

The British Journal for the Philosophy of Science

VOLUME XIV

May, 1963

No. 53

PROOFS AND REFUTATIONS (I) *†

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For George Pólya's 75th and Karl Popper's 60th birthday

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* Received 3.x.60

† This paper was written in 1958-59 at King's College, Cambridge. It was first read in Karl Popper's seminar in London in March 1959. The paper was then mimeographed and widely circulated. An improved version has been included in the author's Cambridge Ph.D. thesis prepared under Professor R. B. Braithwaite's supervision (*Essays in the Logic of Mathematical Discovery*, 1961). The author also received much help, encouragement and valuable criticism from Dr T. J. Smiley. The thesis would not have been written but for the generous help of the Rockefeller Foundation.

When preparing this latest version at the London School of Economics the author tried to take note especially of the criticisms and suggestions of Dr J. Agassi, Dr I. Hacking, Professors W. C. Kneale and R. Montague, A. Musgrave, Professor M. Polányi and J. W. N. Watkins. The treatment of the exception-barring method was improved under the stimulus of the critical remarks of Professors G. Pólya and B. L. van der Waerden. The distinction between the methods of monster-barring and monster-adjustment was suggested by B. MacLennan.

The paper should be seen against the background of Pólya's revival of mathematical heuristic, and of Popper's critical philosophy.

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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 metamathematical 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].² 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 . . . ',

¹ Church [1956] I, pp. 76-77. Also cf. Peano [1894], p. 49 and Whitehead-Russell [1910-13], I, p. 12. This is an integral part of the Euclidean programme as formulated by Pascal [1657-58]: cf. Lakatos [1962], p. 158.

² For full details of this and similar references see the list of works at the end of Part III of this article.

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(c) ‘metamathematics is the syntax of mathematical language’ (pp. xiii and 9). Or: philosophy of mathematics is to be replaced by metamathematics.

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’.¹ 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. 56–57). 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 ‘tautological’ or empirical. Since informal mathematics is neither

¹ B. Russell [1901]. The essay was republished as Chapter V 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 [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”’.

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'tautological' nor empirical, it must be meaningless, sheer nonsense.¹ 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'² and Popper's 'logic of discovery' or 'situational logic'.³ 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.⁴ According to formalists,

¹ 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 peculiar status of Gödelian sentences from this point of view is that these theorems are theorems of informal mathematics, and that in fact they discuss the status of informal mathematics in a particular case. Neither do they notice that theorems of informal mathematics are surely guesses, which one can hardly classify dogmatist-wise as '*a priori*' and '*a posteriori*' guesses.

² Pólya [1945], especially p. 102, and also [1954], [1962]; Bernays [1947], esp. p. 187

³ Popper [1934], then [1945], especially p. 90 (or the fourth edition [1962] p. 97); and also [1957], pp. 147 ff.

⁴ 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

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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:¹ 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,

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 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.*).

¹ 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* 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.

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i.e. the phylogenesis and the ontogenesis of mathematical thought,¹ 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 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.² 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.

¹ 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).

² For a discussion of the rôle of mathematics in the dogmatist-sceptic controversy, cf. my [1962].

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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.*

I 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

¹ First noticed by Euler [1750]. 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 [*ca.* 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 $2\phi + 2a - 4$ where by ϕ he means the number of faces and by a the number of solid angles. It is also true that he states that there are twice as many plane angles as edges (*latera*). The trivial 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.

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Polyhedron 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.¹ 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.'² 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 , E and F will not alter, so that if and only if $V - E + F = 2$ for the original polyhedron, then $V - E + F = 1$ 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

¹ 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. I, 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.

² Euler ([1750], p. 119 and p. 124). But later [1751] he proposed a proof.

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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). *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. 3a), or we remove two edges and a vertex—upon which one face, two edges and one vertex disappear (Fig. 3b). Thus if $V - E + F = 1$

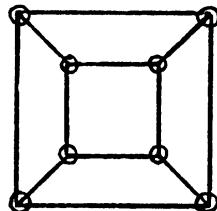


FIG. 1

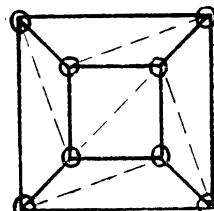


FIG. 2

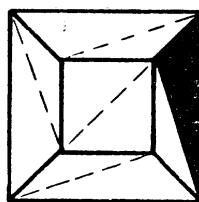


FIG. 3a

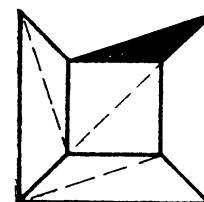


FIG. 3b

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.¹

PUPIL DELTA: You should now call it a *theorem*. There is nothing conjectural about it any more.²

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.

¹ This proof-idea stems from Cauchy [1811].

