Read the section on definitions and "monsters" on p14-23 below

I

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 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

[1] First noticed by Euler [1758*a*]. 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 $2\phi+2\alpha-4$ where by ϕ 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.

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 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). *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,

[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. 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.

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

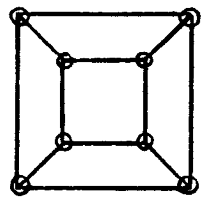


Fig. 1.

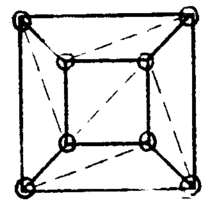


Fig. 2.

(a) Fig. 3. (b)

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.

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.[3]

TEACHER: Of course I am not sure.

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

[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 [1826–7], 2, pp. 668–71, Matthiessen [1863], p. 449, Jonquières [1890*a*] and [1890*b*]. 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 [1890*a*], pp. 111–12.

[3] 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.

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.[1]

[1] 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 Proclos, '...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).

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 have 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 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

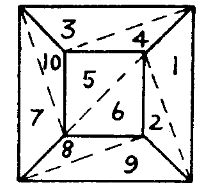


Fig. 4.

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, and *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 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.

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

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

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 instruction: 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.

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.

[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 ([1813*a*], p. 79). This is connected with his inability to imagine a polyhedron that is not homeomorphic with the sphere.

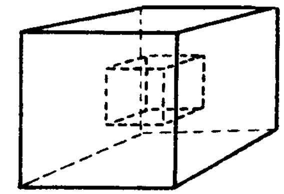


Fig. 5.

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). 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?

(*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

[1] This *Counterexample 1* was first noticed by Lhuilier ([1812–13*a*], p. 194). But Gergonne the Editor, added (p. 186) that he himself noticed this long before Lhuilier's 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*a*], p. 188). Hessel refers to lead sulphide cubes enclosed in translucent calcium fluoride crystals ([1832], p. 16).

subconjectures', 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.

Start here

(*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 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]

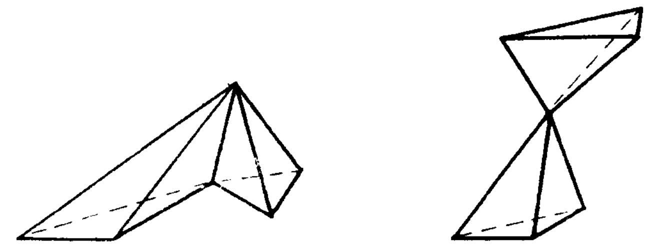
DELTA: Your definition is incorrect. A polyhedron must be a *surface*: it has faces, edges, vertices, it can be deformed, stretched out on a blackboard, and has nothing to do with the concept of 'solid'. *A polyhedron is a surface consisting of a system of polygons.*

TEACHER: Call this *Def. 2*.[2]

DELTA: So really you showed us *two* polyhedra – *two* surfaces, one

[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 [1809], p. 160). A similar definition is given by Euler ([1758*a*]). 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 monster-barring techniques. He thunders against Lhuilier's monstrous pair of nested



(*a*) Fig. 6. (*b*)

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. 6(*a*)). Or, take two tetrahedra which have a vertex in common (fig. 6(*b*)). 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*.[1]

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.

TEACHER: *Def. 3*.[2]

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' ([1890*b*], 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 only as a handmaiden for the theory of surfaces.

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

[2] *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 monster-barring background – that would at least explain it – is not told (e.g. Hilbert and Cohn-Vossen [1956], p. 290).

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? This Perfect Definition...

Kappa: *Def. P.*[1]

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.

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 an *urchin* (fig. 7). This consists of 12 star-pentagons (fig. 8). It has 12 vertices, 30 edges, and 12 pentagonal faces – you may check it if you like by counting. Thus the Descartes–Euler thesis is not true at all, since for this polyhedron $V-E+F = -6$.[2]

1 *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' ([1862], vol. 2, 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 2 below.

