Author's introduction

It frequently happens in the history of thought that when a powerful new method emerges the study of those problems which can be dealt with by the new method advances rapidly and attracts the limelight, while the rest tends to be ignored or even forgotten, its study despised.

This situation seems to have arisen in our century in the Philosophy of Mathematics as a result of the dynamic development of metamathematics.

The subject matter of metamathematics is an abstraction of mathematics in which mathematical theories are replaced by formal systems, proofs by certain sequences of well-formed formulae, definitions by 'abbreviatory devices' which are 'theoretically dispensable' but 'typographically convenient'.[1] This abstraction was devised by Hilbert to provide a powerful technique for approaching some of the problems of the methodology of mathematics. At the same time there are problems which fall outside the range of metamathematical abstractions. Among these are all problems relating to informal (*inhaltliche*) mathematics and to its growth, and all problems relating to the situational logic of mathematical problem-solving.

I shall refer to the school of mathematical philosophy which tends to identify mathematics with its formal axiomatic abstraction (and the philosophy of mathematics with metamathematics) as the 'formalist' school. One of the clearest statements of the formalist position is to be found in Carnap [1937]. Carnap demands that (*a*) 'philosophy is to be replaced by the logic of science . . .', (*b*) 'the logic of science is nothing other than the logical syntax of the language of science . . .', (*c*) 'metamathematics is the syntax of mathematical language' (pp. xiii and 9). Or: philosophy of mathematics is to be replaced by metamathematics.

1 Church [1956], I, pp. 76–7. Also cf. Peano [1894], p. 49 and Russell and Whitehead [1910–13], I, p. 12. This is an integral part of the Euclidean programme as formulated in Pascal [1659]: cf. Lakatos [1962], p. 158.

Formalism disconnects the history of mathematics from the philosophy of mathematics, since, according to the formalist concept of mathematics, there is no history of mathematics proper. Any formalist would basically agree with Russell's 'romantically' put but seriously meant remark, according to which Boole's *Laws of Thought* (1854) was 'the first book ever written on mathematics'.[2] Formalism denies the status of mathematics to most of what has been commonly understood to be mathematics, and can say nothing about its growth. None of the 'creative' periods and hardly any of the 'critical' periods of mathematical theories would be admitted into the formalist heaven, where mathematical theories dwell like the seraphim, purged of all the impurities of earthly uncertainty. Formalists, though, usually leave open a small back door for fallen angels: if it turns out that for some 'mixtures of mathematics and something else' we can find formal systems 'which include them in a certain sense', then they too may be admitted (Curry [1951], pp. 56–7). On those terms Newton had to wait four centuries until Peano, Russell, and Quine helped him into heaven by formalising the Calculus. Dirac is more fortunate: Schwartz saved his soul during his lifetime. Perhaps we should mention here the paradoxical plight of the metamathematician: by formalist, or even by deductivist, standards, he is not an honest mathematician. Dieudonné talks about 'the absolute necessity imposed on any mathematician *who cares for intellectual integrity*' (my italics) to present his reasonings in axiomatic form ([1939], p. 225).

Under the present dominance of formalism, one is tempted to paraphrase Kant: the history of mathematics, lacking the guidance of philosophy, has become *blind,* while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become *empty.*

'Formalism' is a bulwark of logical positivist philosophy. According to logical positivism, a statement is meaningful only if it is either 'tautological' or empirical. Since informal mathematics is neither 'tautological' nor empirical, it must be meaningless, sheer nonsense.[3]

2 Russell [1901]. The essay was republished as chapter 5 of Russell's [1918], under the title 'Mathematics and the Metaphysicians'. In the 1953 Penguin edition the quotation can be found on p. 74. In the preface of his [1918] Russell says of the essay: 'Its tone is partly explained by the fact that the editor begged me to make the article "as romantic as possible".'