² Delta's view that this proof has established the 'theorem' beyond doubt was shared by many mathematicians in the nineteenth century, e.g. Crelle [1826-27], II, pp. 668-671, 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. 111-112.

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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.¹

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.

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.²

¹ 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.

² 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], I, 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.* I, 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.* I, p. 278). We read in the editorial summary to Euler [1753] that arithmetical theorems ‘were discovered

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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 on a wider front, so that our criticism has more targets. We now have at least three opportunities for counterexamples instead of one!

GAMMA: I already expressed my dislike of your third lemma (viz. that in removing triangles from the network which resulted from the stretching and subsequent triangulation, we have only two possibilities: either we remove an edge or we remove two edges and a vertex). I suspect that other patterns may emerge when removing a triangle.

TEACHER: Suspicion is not criticism.

GAMMA: Then is a *counterexample* criticism?

TEACHER: Certainly. Conjectures ignore dislike and suspicion, but they cannot ignore counterexamples.

THETA (*aside*): Conjectures are obviously very different from those who represent them.

GAMMA: I propose a trivial counterexample. Take the triangular network which results from performing the first two operations on a cube (Fig. 2). Now if I remove a triangle from the *inside* of this network, as one might take a piece out of a jigsaw puzzle, I remove one triangle without removing a single edge or vertex. So the third lemma

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 [ca. 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 [1954], 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. I, 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; he mistakenly attributes the quotation to Euler in his [1954], I, p. 3).

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is false—and not only in the case of the cube, but for *all* polyhedra except the tetrahedron, in the flat network of which all the triangles are boundary triangles. Your proof thus proves the Euler theorem for the tetrahedron. But we already *knew* that $V - E + F = 2$ for the tetrahedron, so why prove it?

TEACHER: You are right. But notice that the cube which is a counterexample to the third lemma is not also a counterexample to the main conjecture, since for the cube $V - E + F = 2$. You have shown the poverty of the argument—the proof—but not the falsity of our conjecture.

ALPHA: Will you scrap your proof then?

TEACHER: No. Criticism is not necessarily destruction. I shall improve my proof so that it will stand up to the criticism.

GAMMA: How?

TEACHER: Before showing how, let me introduce the following terminology. I shall call a '*local counterexample*' an example which refutes a lemma (without necessarily refuting the main conjecture), and I shall call a '*global counterexample*' an example which refutes the main conjecture itself. Thus your counterexample is local but not global. A local, but not global, counterexample is a criticism of the proof, but not of the conjecture.

GAMMA: So, the conjecture may be true, but your proof does not prove it.

TEACHER: But I can easily elaborate, *improve the proof*, by replacing the false lemma by a slightly modified one, which your counterexample will not refute. I no longer contend that *the removal of any triangle follows one of the two patterns mentioned*, but merely that *at each stage of the removing operation the removal of any boundary triangle follows one of these patterns*. Coming back to my thought-experiment, all that I have to do is to insert a single word in my third step, to wit, that '*from the triangulated network we now remove the boundary triangles one by one*'. You will agree that it only needed a trifling observation to put the proof right.¹

GAMMA: I do not think your observation was so trifling; in fact it was quite ingenious. To make this clear I shall show that it is false. Take the flat network of the cube again and remove eight of the ten

¹ Lhuilier, when correcting in a similar way a proof of Euler, says that he made only a '*trifling observation*' ([1812-13], p. 179). Euler himself, however, gave the proof up, since he noticed the trouble but could not make that '*trifling observation*'.

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triangles in the order given in Fig. 4. At the removal of the eighth triangle, which is certainly by then a boundary triangle, we removed two edges and no vertex—this changes $V - E + F$ by 1. And we are left with the two disconnected triangles 9 and 10.

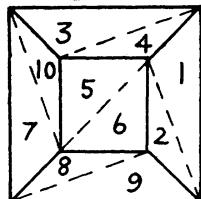


FIG. 4

TEACHER: Well, I might save face by saying that I meant by a boundary triangle a triangle whose removal does not disconnect the network. But intellectual honesty prevents me from making surreptitious changes in my position by sentences starting with ‘I meant . . .’ so I admit that now I must *replace* the second version of the triangle-removing operation with a third version: that we remove the triangles one by one in such a way that $V - E + F$ does not alter.

KAPPA: I generously agree that the lemma corresponding to this operation is true: namely, that if we remove the triangles one by one in such a way that $V - E + F$ does not alter, then $V - E + F$ does not alter.

TEACHER: No. The lemma is that *the triangles in our network can be so numbered that in removing them in the right order $V - E + F$ will not alter till we reach the last triangle.*

KAPPA: But how should one construct this right order, if it exists at all?¹ Your original thought-experiment gave the instructions: remove the triangles in any order. Your modified thought-experiment gave the instruction: remove boundary triangles in any order. Now you say we should follow a definite order, but you do not say which and whether that order exists at all. Thus the thought-experiment breaks down. You improved the proof-analysis, i.e. the list of lemmas; but the thought-experiment which you called ‘the proof’ has disappeared.