2 The 'urchin' was first discussed by Kepler in his cosmological theory ([1619], *Lib.* II, XIX and XXVI, on p. 72 and pp. 82–3 and *Lib.* V, *Cap.* I, p. 293, *Cap.* III, p. 299 and *Cap.* IX, XLVII). The name 'urchin' is Kepler's ('*cui nomen Echino feci*'). Fig. 7 is copied

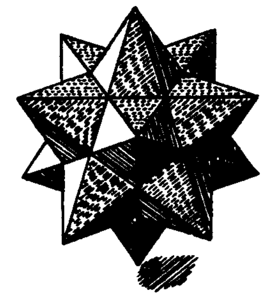


Fig. 7. Kepler's star-polyhedron, each face shaded in a different way to show which triangles belong to the same pentagonal face.

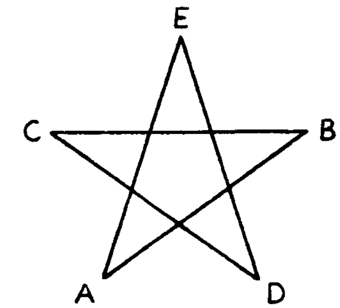


Fig. 8.

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 correct 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

from his book (p. 79) which contains also another picture on p. 293. Poinsot independently rediscovered it, and it was he who pointed out that the Euler formula did not apply to it ([1810], p. 48). The now standard term 'small stellated dodecahedron' 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).

other. You are misled by your embedding the polygon in a plane – you should let its limbs stretch out in space![1]

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.[2]

TEACHER: Let us leave this dispute for a moment, and proceed as before. Consider the last two definitions together – *Def. 4* and *Def. 4'*.

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 become ordinary polygons when embedded 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 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' ([1810], 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.

2 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 [1771], 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. 114–15). Essentialism has been a permanent feature of definitional quarrels.

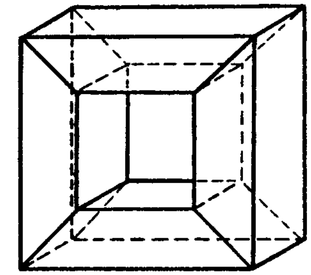


Fig. 9.

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 $V-E+F = 0$.

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!

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*a*], on p. 185 – Gergonne again added that he knew it. But Grunert did not know it fourteen years later ([1827]) nor did 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

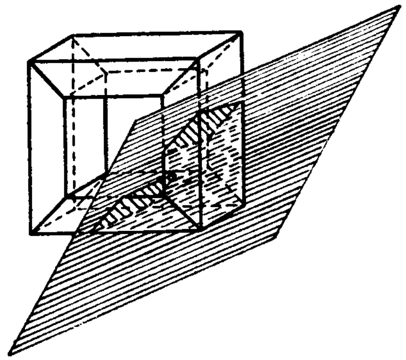
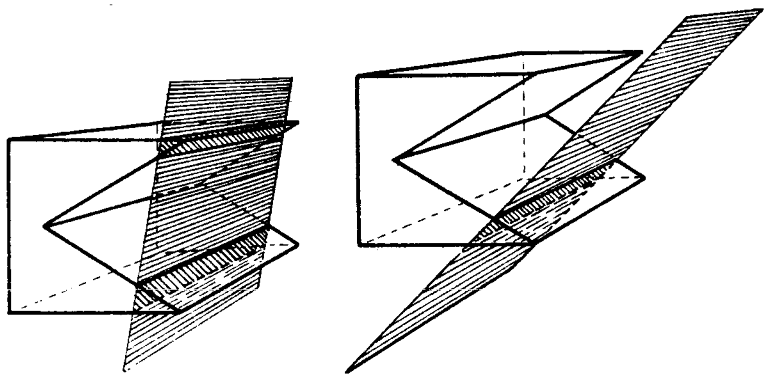


Fig. 10.



(*a*) Fig. 11. (*b*)

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.

battles between monster-barrers (like Hermite!) and 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 footnotes 2, p. 22, and 1, p. 23.

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 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 (fig. 11, (*a*) and (*b*)). 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.

[1] *Definition 5* was put forward by the indefatigable monster-barrer 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' ([1890*b*], pp. 170–1). One wonders whether de Jonquières has noticed that his *Def. 5* excludes also some concave spheroid polyhedra.

