that his sceptical position is untenable.

Hilbert was too extreme. His meta-mathematics was too austere, his ontology too relaxed. Consistency proofs give us confidence to embark on new fields of mathematics without fear of disaster, but do not need to be finitary; nor need the new field

[39] For other discussions of Hilbert's programme see: M.Giaquinto, "Hilbert's Philosophy of Mathematics", *British Journal for the Philosophy of Science*, **34**, 1983, pp.119-132; Michael Detlefsen, *Hilbert's Program*, Dordrecht, 1986; G.Kreisel, "Hilbert's Programme", in Paul Benacerraf and Hilary Putnam, eds. *The Philosophy of Mathematics*, 2nd ed., Cambridge, 1983, pp.207-238; P.Kitcher, "Hilbert's Epistemology", *Philosophy of Science*, **43**, 1976, pp.99-115.

[40] Judson C. Webb, *Mechanism, Mentalism and Metamathematics*, Dordrecht, 1980; and Michael Detlefsen, *Hilbert's Program*, Dordrecht, 1986.

be merely a conservative extension of the old, provided we are ready to justify any new inferences not already available in the old. But questions about ideal elements are not to be ruled out of court. We are not, *pace* the Logical Positivists, precluded from asking questions about electrons, quarks, and electromagnetic waves, even though the sense-experience on which our physical theories are based is very different. It may be that ∞ is "carried" by the finite numbers, but questions can be asked all the same. If we moderate the rigour of Hilbert's programme, we can accommodate the successive extensions of mathematical theories in the "bed theory of truth".

§14*.8 The Bed Theory of Truth

In Chapter Two we proved Desargues' theorem for three dimensions, though in two-dimensional projective geometry it cannot be proved and has to taken as an additional axiom. But we are quite sure that it should be taken as an axiom. For one thing, non-Desarguian geometry is, we are told, very messy. But even more important, we think two-dimensional geometry ought to be embeddable in three- and more-dimensional geometry. We generalise. Wherever possible, we see a mathematical proposition, theory, or example, as a special case of something more general with a wider range of applications. We embed theories in larger theories. These theories are often, *pace* Hilbert, not conservative. Propositions which can be expressed in the terminology of the more limited theory turn out to be theorems of the larger theory, though not of the more limited one. We cannot, therefore defend the larger theory as being just a convenience, which enables us to prove in three lines what could be proved otherwise in 515 lines. It proves more. Also embedment goes in a different direction from that of the classical logicist programme. There we justify axioms by showing them to be theorems of a *more basic* theory: here the theory is *more general*.

Often the two directions are intertwined. In the classical logicist programme real numbers are defined as sets of rationals, rationals as equivalence classes of positive rationals, and positive rationals as equivalence classes of natural numbers.[41] But the point of the construction is not to produce complicated collocations of natural numbers, but to *extend* the concept of number to other sorts of

[41] See above, §3.7.

number—rational numbers, negative numbers, real numbers, complex numbers. Although the details of the extension are different in each case, and the conceptual commitments different also, the strategy is the same, and should be compared with the extension of two-dimensional to three-dimensional projective geometry. In the case of geometry, there is a natural generalisation from two to three, and from three to any finite number, and from any finite number to a denumerably infinite number, of dimensions. In the case of numbers there is no parameter, such as dimension number, which invites extension, but rather some ideal of completeness[42] that leads us to extend the concept of number in order to be able always to carry out some operation which hitherto could not be carried out uniformly in every case. It is good to be able to divide a dozen apples among two, three, four, or six people, but what if we are a party of five? Instead of just saying "Five into twelve won't go", or "Five into twelve goes twice with remainder two", it is often better to say that five into twelve is 12/5, and to be able to express this as $2\frac{2}{5}$: equally, if I owe the bank money, instead of their owing me, it is possible to indicate the fact by writing it in red, or typing 'o/d' after the figures in my bank statement; but the use of the minus sign is simpler and easier. It is a more sophisticated requirement that there should always be a square root, not only of perfect squares, like four, nine, sixteen, *etc.*, but of two, three, five, *etc.*; and even more sophisticated that there should be square roots of negative numbers, so that every quadratic, indeed every analytic, equation should have a root. None the less, these requirements are made, and can be met if we extend numbers in suitable ways. And hence we are led not merely to rational and negative numbers, as expounded in §3.7, but to real numbers and complex numbers as well.