3 According to Turquette, Gödelian sentences are meaningless ([1950], p. 129). Turquette argues against Copi, who claims that since they are *a priori truths* but not analytic, they refute the analytic theory of *a priori* ([1949] and [1950]). Neither of them notices that the

The dogmas of logical positivism have been detrimental to the *history and philosophy of mathematics.*

The purpose of these essays is to approach some problems of the *methodology of mathematics.* I use the word 'methodology' in a sense akin to Pólya's and Bernays' 'heuristic'[4] and Popper's 'logic of discovery' or 'situational logic'.[5] The recent expropriation of the term 'methodology of mathematics' to serve as a synonym for 'metamathematics' has undoubtedly a formalist touch. It indicates that in formalist philosophy of mathematics there is no proper place for methodology qua logic of discovery.[6]

peculiar status of Gödelian sentences from this point of view is that these theorems are theorems of informal mathematics, and that in fact they are discussing the status of informal mathematics in a particular case.

4 Pólya [1945], especially p. 102, and also [1954], [1962a]; Bernays [1947], esp. p. 187.

5 Popper [1934], then [1945], especially p. 90 (or the fourth edition [1962], p.97); and also [1957], pp. 147 ff.

6 One can illustrate this, e.g. by Tarski [1930a] and Tarski [1930b]. In the first paper Tarski uses the term 'deductive sciences' *explicitly* as a shorthand for 'formalised deductive sciences'. He says: 'Formalised deductive disciplines form the field of research of metamathematics roughly in the same sense in which spatial entities form the field of research in geometry.' This sensible formulation is given an intriguing imperialist twist in the second paper: 'The deductive disciplines constitute the subject-matter of the methodology of the deductive sciences in much the same sense in which spatial entities constitute the subject-matter of geometry and animals that of zoology. Naturally not all deductive disciplines are presented in a form suitable for objects of scientific investigation. Those, for example, are not suitable which do not rest on a definite logical basis, have no precise rules of inference, and the theorems of which are formulated in the usually ambiguous and inexact terms of colloquial language – in a word those which are not formalised. Metamathematical investigations are confined in consequence to the discussion of formalised deductive disciplines.' The innovation is that while the first formulation stated that the subject matter of metamathematics is the formalised deductive disciplines, the second formulation states that the subject-matter of metamathematics is confined to formalised deductive disciplines only because non-formalised deductive sciences are not suitable objects for scientific investigation at all. This implies that the pre-history of a formalised discipline cannot be the subject-matter of a scientific investigation – unlike the pre-history of a zoological species, which can be the subject-matter of a very scientific theory of evolution. Nobody will doubt that some problems about a mathematical theory can only be approached after it has been formalised, just as some problems about human beings (say concerning their anatomy) can only be approached after their death. But few will infer from this that human beings are 'suitable for scientific investigation' only when they are 'presented in "dead" form', and that biological investigations are confined in consequence to the discussion of dead human beings – although, I should not be surprised if some enthusiastic pupil of Vesalius in those glorious days of early anatomy, when the powerful new method of dissection emerged, had identified biology with the analysis of dead bodies.

In the preface of his [1941] Tarski enlarges on his negative attitude towards the possibility of any sort of methodology other than formal systems: 'A course in the methodology of empirical sciences . . . must be largely confined to evaluations and

According to formalists, mathematics is identical with formalised mathematics. But what can one *discover* in a formalised theory? Two sorts of things. First, one can discover the solution to problems which a suitably programmed Turing machine could solve in a finite time (such as: is a certain alleged proof a proof or not?). No mathematician is interested in following out the dreary mechanical 'method' prescribed by such decision procedures. *Secondly*, one can discover the solutions to problems (such as: is a certain formula in a non-decidable theory a theorem or not?), where one can be guided only by the 'method' of 'unregimented insight and good fortune'.

Now this bleak alternative between the rationalism of a machine and the irrationalism of blind guessing does not hold for live mathematics:[7] an investigation of informal mathematics will yield a rich situational logic for working mathematicians, a situational logic which is neither mechanical nor irrational, but which cannot be recognised and still less, stimulated, by the formalist philosophy.

The history of mathematics and the logic of mathematical discovery, i.e. the phylogenesis and the ontogenesis of mathematical thought,[8] cannot be developed without the criticism and ultimate rejection of formalism.