GAMMA: Geneticists can easily refute that. Have you not heard that mutations producing monstrosities play a considerable role in macro-evolution? They call such monstrous mutants 'hopeful 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.'[2]

[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. 131–2). 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 case. There remains for them only a very small corner.

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 counterexamples 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 monster-barring*. 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).

'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 teratologic museum...' (G. B. Halsted's authorised translation, pp. 435–6). 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).

end here

2-2

(c) Improving the conjecture by exception-barring methods. Piecemeal exclusions. Strategic withdrawal or playing for safety

BETA: I suppose, sir, you are going to explain your puzzling remarks. But, with all apologies for my impatience, I must get this off my chest.

TEACHER: Go on.

(ALPHA *re-enters.*)

BETA: I find some aspects of Delta's arguments silly, but I have come to believe that there is a reasonable kernel to them. It now seems to me that no conjecture is generally valid, but only valid in a certain restricted domain that excludes the *exceptions*. I am against dubbing these exceptions 'monsters' or 'pathological cases'. That would amount to the methodological decision not to consider these as interesting *examples* in their own right, worthy of a separate investigation. But I am also against the term '*counterexample*'; it rightly admits them as examples on a par with the supporting examples, but somehow paints them in war-colours, so that, like Gamma, one panics when facing them, and is tempted to abandon beautiful and ingenious proofs altogether. No: they are just *exceptions*.

SIGMA: I could not agree more. The term 'counterexample' has an aggressive touch and offends those who have invented the proofs. 'Exception' is the right expression. 'There are three sorts of mathematical propositions:

'1. Those which are always true and to which there are neither restrictions nor exceptions, e.g. the angle sum of all plane triangles is always equal to two right angles.

'2. Those which rest on some false principle and so cannot be admitted in any way.

'3. Those which, although they hinge on true principles, nevertheless admit restrictions or exceptions in certain cases...'

EPSILON: What?

SIGMA: '...One should not confuse false theorems with theorems subject to some restriction.'[1] As the proverb says: *The exception proves the rule.*

EPSILON (*to* KAPPA): Who is this muddlehead? He should learn something about logic.

KAPPA (*to* EPSILON): And about non-Euclidean plane triangles.

DELTA: I find it embarrassing to have to predict that in this discussion Alpha and I shall probably be on the same side. We both argued on the basis of a proposition's being either true or false and disagreed only on whether the Euler theorem, in particular, is true or false. But Sigma wants us to admit a third category of propositions that are 'in principle' true but 'admit exceptions in certain cases'. To agree to a peaceful coexistence of theorems and exceptions means to yield to confusion and chaos in mathematics.

ALPHA: *D'accord.*

ETA: I did not want to interfere with the brilliant argumentation of Delta, but now I think it may be profitable if I briefly explain the story of *my* intellectual development. In my schooldays I became – as you would put it – a monster-barrer, not as a defence against Alpha-types but as a defence against Sigma-types. I remember reading in a periodical about the Euler theorem: 'Brilliant mathematicians have put forward proofs of the general validity of the theorem. Nevertheless it suffers exceptions...it is necessary to draw attention to these exceptions since even recent authors do not always recognise them explicitly.'[1] This paper was not an isolated exercise in diplomacy. 'Although in geometry textbooks and lectures it is always pointed out that Euler's beautiful theorem $V+F = E+2$ is subject to "restriction" in some cases, or "does not seem to be valid", one does not learn the real reason for these exceptions.'[2] Now I looked at the 'exceptions' very carefully and I came to the conclusion that they do not comply with the true definition of the entities in question. So the proof and the theorem can be reinstated and the chaotic coexistence of theorems and exceptions vanishes.

ALPHA: Sigma's chaotic position may serve as an explanation for your monster-barring, but not as an excuse, let alone a justification. Why not eliminate the chaos by accepting the credentials of the counterexample and rejecting the 'theorem' and the 'proof'?

[1] Bérard [1818–19], p. 347 and p. 349.