But it is not good enough just to require, and then to assume that entities exist meeting our requirements. Bertrand Russell, in a famous phrase, said that the method of postulating entities had all the advantages of theft over honest toil.[43] The jibe strikes home. Once we abandon a hard-line formalist position, we cannot expect to be able simply to lay down by *fiat* what entities there are, and

[42] Used here in a different sense from those of the formal logicians, as in §3.4, §15.5.

[43] Bertrand Russell, *An Introduction to Mathematical Philosophy*, London, 1919, ch. VII, p.71.

say, as it were, "*Volo, ergo est*". Were complex numbers invented or discovered? In either case we owe an account, ultimately of their title to exist, and immediately of how we know that complex number theory is true.

Complex numbers are often defined as ordered pairs of real numbers, with fresh definitions of addition, multiplication, and their inverses. The rules for addition and subtraction are straightforward: the operations on complex numbers are symbolized by large circled signs, which are then defined in terms of ordinary operations on real numbers thus:

$$(a, b) \oplus (c, d) = (a + c, b + d)$$

$$(a, b) \ominus (c, d) = (a - c, b - d)$$

The rule for multiplication is more complicated as regards the second pair of each ordered couple:

$$(a, b) \otimes (c, d) = ((a \times c) - (b \times d), (a \times d) + (b \times c))$$

The rule for division is very complicated, but need not be considered here.

Granted these rules, it follows that in the special case where $b = 0$ and $d = 0$,

$$(a, 0) \oplus (c, 0) = (a + c, 0 + 0) = (a + c, 0)$$

$$(a, 0) \ominus (c, 0) = (a - c, 0 - 0) = (a - c, 0)$$

and

$$\begin{aligned}(a, 0) \otimes (c, 0) &= ((a \times c) - (0 \times 0), (a \times 0) + (0 \times c)) \\ &= (a \times c, 0)\end{aligned}$$

So if we translate

$$\begin{aligned}(a, 0) &\longmapsto a \\ \oplus &\longmapsto + \\ \ominus &\longmapsto - \\ \otimes &\longmapsto \times\end{aligned}$$

we have our familiar real number theory back again. Though we have extended the real numbers, so that now every polynomial equation has a root, we have not really lost anything, because by considering just those complex numbers in which the "imaginary part", that is the second of the ordered pair, is zero, we have an exact replica of the real numbers.

There are thus *two* relations between the newly defined complex numbers and the old real numbers: the theory of complex numbers is a special part of the theory of real numbers; and there is a special part of the theory of complex numbers which is isomorphic to the theory of real numbers. On the standard logicist account the latter relation is just a happy accident. When we are talking about complex numbers, we are really talking about ordered pairs of real numbers with rather a funny multiplication rule, and it just so happens that complex numbers of the form $(a, 0)$ turn out to behave just like real numbers a. But it was no accident. We wanted complex numbers to be an extension of real numbers, and so needed there to be a special part of the theory of complex numbers that was isomorphic to the theory of real numbers. We do not think that $(a, 0)$ is rather like a—we think that it *is* a, only now embedded in a more extended universe, so that we can attach sense to $\sqrt{-1}$, and can say that every number has a square root. The standard account involves us in a lot of double talk, in which we have not only the natural number 1, but the rational number $\frac{1}{1}$, the integer $1 - 0$, the real number that is the Least Upper Bound of all the proper fractions, and the complex number $(1, 0)$, with only a similarity, not complete identity, between them. But it is not just typographical economy that discourages us from distinguishing between them on all occasions of their use; it is also, and more importantly, the sense of an underlying identity of concept in all cases, in spite of the occasionally relevant differences. We do not think that the complex number $(1, 0)$ is really an ordered pair of two least upper bounds of sets of equivalence classes of ordered pairs of equivalence classes of ordered pairs of natural numbers. Such a construction is toilsome, and is not what we want or are prepared to work with.