But formalist philosophy of mathematics has very deep roots. It is the latest link in the long chain of *dogmatist* philosophies of mathematics. For more than two thousand years there has been an argument between *dogmatists* and *sceptics.* The dogmatists hold that – by the power of our human intellect and/or senses – we can attain truth and know that we

criticisms of tentative gropings and unsuccessful efforts.' The reason is that empirical sciences are unscientific: for Tarski defines a scientific theory 'as a system of asserted statements arranged according to certain rules' (ibid.).

7 One of the most dangerous vagaries of formalist philosophy is the habit of (1) stating something – rightly – about formal systems; (2) then saying that this applies to 'mathematics' – this is again right if we accept the identification of mathematics and formal systems; (3) subsequently, with a surreptitious shift in meaning, using the term 'mathematics' in the ordinary sense. So Quine says ([1951], p. 87) that 'this reflects the characteristic mathematical situation; the mathematician hits upon his proof by unregimented insight and good fortune, but afterwards other mathematicians can check his proof'. But often the checking of an *ordinary* (informal) proof is a very delicate enterprise, and to hit on a 'mistake' requires as much insight and luck as to hit on a proof: the discovery of 'mistakes' in informal proofs may sometimes take decades – if not centuries.

8 Both H. Poincaré and G. Pólya propose to apply E. Haeckel's 'fundamental biogenetic law' about ontogeny recapitulating phylogeny to mental development, in particular to mathematical mental development. (Poincaré [1908], p. 135, and Pólya [1962b].) To quote Poincaré: 'Zoologists maintain that the embryonic development of an animal recapitulates in brief the whole history of its ancestors throughout geologic time. It seems it is the same in the development of minds . . . For this reason, the history of science should be our first guide' (C. B. Halsted's authorised translation, p. 437).

have attained it. The sceptics on the other hand either hold that we cannot attain the truth at all (unless with the help of mystical experience), or that we cannot know if we can attain it or that we have attained it. In this great debate, in which arguments are time and again brought up to date, mathematics has been the proud fortress of dogmatism. Whenever the mathematical dogmatism of the day got into a 'crisis', a new version once again provided genuine rigour and ultimate foundations, thereby restoring the image of authoritative, infallible, irrefutable mathematics, 'the only Science that it has pleased God hitherto to bestow on mankind' (Hobbes [1651], p. 15). Most sceptics resigned themselves to the impregnability of this stronghold of dogmatist epistemology.[9] A challenge is now overdue.

The core of this case-study will challenge mathematical formalism, but will not challenge directly the ultimate positions of mathematical dogmatism. Its modest aim is to elaborate the point that informal, quasi-empirical, mathematics does not grow through a monotonous increase of the number of indubitably established theorems but through the incessant improvement of guesses by speculation and criticism, by the logic of proofs and refutations. Since, however, metamathematics is a paradigm of informal, quasi-empirical mathematics just now in rapid growth, the essay, by implication, will also challenge modern mathematical dogmatism. The student of recent history of metamathematics will recognise the patterns described here in his own field.

The dialogue form should reflect the dialectic of the story; it is meant to contain a sort of *rationally reconstructed or 'distilled' history. The real history will chime in in the footnotes, most of which are to be taken, therefore, as an organic part of the essay.*

9 For a discussion of the rôle of mathematics in the dogmatist-sceptic controversy, cf. my [1962].

Chapter 1

1. A problem and a conjecture

The dialogue takes place in an imaginary classroom. The class gets interested in a *PROBLEM*: is there a relation between the number of vertices V, the number of edges E and the number of faces F of polyhedra – particularly of *regular polyhedra* – analogous to the trivial relation between the number of vertices and edges of *polygons*, namely, that there are as many edges as vertices: $V = E$? This latter relation enables us to classify *polygons* according to the number of edges (or vertices): triangles, quadrangles, pentagons, etc. An analogous relation would help to classify *polyhedra.*

After much trial and error they notice that for all regular polyhedra $V - E + F = 2$.[1] Somebody *guesses* that this may apply for any polyhedron

1 First noticed by Euler [1758a]. His original problem was the classification of polyhedra, the difficulty of which was pointed out in the editorial summary: 'While in plane geometry polygons (*figurae rectilineae*) could be classified very easily according to the number of their sides, which of course is always equal to the number of their angles, in stereometry the classification of polyhedra (*corpora hedris planis inclusa*) represents a much more difficult problem, since the number of faces alone is insufficient for this purpose.'