[1] Hessel [1832], p. 13. Hessel rediscovered Lhuilier's 'exceptions' in 1832. Just after submitting his manuscript he came across Lhuilier's [1812–13*a*]. He nevertheless decided not to withdraw the paper, most of whose results thus turned out to have already been published, because he thought that the point should be driven home to the 'recent authors' ignoring these exceptions. One of these authors, by the way, happened to be the Editor of the Journal to which Hessel submitted the paper: A. L. Crelle. In his [1826–7] textbook he 'proved' that Euler's theorem was true for *all* polyhedra (vol. 2, pp. 668–71).

[2] Matthiessen ([1863], p. 449). Matthiessen refers here to Heis and Eschweiler's *Lehrbuch der Geometrie* and to Grunert's *Lehrbuch der Stereometrie*. Matthiessen however does not solve the problem – like Eta – by monster-barring, but – like Rho – by monster-adjustment (cf. footnote 2, p. 38).

ETA: Why should I reject the proof? I cannot see anything wrong with it. Can you? My monster-barring seems more rational to me than your proof-barring.

TEACHER: This debate showed that monster-barring may get a more sympathetic audience when it stems from Eta's dilemma. But let us come back to Beta and Sigma. It was Beta who rechristened the counterexamples exceptions. Sigma agreed with Beta...

BETA: I am glad that Sigma agreed with me, but I am afraid that I cannot agree with him. There are certainly three types of propositions: true ones, hopelessly false ones and hopefully false ones. This last type can be improved into true propositions by adding a restrictive clause which states the exceptions. I never 'attribute to formulae an undetermined domain of validity. In reality most of the formulae are true only if certain conditions are fulfilled. By determining these conditions and, of course, pinning down precisely the meaning of the terms I use, I make all uncertainty disappear.'[1] So, as you see, I do not advocate any sort of peaceful coexistence between unimproved formulae and exceptions. I improve my formulae and turn them into *perfect* ones, like those in Sigma's first class. This means that I *accept* the method of monster-barring in so far as it serves for finding *the domain of validity of the original conjecture*; I *reject* it in so far as it functions as a linguistic trick for rescuing 'nice' theorems by restrictive concepts. These two functions of Delta's method should be kept separate. I should like to baptise *my* method, which is characterised by the first of these functions only, '*the exception-barring method*'. I shall use it to determine precisely the domain in which the Euler conjecture holds.

TEACHER: What is the 'precisely determined domain' of Eulerian polyhedra you promised? What is your 'perfect formula'?

BETA: *For all polyhedra that have no cavities (like the pair of nested cubes) and tunnels (like the picture-frame)*, $V-E+F = 2$.

TEACHER: Are you sure?

BETA: Yes, I am sure.

TEACHER: What about the twintetrahedra?

BETA: I am sorry. *For all polyhedra that have no cavities, tunnels or 'multiple structure'*, $V-E+F = 2$.[2]

[1] This is from Cauchy's introduction to his celebrated [1821].

[2] Lhuilier and Gergonne seem to have been sure that Lhuilier's list had enumerated all the exceptions. We read in the introduction to this part of the paper: 'One will easily be convinced that Euler's Theorem is true in general, for all polyhedra, whether they are convex or not, except for those instances that will be specified...' (Lhuilier [1812–13*a*], p. 177). Then we read again in Gergonne's comment: '...the specified exceptions which seem to be the only ones that can occur...' (ibid. p. 188). *But in fact Lhuilier missed the twintetrahedra, which were only noticed twenty years later by Hessel* ([1832]). That some leading mathematicians, even mathematicians with a lively interest in methodology like Gergonne, could believe that one could rely upon the exception-barring method, is noteworthy. The belief is analogous to the 'method of division' in inductive logic, according to which there can be a complete enumeration of possible explanations of a phenomenon, and therefore if we can eliminate all but one by the method of *experimentum crucis*, then this last one is proved.

TEACHER: I see. I agree with your policy of improving the conjecture instead of just taking or leaving it. I prefer it both to the method of monster-barring and to that of surrender. However, I have two objections. *First* I contend that your claim that your method not only improves, but 'perfects' the conjecture, that it 'renders it strictly correct', that 'it makes all uncertainties disappear' is untenable.

BETA: Indeed?