The virtue of such a construction is, rather, that it proves that the theory of complex numbers is consistent provided the theory of real numbers is. Exactly the same arguments apply as with non-Euclidean geometries.[44] So we have not just postulated entities regardless: we have at least met the first requirement of consistency. Moreover, in virtue of there being a special part of the theory of complex numbers which is isomorphic to the theory of real

[44] See above, §2.3.

numbers the theory of complex numbers can claim to be a genuine *extension* of the theory of real numbers. In constructing a model with ordered pairs of real numbers, we are not just playing about with models, but are carefully creating a bed in which real number theory can be seen as being embedded. The complex operations, $\oplus$, $\ominus$ and $\otimes$, are seen as complex versions of the corresponding $+$, , and $\times$, because when restricted to complex numbers whose imaginary part is zero they are exactly analogous. Hence we are justified as fully as anyone could reasonably demand in our use of similar symbols to express those operations, and in considering the operations on complex numbers to be essentially the same as those on real numbers, while being, when not restricted to complex numbers that lack an imaginary component, complete in the sense that every number, even the analogues of negative real numbers, has a square root, and every polynomial equation has a root. If pressed on the score of consistency or existence, we rely on the complex numbers being modelled within the real numbers, and claim that if the real numbers are consistent then so are the complex numbers, and that if the real numbers exist, then so do ordered pairs of them. If pressed on the use of the same terms for operations on complex numbers as for operations on real numbers we point out the exact analogue between real numbers and the restricted part of complex numbers in which the imaginary component is zero; and completeness follows from the special properties of the model we have made, and justifies us in adopting complex numbers not merely as an extension of real numbers but as one in which certain basic operations can be carried out uniformly and invariably, instead of only under certain conditions.

Greater uniformity has been the criterion in the successive extensions of the concept, and corresponding theory, of number, and has provided the justification of its rules and axioms. We are justifying axioms not by deducing them from some more basic ones, nor by some inductive argument, but by some search for greater generality, in which we generalise not from observation and sense-experience, as in genuine inductive arguments, but in the goals we set ourselves and the *desiderata* we lay down for a mathematical theory. We seek more and more general theories; and if a proposition which cannot be proved in one theory, and has to be postulated as an axiom in that theory, can none the less be proved in another, more general, theory in which the first theory can be embedded, we accept that fact as a good reason for having it as an axiom in the smaller theory.

The bed theory of truth is super-Hilbertian. Like Hilbert, it is formalist, replete with all the rigour that Formalism offers, but not just a free-floating game, because it is *inhältlich*, anchored in the fundamental logic of quotifiers, modal operators and transitive relations. But the bed theory of truth goes beyond Hilbert, and makes stronger claims. It is not confined to conservative extensions of theories, but embraces non-conservative extensions too, in which the proposition in question can be proved only in the extended theory and not in the smaller one. And it claims not merely availability as an optional formal exercise, but truth, because we are led to them by our desire for greater generality in application, or search for greater uniformity in operation.

§14*.9 Mathematical Knowledge

Mathematical knowledge is *a priori*. Plato's argument against Protagoras holds. Mathematical knowledge is not about the world of sensible experience, but about the way we think—the way we have to think, if our thought is to be more than the fleeting consciousness of a subjective self. Mathematical knowledge is knowledge of the way concepts are connected, and is constrained not by experience of the external world, but by our own self-knowledge as agents, reflecting on what we do, and what we can envisage our being able to do in other circumstances, and by necessities of communication and cooperation with other truth-seekers.