The key to Euler's result was just the invention of the concepts of *vertex* and *edge*: it was he who first pointed out that besides the number of faces the number of *points* and *lines* on the surface of the polyhedron determines its (topological) character. It is interesting that on the one hand he was eager to stress the novelty of his conceptual framework, and that he had to invent the term *'acies'* (edge) instead of the old *'latus'* (side), since *latus* was a polygonal concept while he wanted a polyhedral one, on the other hand he still retained the term *'angulus solidus'* (solid angle) for his point-like vertices. It has been recently generally accepted that the priority of the result goes to Descartes. The ground for this claim is a manuscript of Descartes [c. 1639] copied by Leibniz in Paris from the original in 1675–6, and rediscovered and published by Foucher de Careil in 1860. The priority should not be granted to Descartes without a minor qualification. It is true that Descartes states that the number of plane angles equals 2□ +2α-4 where by □ he means the number of faces and by α the number of solid angles. It is also true that he states that there are twice as many plane angles as edges (*latera*). The conjunction of these two statements of course yields the Euler formula. But Descartes did not see the point of doing so, since he still thought in terms of angles (plane and solid) and faces, and did not make a conscious revolutionary change to the concepts of 0-dimensional vertices, 1-dimensional edges and 2-dimensional faces as a necessary and sufficient basis for the full topological characterisation of polyhedra.

whatsoever. Others try to falsify this *conjecture*, try to test it in many different ways – it holds good. The results *corroborate* the conjecture, and suggest that it could be *proved.* It is at this point – after the stages *problem* and *conjecture* – that we enter the classroom.[2] The teacher is just going to offer a *proof.*

2. A proof

TEACHER: In our last lesson we arrived at a conjecture concerning polyhedra, namely, that for all polyhedra $V - E + F = 2$, where V is the number of vertices, E the number of edges and F the number of faces. We tested it by various methods. But we haven't yet proved it. Has anybody found a proof?

PUPIL SIGMA: 'I for one have to admit that I have not yet been able to devise a strict proof of this theorem . . . As however the truth of it has been established in so many cases, there can be no doubt that it holds good for any solid. Thus the proposition seems to be satisfactorily demonstrated.'[3] But if you have a proof, please do present it.

TEACHER: In fact I have one. It consists of the following thought-experiment. *Step 1:* Let us imagine the polyhedron to be hollow, with a surface made of thin rubber. If we cut out one of the faces, we can stretch the remaining surface flat on the blackboard, without tearing it. The faces and edges will be deformed, the edges may become curved, but V and E will not alter, so that if and only if $V - E + F = 2$ for the original polyhedron, $V - E + F = 1$ for this flat network – remember that we have removed one face. (Fig. 1 shows the flat network for the case of a cube.) Step *2:* Now we triangulate our map – it does indeed look like a geographical map. We draw (possibly curvilinear) diagonals in those (possibly curvilinear) polygons which are not already (possibly curvilinear) triangles. By drawing each diagonal we increase both E and F by one, so that the total $V - E + F$ will not be altered (fig. 2).

2 Euler tested the conjecture quite thoroughly for consequences. He checked it for prisms, pyramids and so on. He could have added that the proposition that there are only five regular bodies is also a consequence of the conjecture. Another suspected consequence is the hitherto corroborated proposition that four colours are sufficient to colour a map. The phase of *conjecturing* and *testing* in the case of $V - E + F = 2$ is discussed in Pólya ([1954], vol. 1, the first five sections of the third chapter, pp. 35–41). Pólya stopped here, and does not deal with the phase of *proving* – though of course he points out the need for a heuristic of 'problems to prove' ([1945], p. 144). Our discussion starts where Pólya stops.