TEACHER: You must admit that each new version of your conjecture is only an *ad hoc* elimination of a counterexample which has just cropped up. When you stumble upon nested cubes you exclude polyhedra with *cavities*. When you happen to notice a picture-frame, you exclude polyhedra with *tunnels*. I appreciate your open and observant mind; to take notice of these exceptions is all very well, but I think it would be worth while to inject some method into your blind groping for 'exceptions'. It is good to admit that 'All polyhedra are Eulerian' is only a conjecture. But why give 'All polyhedra without cavities, tunnels and what not are Eulerian' the status of a theorem that is not conjectural any more? How can you be sure that you have enumerated *all* exceptions?

BETA: Can you give one that I did not take into account?

ALPHA: What about my urchin?

GAMMA: And my cylinder?

TEACHER: I do not even need a concrete new 'exception' for my argument. My argument was for the *possibility* of further exceptions.

BETA: You may well be right. One should not just shift one's position whenever a new counterexample turns up. One should not say: 'If no exception occur from phenomena, the conclusion may be pronounced generally. But if at any time afterwards any exception should occur, it may then begin to be pronounced with such exceptions as occur.'[1] Let me think. We first guessed that for *all* polyhedra $V-E+F = 2$, because we found it to be true for cubes, octahedra, pyramids, and prisms. We certainly cannot accept 'this miserable way

[1] I. Newton [1717], p. 380.

of inferring from the special to the general'.[1] No wonder exceptions cropped up; it is rather surprising that many more were not found much earlier. To my mind this was because we were mostly occupied with *convex* polyhedra. As soon as other polyhedra entered, our generalisations did not work any more.[2] So instead of barring exceptions piecemeal, I shall draw the borderline modestly, but safely: *All convex polyhedra are Eulerian.*[3] And I hope you will grant that this has nothing conjectural about it: that it is a theorem.

GAMMA: What about my cylinder? It is convex!

BETA: It is a joke!

TEACHER: Let us forget about the cylinder for the moment. We can offer some criticism even without the cylinder. In this new, modified version of the exception-barring method, which Beta devised so briskly in answer to my criticism, piecemeal withdrawal has been replaced by a strategic retreat into a domain hoped to be a stronghold of the conjecture. You are playing for safety. But are you as safe as you claim to be? You still have no guarantee that there will not be any exceptions inside your stronghold. Besides, there is the opposite danger. Could you have withdrawn too radically, leaving lots of Eulerian polyhedra outside the walls? Our original conjecture might

1 Abel [1826*a*]. His criticism seems to be directed against Eulerian inductivism.

2 This too is paraphrased from the quoted letter, in which Abel was concerned to eliminate the exceptions to general 'theorems' about functions and thereby establish absolute rigour. The original text (including the previous quotation) is this: 'In Higher Analysis very few propositions are proved with definitive rigour. *One finds everywhere the miserable way of inferring from the special to the general,* and it is a marvel that such procedure leads only rarely to what are called paradoxes. It is really very interesting to look for the reason. In my opinion the reason is to be found in the fact that *analysts have been mostly occupied with functions that can be expressed as power series. As soon as other functions enter – which certainly is rarely the case – one does not get on any more* and as soon as one starts drawing false conclusions, an infinite multitude of mistakes will follow, all supporting each other...' (my italics). Poinsot discovered that inductive generalisations 'often' break down in the theory of polyhedra, just as in number theory: 'Most properties are individual and do not obey any general laws' ([1810], § 45). The intriguing characteristic of this caution towards induction is that it puts down its occasional breakdown to the fact that the universe (of facts, numbers, polyhedra) of course contains miraculous exceptions.

3 This again is very much in keeping with Abel's method. In the same way Abel restricted the domain of suspect theorems about functions to power-series. In the story of the Euler conjecture this restriction to convex polyhedra was fairly common. Legendre, for instance, after giving his rather general definition of polyhedron (cf. footnote 1, p. 14), presents a proof which on the one hand certainly does not apply to all his general polyhedra, but on the other hand applies to more than convex ones. Nevertheless, in an additional note, in fine print (an afterthought after having stumbled on exceptions never stated?), he withdraws, modestly but safely, to convex polyhedra ([1809], pp. 161, 164, 228).

have been an overstatement, but your 'perfected' thesis looks to me very much like an understatement; yet you still cannot be sure that it is not an overstatement as well.