Concepts, in being connected with one another, form patterns, and mathematicians recognise these patterns, and point them out to other mathematicians. But pattern-recognition is *not* a form of perception, the patterns being exemplified as much in the way we do things and in the structure of our thinking, as in what we perceive by our senses. It is the force of reason, not factual observation, that compels consent. Although sometimes we accept proofs second-hand, we do not have to, whereas in history and geography, if we were not in the right place at the right time, we have to accept other men's word for how things were. Rather than observe how things happen to be, and what patterns they fit into, the mathematician discerns patterns in themselves and the necessary connexions between them. But although he needs to be a man of discernment, his discerning is not authoritative, as that of a historian or literary critic sometimes is. Mommsen's judgement is to be respected because it is Mommsen's. But we do not believe Fermat's Last Theorem on Fermat's say-so: only if there is a proof, open in principle to any mathematical enquirer, is Fermat's Last Theorem proved. Though we may sometimes accept some mathematics second-hand, it is necessarily the case that we do not necessarily have to. In mathematics, as in the natural sciences, there are no privileged authorities; only, whereas in the natural sciences, universality is secured by experimental observation, which is in principle open to anyone, universal access is secured in mathematics by arguments that cannot be gainsaid by anyone concerned to know the truth.

The requirement that mathematical proof be universally accessible, leads to modified Formalism. We formalise mathematical arguments, though not fully, in order that they shall be effectively incontrovertible. Many of the inferences are deductive inferences which can be formalised in first-order logic, and which effectively reduce the argument to a monologue. But not all are. Some arguments involve quotification over monadic or polyadic predicate variables, expressing qualities or relations, which can only be formulated in second-order logic; some arguments obtain their purchase through a dialogue, sometimes requiring the respondent to move to a meta-level, and view the course of argument as a whole, and reason about it informally. Implicit in our adopting formal systems and use of symbols in accordance with formal rules are the Rules of Substitution and of Generalisation; but implicit in our reasoning about formal systems informally at the meta-level is the principle that reason transcends rules, and cannot be adequately explicated in terms of rule-following alone. We often formalise for good reason, but ought always to acknowledge that formal argument does not exhaust the whole range of rationality. When we formalise, we project into monologous form arguments that are essentially dialectical: and sometimes if we want to understand the rationale of an axiom or principle monologously expressed, we need to go back to the underlying dialogue, where the parties, being rational, can each see the other's point of view, and thus, for instance, recognise the necessity imposed on his choices by the other's possibilities of choosing.

Formal approaches require axioms, but cannot adequately account for them. Mathematicians often cite beauty, harmony and depth as reasons for accepting an axiomatic theory as true. These are good reasons, but we need to be more specific. In Chapter Two a variety of different considerations were adduced in favour of the axioms of Euclidean geometry. The Pythagorean rule was simpler than the corresponding distance rule for non-Euclidean geometries. Euclidean geometry was more non-committal than any other, and therefore better suited to constituting the arena on which various physical theories were compared, and not itself predisposed towards one of the physical theories in issue. There were group-theoretical considerations for preferring the Euclidean group, under which Euclidean geometry is invariant, to any group characteristic of some other geometry. The successive extensions of the concept and theory of number from the natural numbers to the complex numbers was driven by ideals of uniformity and generality. In set theory Platonist and nominalist views about the existence of universals bear on our adopting or rejecting other axioms, and Bolzano and others have adduced further *a priori* arguments in favour of the Axiom of Infinity.

These are cogent considerations; comparable ones operate in the natural sciences. Some thinkers have sought to justify the axioms used in mathematical theories on an analogy with the physical sciences where the axioms are reckoned to be true on account of their consequences being found to agree with observation. Gödel explicitly makes the comparison in the passage quoted in Chapter One,[45] But the analogy is broken-backed. Mathematical intuition is not some sixth sense, and if it were it would not explain the relevance of mathematics to the empirical world observed by our ordinary means of perception.[46] Physical theories in fact are accepted as true not solely because they have been verified empirically, but on grounds of "logicality and harmony"[47] and Field holds:

> ...it is then plausible to argue that considerations other than applications to the physical world, for example, considerations of simplicity and coherence within mathematics, are

[45] §1.5.