3 Euler ([1758a], p. 119 and p. 124). But later ([1758b]) he proposed a proof.

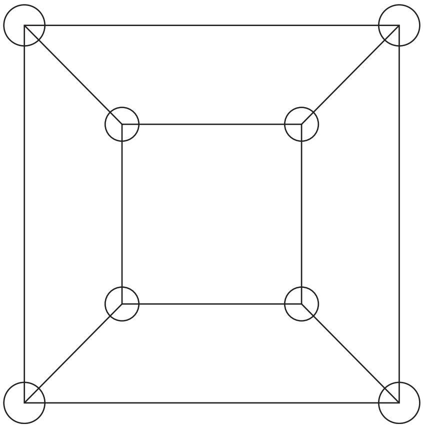

Fig. 1.

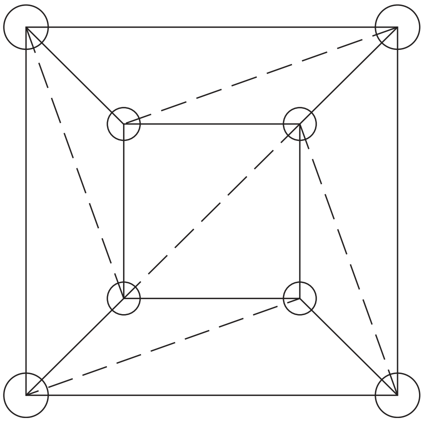

Fig. 2.

Step 3: From the triangulated network we now remove the triangles one by one. To remove a triangle we either remove an edge – upon which one face and one edge disappear (fig. 3(a)), or we remove two edges and a vertex-upon which one face, two edges and one vertex disappear (fig. 3(b)). Thus if $V - E + F = 1$ before a triangle is removed, it remains so after the triangle is removed. At the end of this procedure we get a single triangle. For this $V - E + F = 1$ holds true. Thus we have proved our conjecture.[4]

4 This proof-idea stems from Cauchy [1813a].

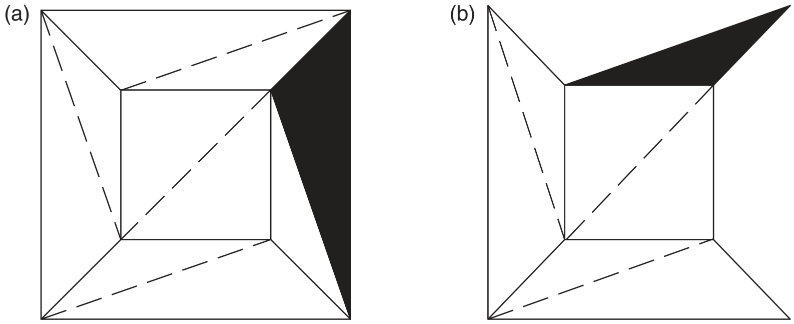

Fig. 3.

PUPIL DELTA: You should now call it a *theorem*. There is nothing conjectural about it any more.[5]

PUPIL ALPHA: I wonder. I see that this experiment can be performed for a cube or for a tetrahedron, but how am I to know that it can be performed for *any* polyhedron? For instance, are you sure, Sir, that *any polyhedron, after having a face removed, can be stretched flat on the blackboard*? I am dubious about your first step.

PUPIL BETA: Are you sure that in *triangulating the map one will always get a new face for any new edge*? I am dubious about your second step.

PUPIL GAMMA: Are you sure that *there are only two alternatives – the disappearance of one edge or else of two edges and a vertex – when one drops the triangles one by one*? Are you even sure that *one is left with a single triangle at the end of this process*? I am dubious about your third step.[6]

TEACHER: Of course I am not sure.

ALPHA: But then we are worse off than before! Instead of one conjecture we now have at least three! And this you call a 'proof'!