RHO: Only the third step has disappeared.

¹ Cauchy thought that the instruction to find at each stage a triangle which can be removed either by removing two edges and a vertex or one edge can be trivially carried out for any polyhedron ([1811], p. 79). This is of course connected with his inability to imagine a polyhedron that is not homeomorphic with the sphere.

I. LAKATOS

KAPPA: Moreover, did you *improve* the lemma? Your first two simple versions at least looked trivially true before they were refuted; your lengthy, patched up version does not even look plausible. Can you really believe that it will escape refutation?

TEACHER: ‘Plausible’ or even ‘trivially true’ propositions are usually soon refuted: sophisticated, implausible conjectures, matured in criticism, might hit on the truth.

OMEGA: And what happens if even your ‘sophisticated conjectures’ are falsified and if this time you cannot replace them by unfalsified ones? Or, if you do *not* succeed in improving the argument further by local patching? You have succeeded in getting over a local counterexample which was not global by replacing the refuted lemma. What if you do not succeed next time?

TEACHER: Good question—it will be put on the agenda for tomorrow.

4. Criticism of the Conjecture by Global Counterexamples

ALPHA: I have a counterexample which will falsify your first lemma—but this will also be a counterexample to the main conjecture, i.e. this will be a global counterexample as well.

TEACHER: Indeed! Interesting. Let us see.

ALPHA: Imagine a solid bounded by a pair of nested cubes—a pair of cubes, one of which is inside, but does not touch the other (Fig. 5).

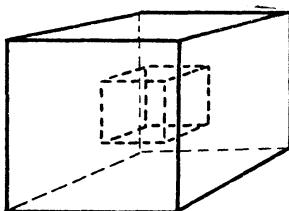


FIG. 5

This hollow cube falsifies your first lemma, because on removing a face from the inner cube, the polyhedron will not be stretchable on to a plane. Nor will it help to remove a face from the outer cube instead. Besides, for each cube $V - E + F = 2$, so that for the hollow cube $V - E + F = 4$.

TEACHER: Good show. Let us call it *Counterexample 1*.¹ Now what?

¹ This *Counterexample 1* was first noticed by Lhuilier ([1812-13], p. 194). But Gergonne, the Editor, added (p. 186) that he himself noticed this long before Lhuilier’s

PROOFS AND REFUTATIONS (I)

(a) Rejection of the conjecture. The method of surrender

GAMMA: Sir, your composure baffles me. A single counterexample refutes a conjecture as effectively as ten. The conjecture and its proof have completely misfired. Hands up! You have to surrender. Scrap the false conjecture, forget about it and try a radically new approach.

TEACHER: I agree with you that the *conjecture* has received a severe criticism by Alpha's counterexample. But it is untrue that the *proof* has 'completely misfired'. If, for the time being, you agree to my earlier proposal to use the word 'proof' for a 'thought-experiment which leads to decomposition of the original conjecture into sub-conjectures', instead of using it in the sense of a 'guarantee of certain truth', you need not draw this conclusion. My proof certainly proved Euler's conjecture in the first sense, but not necessarily in the second. You are interested only in proofs which 'prove' what they have set out to prove. I am interested in proofs even if they do not accomplish their intended task. Columbus did not reach India but he discovered something quite interesting.

ALPHA: So according to your philosophy—while a local counterexample (if it is not global at the same time) is a criticism of the proof, but not of the conjecture—a global counterexample is a criticism of the conjecture, but not necessarily of the proof. You agree to surrender as regards the conjecture, but you defend the proof. But if the conjecture is false, what on earth does the proof prove?

GAMMA: Your analogy with Columbus breaks down. Accepting a global counterexample must mean total surrender.

(b) Rejection of the counterexample. The method of monster-barring

DELTA: But why accept the counterexample? We proved our conjecture—now it is a theorem. I admit that it clashes with this so-called 'counterexample'. One of them has to give way. But why should the theorem give way, when it has been proved? It is the 'criticism' that should retreat. It is fake criticism. This pair of paper. Not so Cauchy, who published his proof just a year before. And this counterexample was to be rediscovered twenty years later by Hessel ([1832], p. 16). Both Lhuilier and Hessel were led to their discovery by mineralogical collections in which they noticed some double crystals, where the inner crystal is not translucent, but the outer is. Lhuilier acknowledges the stimulus of the crystal collection of his friend Professor Pictet ([1812-13], p. 188). Hessel refers to lead sulphide cubes enclosed in translucent calcium fluoride crystals ([1832], p. 16).

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nested cubes is not a polyhedron at all. It is a *monster*, a pathological case, not a counterexample.

GAMMA: Why not? *A polyhedron is a solid whose surface consists of polygonal faces.* And my counterexample is a solid bounded by polygonal faces.