But I should also like to put forward my *second* objection: your argument forgets about the proof; in guessing the domain of validity of the conjecture, you do not seem to need the proof at all. Surely you do not believe that proofs are redundant?

BETA: I have never said that.

TEACHER: No, you did not. But you discovered that our proof did not prove our original conjecture. Does it prove your improved conjecture? Tell me.

BETA: Well...[1]

ETA: Thank you, sir, for this argument. Beta's embarrassment clearly displays the superiority of the defamed monster-barring method. For we say that the proof proves what it has set out to prove and our answer is unequivocal. We do not allow wayward counterexamples to destroy respectable proofs at liberty, even if they are disguised as meek 'exceptions'.

BETA: I do not find it embarrassing at all that I have to elaborate, improve, and – excuse me, sir – *perfect* my methodology on the stimulus of criticism. My answer is this. I reject the original conjecture as false because there are exceptions to it. I also reject the proof because the same exceptions are exceptions to at least one of the lemmas. (In your terminology this would be: a global counterexample is necessarily also a local counterexample.) Alpha would stop at this point since refutations seem to satisfy his intellectual needs completely. But I go on. By suitably restricting *both* conjecture and proof to the proper domain, I

1 Many working mathematicians are puzzled about what proofs are for if they do not prove. On the one hand they know from experience that proofs are fallible but on the other hand they know from their dogmatist indoctrination that *genuine* proofs must be infallible. *Applied mathematicians* usually solve this dilemma by a shamefaced but firm belief that the proofs of the *pure mathematicians* are 'complete', and so *really* prove. Pure mathematicians, however, know better – they have such respect only for the 'complete proofs' of *logicians*. If asked what is then the use, the function, of their 'incomplete proofs', most of them are at a loss. For instance, G. H. Hardy had a great respect for the logicians' demand for formal proofs, but when he wanted to characterise mathematical proof 'as we working mathematicians are familiar with it', he did it in the following way: 'There is strictly speaking no such thing as mathematical proof; we can, in the last analysis, do nothing but point;...proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate the imagination of pupils' ([1928], p. 18). R. L. Wilder thinks that a proof is 'only a testing process that we apply to suggestions of our intuition' ([1944], p. 318). G. Pólya points out that proofs, even if incomplete, establish connections between mathematical facts and this helps us to keep them in our memory: proofs yield a mnemotechnic system ([1945], pp. 190–1).

perfect the *conjecture* which will now be *true*, and perfect the basically sound *proof* which will now be *rigorous* and will obviously contain no more false lemmas. For instance we saw that not all polyhedra can be stretched flat onto a plane after having a face removed. But all *convex* polyhedra can. I can rightly call my perfected and rigorously proved conjecture a *theorem*. I state it again: '*All convex polyhedra are Eulerian.*' For convex polyhedra all the lemmas will be manifestly true and the proof, which was not rigorous in its false generality, will be rigorous for the restricted domain of convex polyhedra. So, sir, I have answered your question.

TEACHER: So the lemmas, which once looked manifestly true before the exception was discovered, will again look manifestly true... until the discovery of the next exception. You admit that 'All polyhedra are Eulerian' was guesswork; you admitted just now that 'All polyhedra without cavities and tunnels are Eulerian' was also guesswork; why not admit that 'All convex polyhedra are Eulerian' is guesswork once again!

BETA: Not '*guesswork*' this time, but *insight*!

TEACHER: I abhor your pretentious 'insight'. I respect conscious *guessing*, because it comes from the best human qualities: courage and modesty.

BETA: I proposed a theorem: 'All convex polyhedra are Eulerian.' You offered only a sermon against it. Could you offer a counterexample?

TEACHER: You cannot know that I shall not. You *improved* the original conjecture, but you cannot claim to have *perfected* the conjecture, to have achieved perfect rigour in your proof.

BETA: Can *you*?