TEACHER: I admit that the traditional name 'proof' for this thought-experiment may rightly be considered a bit misleading. I do not think that it establishes the truth of the conjecture.

5 Delta's view that this proof has established the 'theorem' beyond doubt was shared by many mathematicians in the nineteenth century, e.g. Crelle [1826–7], 2, pp. 668–71, Matthiessen [1863], p. 449, Jonquières [1890a] and [1890b]. To quote a characteristic passage: 'After Cauchy's proof, it became absolutely indubitable that the elegant relation $V + F = E + 2$ applies to all sorts of polyhedra, just as Euler stated in 1752. In 1811 all indecision should have disappeared.' Jonquières [1890a], pp. 111–12.

6 The class is a rather advanced one. To Cauchy, Poinsot, and to many other excellent mathematicians of the nineteenth century these questions did not occur.

DELTA: What does it do then? What do you think a mathematical proof proves?

TEACHER: This is a subtle question which we shall try to answer later. Till then I propose to retain the time-honoured technical term 'proof' for a *thought-experiment – or 'quasi-experiment' – which suggests a decomposition of the original conjecture into subconjectures or lemmas*, thus *embedding it* in a possibly quite distant body of knowledge. Our 'proof', for instance, has embedded the original conjecture – about crystals, or, say, solids – in the theory of rubber sheets. Descartes or Euler, the fathers of the original conjecture, certainly did not even dream of this.[7]

3. Criticism of the proof by counterexamples which are local but not global

TEACHER: This decomposition of the conjecture suggested by the proof opens new vistas for testing. The decomposition deploys the conjecture

7 Thought-experiment (*deiknymi*) was the most ancient pattern of mathematical proof. It prevailed in pre-Euclidean Greek mathematics (cf. Á. Szabó [1958]).

That conjectures (or theorems) precede proofs in the heuristic order was a commonplace for ancient mathematicians. This followed from the heuristic precedence of *'analysis'* over *'synthesis'*. (For an excellent discussion see Robinson [1936].) According to Proclus, '. . . it is . . . necessary to know beforehand what is sought' (Heath [1925], 1, p. 129). 'They said that a theorem is that which is proposed with a view to the demonstration of the very thing proposed' – says Pappus (ibid. 1, p. 10). The Greeks did not think much of propositions which they happened to hit upon in the deductive direction without having previously guessed them. They called them *porisms*, corollaries, incidental results springing from the proof of a theorem or the solution of a problem, results not directly sought but appearing, as it were, by chance, without any additional labour, and constituting, as Proclus says, a sort of windfall (*ermaion*) or bonus (*kerdos*) (ibid. 1, p. 278). We read in the editorial summary to Euler [1756–7] that arithmetical theorems 'were discovered long before their truth has been confirmed by rigid demonstrations'. Both the Editor and Euler use for this process of discovery the modern term *'induction'* instead of the ancient *'analysis'* (ibid.). The heuristic precedence of the result over the argument, of the theorem over the proof, has deep roots in mathematical folklore. Let us quote some variations on a familiar theme: Chrysippus is said to have written to Cleanthes: 'Just send me the theorems, then I shall find the proofs' (cf. Diogenes Laertius [c. 200], VII. 179). Gauss is said to have complained: 'I have had my results for a long time; but I do not yet know how I am to arrive at them' (cf. Arber [1945], p. 47), and Riemann: 'If only I had the theorems! Then I should find the proofs easily enough.' (Cf. Hölder [1924], p. 487.) Pólya stresses: 'You have to guess a mathematical theorem before you prove it' ([1954], vol. 1, p. vi).

The term *'quasi-experiment'* is from the above-mentioned editorial summary to Euler [1753]. According to the Editor: 'As we must refer the numbers to the pure intellect alone, we can hardly understand how observations and *quasi-experiments* can be of use in investigating the nature of the numbers. Yet, in fact, as I shall show here with very good reasons, the properties of the numbers known today have been mostly discovered by observation . . .' (Pólya's translation; in his [1954], 1, p. 3 he mistakenly attributes the quotation to Euler).