TEACHER: Let us call this definition *Def. 1*.¹

DELTA: Your definition is incorrect. A polyhedron must be a *surface*: it has faces, edges, vertices, it can be deformed, stretched out on a blackboard, and has nothing to do with the concept of ‘solid’. *A polyhedron is a surface consisting of a system of polygons.*

TEACHER: Call this *Def. 2*.²

DELTA: So really you showed us *two* polyhedra—*two* surfaces, one completely inside the other. A woman with a child in her womb is not a counterexample to the thesis that human beings have one head.

ALPHA: So! My counterexample has bred a new concept of polyhedron. Or do you dare to assert that by polyhedron you *always* meant a surface?

TEACHER: For the moment let us accept Delta’s *Def. 2*. Can you refute our conjecture now if by polyhedron we mean a surface?

ALPHA: Certainly. Take two tetrahedra which have an edge in common (Fig. 6a). Or, take two tetrahedra which have a vertex in common (Fig. 6b). Both these twins are connected, both constitute one single surface. And, you may check that for both $V - E + F = 3$

TEACHER: *Counterexamples 2a and 2b*.³

¹ *Definition 1* occurs first in the eighteenth century; e.g.: ‘One gives the name *polyhedral solid*, or simply *polyhedron*, to any solid bounded by planes or plane faces’ (Legendre [1794], p. 160). A similar definition is given by Euler ([1750]). Euclid, while defining cube, octahedron, pyramid, prism, does not define the general term polyhedron, but occasionally uses it (e.g. Book XII, Second Problem, Prop. 17).

² We find *Definition 2* implicitly in one of Jonquières’ papers read to the French Academy against those who meant to refute Euler’s theorem. These papers are a thesaurus of monsterbarring techniques. He thunders against Lhuilier’s monstrous pair of nested cubes: ‘Such a system is not really a polyhedron, but a pair of distinct polyhedra, each independent of the other. . . . A polyhedron, at least from the classical point of view, deserves the name only if, before all else, a point can move continuously over its entire surface; here this is not the case . . . This first exception of Lhuilier can therefore be discarded’ ([1890b], p. 170). This definition—as opposed to *Definition 1*—goes down very well with analytical topologists who are not interested at all in the theory of polyhedra as such but as a handmaiden for the theory of surfaces.

³ *Counterexamples 2a and 2b* were missed by Lhuilier and first discovered only by Hessel ([1832], p. 13).

PROOFS AND REFUTATIONS (I)

DELTA: I admire your perverted imagination, but of course I did not mean that *any* system of polygons is a polyhedron. By polyhedron I meant *a system of polygons arranged in such a way that (1) exactly two polygons meet at every edge and (2) it is possible to get from the inside of any polygon to the inside of any other polygon by a route which never crosses any edge at a vertex*. Your first twins will be excluded by the first criterion in my definition, your second twins by the second criterion.

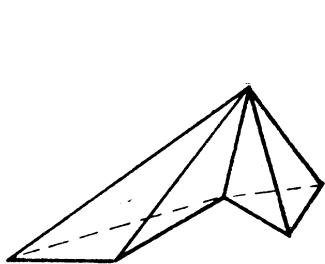


FIG. 6a

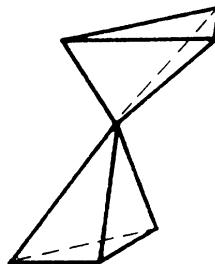


FIG. 6b

TEACHER: *Def. 3.*¹

ALPHA: I admire your perverted ingenuity in inventing one definition after another as barricades against the falsification of your pet ideas. Why don't you just define a polyhedron as a system of polygons for which the equation $V - E + F = 2$ holds, and this Perfect Definition. . . .

KAPPA: *Def. P.*²

ALPHA: . . . would settle the dispute for ever? There would be no need to investigate the subject any further.

DELTA: But there isn't a theorem in the world which couldn't be falsified by monsters.

¹ *Definition 3* first turns up to keep out twintetrahedra in Möbius ([1865], p. 32). We find his cumbersome definition reproduced in some modern textbooks in the usual authoritarian 'take it or leave it' way; the story of its monsterbarring background—that would at least explain it—is not told (e.g. Hilbert-Cohn Vossen [1956], p. 290).

² *Definition P* according to which Eulerianness would be a definitional characteristic of polyhedra was in fact suggested by R. Baltzer: 'Ordinary polyhedra are occasionally (following Hessel) called Eulerian polyhedra. It would be more appropriate to find a special name for non-genuine (*uneigentliche*) polyhedra' ([1860], Vol. II, p. 207). The reference to Hessel is unfair: Hessel used the term 'Eulerian' simply as an abbreviation for polyhedra for which Euler's relation holds in contradistinction to the non-Eulerian ones ([1832], p. 19). For *Def. P* see also the Schläfli quotation in footnote pp. 18–19.