TEACHER: I cannot either. But I think that my method of improving conjectures will be an improvement on yours for I shall establish a unity, a real interaction, between proofs and counterexamples.

BETA: I am ready to learn.

(*d*) *The method of monster-adjustment*

RHO: Sir, may I get a few words in edgeways?

TEACHER: By all means.

RHO: I agree that we should reject Delta's monster-barring as a general methodological approach, for it doesn't really take 'monsters' seriously. Beta doesn't take his 'exceptions' seriously either, for he merely lists them and then retreats into a safe domain. Thus both

these methods are interested only in a limited, privileged field. *My* method does not practise discrimination. I can show that 'on closer examination the exceptions turn out to be only apparent and the Euler theorem retains its validity even for the alleged exceptions'.[1]

TEACHER: Really?

ALPHA: How can my counterexample 3, the 'urchin' (fig. 5), be an ordinary Eulerian polyhedron? It has 12 star-pentagonal faces...

RHO: I don't see any 'star-pentagons'. Don't you see that in actual fact this polyhedron has ordinary *triangular* faces? There are 60 of them. It also has 90 edges and 32 vertices. Its 'Euler characteristic' is 2.[2] The 12 'star-pentagons', their 30 'edges' and 12 'vertices', yielding the 'characteristic' – 6, are only your fancy. Monsters don't exist, only monstrous interpretations. One has to purge one's mind from perverted illusions, one has to learn how to see and how to define correctly what one sees. My method is therapeutic: where you – erroneously – 'see' a counterexample, I teach you how to recognise – correctly – an example. I adjust your monstrous vision...[3]

ALPHA: Sir, please explain *your* method, before Rho brainwashes us.[4]

[1] Matthiessen [1863].

[2] The argument that the 'urchin' is 'really' an ordinary, prosaic Eulerian polyhedron with 60 triangular faces, 90 edges and 32 vertices – '*un hexacontaèdre sans épithète*' – was put forward by the staunch champion of the infallibility of the Euler theorem, E. de Jonquières ([1890*a*], p. 115). The idea of interpreting non-Eulerian star-polyhedra as triangular Eulerian polyhedra does not however stem from Jonquières but has a dramatic history (cf. footnote 4 below).

[3] Nothing is more characteristic of a dogmatist epistemology than its theory of error. For if some truths are manifest, one must explain how anyone can be mistaken about them, in other words, why the truths are not manifest to everybody. According to its particular theory of error, each dogmatist epistemology offers its particular therapeutics to purge minds from error. Cf. Popper [1963*a*], Introduction.

[4] Poinsot certainly was brainwashed some time between 1809 and 1858. It was Poinsot who rediscovered star-polyhedra, first analysed them from the point of view of Eulerianness and stated that some of them, like our small stellated dodecahedron, do not comply with Euler's formula ([1810]). Now this same Poinsot states categorically in his [1858] that Euler's formula 'is not only true for convex polyhedra, but for any polyhedron whatsoever, including star-polyhedra' (p. 67 – Poinsot uses the term *polyèdres d'espèce supérieure* for star-polyhedra). The contradiction is obvious. What is the explanation? What happened to the star-polyhedral *counterexamples*? The clue is in the first casual-looking sentence of the paper: 'One can reduce the whole theory of polyhedra to the theory of polyhedra with *triangular* faces.' That is, Poinsot–Alpha was brainwashed and turned into Poinsot–Rho: now he sees only triangles where he previously saw star-polygons: now he sees only examples where he previously saw counterexamples. The self-criticism had to be surreptitious, cryptic, because in scientific tradition there are no patterns available for articulating such volte-faces. One also wonders, did he ever come across ring-shaped faces and if so, did he knowingly reinterpret them with his triangular vision?

The change of vision need not always operate in the same direction. For example, TEACHER: Let him go on.

RHO: I have made my point.

GAMMA: Could you enlarge on your criticism of Delta's method? Both of you exorcised 'monsters'...