[46] The most careful and sympathetic exegesis of Gödel's views is given by P.Maddy in *Realism in Mathematics*, Oxford, 1990, and especially in her *Naturalism in Mathematics*, Oxford, 1996. H.Wang, *A Logical Journey*, Cambridge, Mass., 1996, reports many further discussions with Gödel.

[47] R.C.Tolman, *Relativity, Thermodynamics and Cosmology*, Oxford, 1950, §29, p.53.

> grounds for accepting some proposed mathematics axioms as true and rejecting others as false.[48]

Gödel argues more fully:

> Secondly, however, even disregarding the intrinsic necessity of some new axiom, and even in case it has no intrinsic necessity at all, a probable decision about its truth is possible also in another way, namely, inductively verifying its "success". Success here means fruitfulness in consequences, in particular in "verifiable" consequences, i.e., consequences demonstrable without the new axiom, whose proofs with the help of the new axiom, however, are considerably simpler and easier to discover, and make it possible to contract into one proof many different proofs. The axioms for the system of real numbers, rejected by the intuitionists, have in this sense been verified to some extent, owing to the fact that analytical number theory frequently allows one to prove number-theoretic theorems which, in a more cumbersome way, can subsequently be verified by elementary methods. A much higher degree of verification than that, however, is conceivable. There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field, and yielding such powerful methods for solving problems (and even solving them constructively, as far as that is possible) that, no matter whether or not they are intrinsically necessary, they would have to be established in the same sense as any well-established physical theory.[49]

But once again an important point is being obscured by a false analogy with physical science. We can adduce considerations of simplicity and coherence both for and against various axioms about the size of the set-theoretical universe and the existence of large cardinals. The Axiom of Choice can be held to be actually *true* on the grounds that with it we can prove numerical and other

[48] H.Field, *Science without Numbers*, Princeton, 1980, p.4; quoted by Maddy, in her *Realism in Mathematics*, Oxford, 1990, p.159.

[49] K.Gödel, "What is Cantor's Continuum Problem?", Paul Benacerraf and Hilary Putnam, eds. *The Philosophy of Mathematics*, 2nd edn., Cambridge, 1983, p.477; reprinted in *The Collected Works of Kurt Gödel*, ii, ed. S.Feferman, Oxford, 1990, p.261.

propositions which can be shown to be true without it, though with much greater difficulty.

We can understand Gödel in the light of the two previous sections. He is going beyond Hilbert. He claims a much more full-blooded truth, and, moreover, the extended theory does not have to be conservative. Provided some consequences of an additional axiom are provable without it, and none are false or implausible, we may accept the axiom as true. In the case of the natural sciences, however, the observations on which inductive arguments are based have empirical content, and confer content—though possibly of a markedly different kind—on the generalisations and theories they support. If, like Hilbert, we regarded propositions about the natural numbers as *inhaltlich*, we could then use them to confer content on more *recherché* realms of discourse. But that, *pace* Kronecker, needs to be argued for. It can. The natural numbers can be grounded in the quotifiers, themselves logical constants essential if communication is to take place between intelligent beings. Equally essential are the transitive relations, giving rise to concepts of similarity and equality on the one hand, and to various sorts of order on the other. Not only the inferences, but the content of mathematics is based on the necessities of communication and co-operation among truth-seekers. This is the most important insight of Logicism.

Mathematics has developed from that foundation through a series of more-or-less formal systems having greater and greater generality, coherence and uniformity. If Logicism gives guidance about the foundations of mathematics, our architectonic view of its structure emphasizes the importance of pattern-recognition. And this partial re-instatement of epistemological Platonism raises once again the ontological question, whether the patterns discerned by the mathematician really exist.