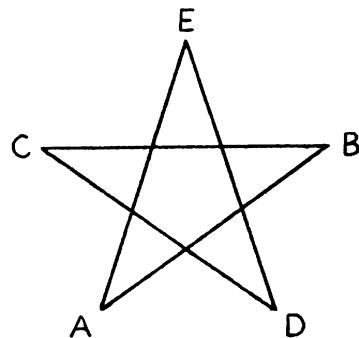
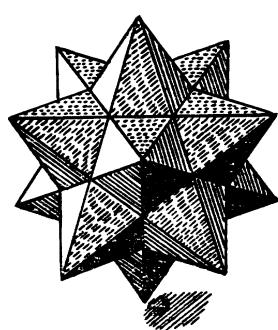
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TEACHER: I am sorry to interrupt you. As we have seen, refutation by counterexamples depends on the meaning of the terms in question. If a counterexample is to be an objective criticism, we have to agree on the meaning of our terms. We *may* achieve such an agreement by defining the term where communication broke down. I, for one, didn't define 'polyhedron'. I assumed *familiarity* with the concept, i.e. the ability to distinguish a thing which is a polyhedron from a thing which is not a polyhedron—what some logicians call knowing the extension of the concept of polyhedron. It turned out that the extension of the concept wasn't at all obvious: *definitions are frequently proposed and argued about when counterexamples emerge*. I suggest that we now consider the rival definitions together, and leave until later the discussion of the differences in the results which will follow from choosing different definitions. Can anybody offer something which even the most restrictive definition would allow as a real counterexample?

KAPPA: Including Def. P?

TEACHER: Excluding Def. P.

GAMMA: I can. Look at this *Counterexample 3*: a star-polyhedron—I shall call it *urchin* (Fig. 7). This consists of 12 star-pentagons (Fig. 8). It has 12 vertices, 30 edges, and 12 pentagonal faces—you



Figs. 7 and 8. Kepler (Fig. 7) shaded each face in a different way to show which triangles belong to the same pentagonal face.

may check it if you like by counting. Thus the Descartes-Euler thesis is not true at all, since for this polyhedron $V - E + F = -6$.¹

¹ The 'urchin' was first discussed by Kepler in his cosmological theory ([1619], *Lib. II*, XIX and XXVI, on p. 52 and p. 60 and *Lib. V*, *Cap. I*, p. 182, *Cap. III*, p. 187 and *Cap. IX*, XLVII). The name 'urchin' is Kepler's ('*cui nomen Echino feci*'). Fig. 7 is copied from his book (p. 52) which contains also another picture on p. 182.

PROOFS AND REFUTATIONS (I)

DELTA: Why do you think that your ‘ urchin ’ is a polyhedron?

GAMMA: Do you not see? This is a polyhedron, whose faces are the twelve star-pentagons. It satisfies your last definition: it is ‘ a system of polygons arranged in such a way that (1) exactly two polygons meet at every edge, and (2) it is possible to get from every polygon to every other polygon without ever crossing a vertex of the polyhedron ’.

DELTA: But then you do not even know what a polygon is! A star-pentagon is certainly not a polygon! *A polygon is a system of edges arranged in such a way that (1) exactly two edges meet at every vertex, and (2) the edges have no points in common except the vertices.*

TEACHER: Let us call this *Def. 4*.

GAMMA: I don’t see why you include the second clause. The right definition of the polygon should contain the first clause only.

TEACHER: *Def. 4'*.

GAMMA: The second clause has nothing to do with the essence of a polygon. Look: if I lift an edge a little, the star-pentagon is already a polygon even in your sense. You imagine a polygon to be drawn in chalk on the blackboard, but you should imagine it as a wooden structure: then it is clear that what you think to be a point in common is not really one point, but two different points lying one above the other. You are misled by your embedding the polygon in a plane—you should let its limbs stretch out in space!¹

Poinsot independently rediscovered it, and it was he who pointed out that the Euler formula did not apply to it ([1809], p. 48). The now standard term ‘ small stellated polyhedron ’ is Cayley’s ([1859], p. 125). Schläfli admitted star-polyhedra in general, but nevertheless rejected our small stellated dodecahedron as a monster. According to him ‘ this is not a genuine polyhedron, for it does not satisfy the condition $V - E + F = 2$ ’ ([1852], § 34).

¹ The dispute whether polygon should be defined so as to include star-polygons or not (*Def. 4* or *Def. 4'*) is a very old one. The argument put forward in our dialogue—that star-polygons can be embedded as ordinary polygons in a space of higher dimensions—is a modern topological argument, but one can put forward many others. Thus Poinsot defending his star-polyhedra argued for the admission of star-polygons with arguments taken from analytical geometry: ‘ . . . all these distinctions (between “ ordinary ” and “ star ”-polygons) are more apparent than real, and they completely disappear in the analytical treatment, in which the various species of polygons are quite inseparable. To the edge of a regular polygon there corresponds an equation with real roots, which simultaneously yields the edges of all the regular polygons of the same order. Thus it is not possible to obtain the edges of a regular inscribed heptagon, without at the same time finding edges of heptagons of the second and third species. Conversely, given the edge of a regular heptagon, one may determine the

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DELTA: Would you mind telling me what is the *area* of a star-pentagon? Or would you say that some polygons have no area?

GAMMA: Was it not you yourself who said that a polyhedron has nothing to do with the idea of solidity? Why now suggest that the idea of polygon should be linked with the idea of area? We agreed that a polyhedron is a closed surface with edges and vertices—then why not agree that a polygon is simply a closed curve with vertices? But if you stick to your idea I am willing to define the area of a star-polygon.¹

TEACHER: Let us leave this dispute for a moment, and proceed as before. Consider the last two definitions together—*Def.* 4 and *Def.* 4'. Can anyone give a counterexample to our conjecture that will comply with *both* definitions of polygons?

ALPHA: Here is one. Consider a *picture-frame* like this (Fig. 9). This is a polyhedron according to any of the definitions hitherto proposed. Nonetheless you will find, on counting the vertices, edges and faces, that $V - E + F = 0$.

radius of a circle in which it can be inscribed, but in so doing, one will find three different circles corresponding to the three species of heptagon which may be constructed on the given edge; similarly for other polygons. Thus we are justified in giving the name “ polygon ” to these new starred figures ([1809], p. 26). Schröder uses the Hankelian argument: ‘The extension to rational fractions of the power concept originally associated only with the integers has been very fruitful in Algebra; this suggests that we try to do the same thing in geometry whenever the opportunity presents itself . . .’ ([1862], p. 56). Then he shows that we may find a geometrical interpretation for the concept of p/q -sided polygons in the star-polygons.

¹ Gamma's claim that he can define the area for star-polygons is not a bluff. Some of those who defended the wider concept of polygon solved the problem by putting forward a wider concept of the area of polygon. There is an especially obvious way to do this in the case of regular star-polygons. We may take the area of a polygon as the sum of the areas of the isosceles triangles which join the centre of the inscribed or circumscribed circle to the sides. In this case, of course, some ‘portions’ of the star-polygon will count more than once. In the case of irregular polygons where we have not got any one distinguished point, we may still take any point as origin and treat negatively oriented triangles as having negative areas (Meister [1769-70], p. 179). It turns out—and this can certainly be expected from an ‘area’—that the area thus defined will not depend on the choice of the origin (Möbius [1827], p. 218). Of course there is liable to be a dispute with those who think that one is not justified in calling the number yielded by this calculation an ‘area’; though the defenders of the Meister-Möbius definition called it ‘the right definition’ which ‘alone is scientifically justified’ (R. Haussner's notes [1906], pp. 114-115). Essentialism has been a permanent feature of definitional quarrels.

PROOFS AND REFUTATIONS (I)

TEACHER: *Counterexample 4.*¹

BETA: So that's the end of our conjecture. It really is a pity, since it held good for so many cases. But it seems that we have just wasted our time.

ALPHA: Delta, I am flabbergasted. You say nothing? Can't you define this new counterexample out of existence? I thought there was no hypothesis in the world which you could not save from falsification with a suitable linguistic trick. Are you giving up now? Do you agree at last that there exist non-Eulerian polyhedra? Incredible!

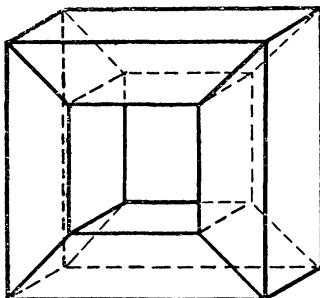


FIG. 9

DELTA: You should really find a more appropriate name for your non-Eulerian pests and not mislead us all by calling them 'polyhedra'. But I am gradually losing interest in your monsters. I turn in disgust from your lamentable 'polyhedra', for which Euler's beautiful theorem doesn't hold.² I look for order and harmony in mathematics, but you only propagate anarchy and chaos.³ Our attitudes are irreconcilable.

¹ We find *Counterexample 4* too in Lhuilier's classical [1812-13], on p. 185—Gergonne again added that he knew it. But Grunert did not know it fourteen years later ([1827]) and Poinsot forty-five years later ([1858], p. 67).

² This is paraphrased from a letter of Hermite's written to Stieltjes: 'I turn aside with a shudder of horror from this lamentable plague of functions which have no derivatives' ([1893]).

³ 'Researches dealing with . . . functions violating laws which one hoped were universal, were regarded almost as the propagation of anarchy and chaos where past generations had sought order and harmony' (Saks [1933], Preface). Saks refers here to the fierce battles of monsterbarrings (like Hermite!) and of refutationists that characterised in the last decades of the nineteenth century (and indeed in the beginning of the twentieth) the development of modern real function theory, 'the branch of mathematics which deals with counterexamples' (Munroe [1953], Preface). The similarly fierce battle that raged later between the opponents and protagonists of modern mathematical logic and set-theory was a direct continuation of this. See also footnote 2 on p. 24 and 1 on p. 25.

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ALPHA: You are a real old-fashioned Tory! You blame the wickedness of anarchists for the spoiling of your 'order' and 'harmony', and you 'solve' the difficulties by verbal recommendations.

TEACHER: Let us hear the latest rescue-definition.

ALPHA: You mean the latest linguistic trick, the latest contraction of the concept of 'polyhedron'! Delta dissolves real problems, instead of solving them.

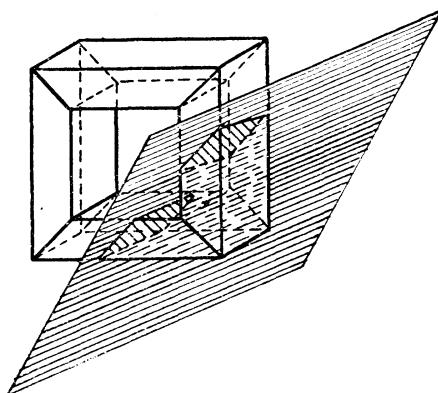


FIG. 10

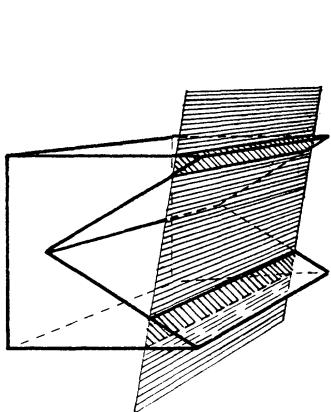


FIG. 11a

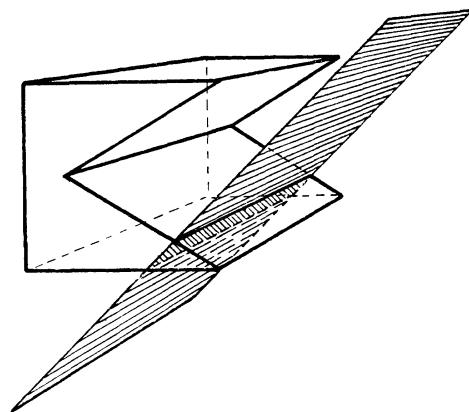


FIG. 11b

DELTA: I do not *contract* concepts. It is you who *expand* them. For instance, this picture-frame is not a genuine polyhedron at all.

ALPHA: Why?

DELTA: Take an arbitrary point in the 'tunnel'—the space bounded by the frame. Lay a plane through this point. You will find that any such plane has always *two* different cross-sections with the

PROOFS AND REFUTATIONS (I)

picture-frame, making two distinct, completely disconnected polygons! (Fig. 10).

ALPHA: So what?

DELTA: *In the case of a genuine polyhedron, through any arbitrary point in space there will be at least one plane whose cross-section with the polyhedron will consist of one single polygon.* In the case of convex polyhedra all planes will comply with this requirement, wherever we take the point. In the case of ordinary concave polyhedra some planes will have more intersections, but there will always be some that have only one. (Figs. 11a and 11b.) In the case of this picture-frame, if we take the point in the tunnel, all the planes will have two cross-sections. How then can you call this a polyhedron?

TEACHER: This looks like another definition, this time an *implicit* one. Call it *Def. 5.*¹

ALPHA: A series of counterexamples, a matching series of definitions, definitions that are alleged to contain nothing new, but to be merely new revelations of the richness of that one old concept, which seems to have as many ‘hidden’ clauses as there are counterexamples. *For all polyhedra $V - E + F = 2$ seems unshakable, an old and ‘eternal’ truth.* It is strange to think that once upon a time it was a wonderful guess, full of challenge and excitement. Now, because of your weird shifts of meaning, it has turned into a poor convention, a despicable piece of dogma. (*He leaves the classroom.*)

DELTA: I cannot understand how an able man like Alpha can waste his talent on mere heckling. He seems engrossed in the production of monstrosities. But monstrosities never foster growth, either in the world of nature or in the world of thought. Evolution always follows an harmonious and orderly pattern.

GAMMA: Geneticists can easily refute that. Have you not heard that mutations producing monstrosities play a considerable role in macroevolution? They call such monstrous mutants ‘hopeful

¹ *Definition 5* was put forward by the indefatigable monsterbarrer E. de Jonquières to get Lhuilier’s polyhedron with a tunnel (picture-frame) out of the way: ‘Neither is this polyhedral complex a true polyhedron in the ordinary sense of the word, for if one takes any plane through an arbitrary point inside one of the tunnels which pass right through the solid, the resulting cross-section will be composed of two distinct polygons completely unconnected with each other; this can occur in an ordinary polyhedron for *certain* positions of the intersecting plane, namely in the case of some concave polyhedra, but not for all of them’ ([1890b], pp. 170–171). One wonders whether de Jonquières has noticed that his *Def. 5* excludes also some concave spheroid polyhedra.

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monsters'. It seems to me that Alpha's counterexamples, though monsters, are 'hopeful monsters'.¹

DELTA: Anyway, Alpha has given up the struggle. No more monsters now.

GAMMA: I have a new one. It complies with all the restrictions in Defs. 1, 2, 3, 4, and 5, but $V - E + F = 1$. This *Counterexample 5* is a simple cylinder. It has 3 faces (the top, the bottom and the jacket), 2 edges (two circles) and no vertices. It is a polyhedron according to your definition: (1) exactly two polygons at every edge and (2) it is possible to get from the inside of any polygon to the inside of any other polygon by a route which never crosses any edge at a vertex. And you have to accept the faces as genuine polygons, as they comply with your requirements: (1) exactly two edges meet at every vertex and (2) the edges have no points in common except the vertices.

DELTA: Alpha stretched concepts, but you tear them! Your 'edges' are not edges! *An edge has two vertices!*

TEACHER: *Def. 6?*

GAMMA: But why deny the status of 'edge' to edges with one or possibly zero vertices? You used to contract concepts, but now you mutilate them so that scarcely anything remains!

DELTA: But don't you see the futility of these so-called refutations? 'Hitherto, when a new polyhedron was invented, it was for some practical end; today they are invented expressly to put at fault the reasonings of our fathers, and one never will get from them anything more than that. Our subject is turned into a teratological museum where decent ordinary polyhedra may be happy if they can retain a very small corner.'

¹ 'We must not forget that what appears to-day as a monster will be to-morrow the origin of a line of special adaptations. . . . I further emphasized the importance of rare but extremely consequential mutations affecting rates of decisive embryonic processes which might give rise to what one might term hopeful monsters, monsters which would start a new evolutionary line if fitting into some empty environmental niche' (Goldschmidt [1933], pp. 544 and 547). My attention was drawn to this paper by Karl Popper.

² Paraphrased from Poincaré ([1908], pp. 131-132). The original full text is this: 'Logic sometimes makes monsters. Since half a century we have seen arise a crowd of bizarre functions which seem to try to resemble as little as possible the honest functions which serve some purpose. No longer continuity, or perhaps continuity, but no derivatives, etc. Nay more, from the logical point of view, it is these strange functions which are the most general, those one meets without seeking no longer appear except as particular cases. There remains for them only a very small corner.'

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GAMMA: I think that if we want to learn about anything really deep, we have to study it not in its ‘normal’, regular, usual form, but in its critical state, in fever, in passion. If you want to know the normal healthy body, study it when it is abnormal, when it is ill. If you want to know functions, study their singularities. If you want to know ordinary polyhedra, study their lunatic fringe. This is how one can carry mathematical analysis into the very heart of the subject.¹ But even if you were basically right, don’t you see the futility of your *ad hoc* method? If you want to draw a borderline between counter-examples and monsters, you cannot do it in fits and starts.

TEACHER: I think we should refuse to accept Delta’s strategy for dealing with global counterexamples, although we should congratulate him on his skilful execution of it. We could aptly label his method *the method of monsterbarring*. Using this method one can eliminate any counterexample to the original conjecture by a sometimes deft but always *ad hoc* redefinition of the polyhedron, of its defining terms, or of the defining terms of its defining terms. We should somehow treat counterexamples with more respect, and not stubbornly exorcise them by dubbing them monsters. Delta’s main mistake is perhaps his dogmatist bias in the interpretation of mathematical proof: he thinks that a proof necessarily proves what it has set out to prove. My interpretation of proof will allow for a *false* conjecture to be ‘proved’, i.e. to be decomposed into subconjectures. If the conjecture is false, I certainly expect at least one of the subconjectures to be false. But the decomposition might still be interesting! I am not perturbed at finding a counterexample to a ‘proved’ conjecture; I am even willing to set out to ‘prove’ a false conjecture!

THETA: I don’t follow you.

KAPPA: He just follows the New Testament: ‘Prove all things; hold fast that which is good’ (1 Thessalonians 5: 21).

(to be continued)

‘Heretofore when a new function was invented, it was for some practical end; to-day they are invented expressly to put at fault the reasonings of our fathers, and one never will get from them anything more than that.

‘If logic were the sole guide of the teacher, it would be necessary to begin with the most general functions, that is to say with the most bizarre. It is the beginner that would have to be set grappling with this teratological museum . . .’ (G. B. Halsted’s authorised translation, pp. 435–436). Poincaré discusses the problem with respect to the situation in the theory of real functions—but that does not make any difference.

¹ Paraphrased from Denjoy ([1919], p. 21).