RHO: Delta was taken in by your hallucinations. He agreed that your 'urchin' has 12 faces, 30 edges and 12 vertices, and is non-Eulerian. His thesis was that it is not a polyhedron either. But he erred on both counts. Your 'urchin' *is* a polyhedron and *is* Eulerian. But its star-polyhedral interpretation was a *mis*interpretation. If you don't mind, it is not the imprint of the urchin on a healthy, pure mind, but its distorted imprint on a sick mind, twisting in pain.[1]

KAPPA: But how can you distinguish healthy minds from sick ones, rational from monstrous interpretations?[2]

RHO: What puzzles *me* is how you can mix them up!

SIGMA: Do you really think, Rho, that Alpha never noticed that his 'urchin' might be interpreted as a triangular polyhedron? Of course it might. But a closer look reveals that 'these triangles always lie in fives in the same plane and surround a regular pentagon hiding – like their heart – behind a solid angle. Now the five regular triangles together with the inner heart – the regular pentagon – form a so-called "pentagramma" that according to Theophrastus Paracelsus was the sign of health...'[3]

J. C. Becker in his [1869*a*] – fascinated by the new conceptual framework of simply- and multiply-connected domains (Riemann [1851]) – allowed for ring-shaped polygons but remained blind to star-polygons (p. 66). Five years after this paper – in which he claimed to have brought the problem to a 'definitive' solution – he broadened his vision and recognised star-polygonal and star-polyhedral patterns where he previously saw only triangles and triangular polyhedra ([1874]).

[1] This is part of a Stoic theory of error, attributed to Chrysippos (cf. Aetius [*c.* 150], IV.12.4; also Sextus Empiricus [*c.* 190], I. 249).

According to the Stoics the 'urchin' would be part of external reality, which produces an imprint upon the soul: the *phantasia* or *visum*. A wise man will not give uncritical assent (*synkatathesis* or *adsensus*) to a *phantasia* unless it matures into a clear and distinct idea (*phantasia katalēptikē* or *comprehensio*), which it cannot do if it is false. The system of clear and distinct ideas forms science (*epistēmē*). In our case the imprint of the 'urchin' on Alpha's mind would be the small stellated dodecahedron, while on Rho's mind it would be the triangular hexacontaeder. Rho would claim that Alpha's star-polyhedral vision cannot possibly mature into a clear and distinct idea, obviously since it would upset the 'proved' Euler formula. Thus the star-polyhedral interpretation would fail and the 'only' alternative to it, namely the triangular interpretation, would become clear and distinct.

[2] This is a standard Sceptic criticism of the Stoic claim that they can distinguish *phantasia* from *phantasia katalēptikē* (e.g. Sextus Empiricus [*c.* 190], I. 405).

[3] Kepler [1619], Lib. II. Propositio XXVI.

RHO: Superstition!

SIGMA: And so for the *healthy* mind the secret of the urchin will be revealed: that it is a new, hitherto undreamt-of regular body, with regular faces and equal solid angles, the beautiful symmetry of which might reveal to us the secrets of universal harmony...[1]

ALPHA: Thank you, Sigma, for your defence which again convinces me that opponents are less embarrassing than allies. Of course my polyhedral figure can be interpreted either as a triangular polyhedron or as a star-polyhedron. I am willing to admit both interpretations on a par...

KAPPA: Are you?

DELTA: But surely one of them is the *true* interpretation!

ALPHA: I am willing to admit both interpretations on a par, but one of them will certainly be a global counterexample to Euler's conjecture. Why admit only the interpretation that is 'well-adjusted' to Rho's preconceptions? Anyway, Sir, will you now explain *your* method?

(e) *Improving the conjecture by the method of lemma-incorporation. Proof-generated theorem versus naive conjecture*

TEACHER: Let us return to the picture-frame. I for one recognise it as a genuine global counterexample to the Euler conjecture, as well as a genuine local counterexample to the first lemma of my proof.

GAMMA: Excuse me, Sir – but how does the picture-frame refute the first lemma?

TEACHER: First remove a face and then try to stretch it flat on the blackboard. You will *not* succeed.

ALPHA: To help your imagination, I will tell you that those and only those polyhedra which you can inflate into a sphere have the property that, after a face is removed, you can stretch the remaining part onto a plane.

It is obvious that such a 'spherical' polyhedron is stretchable onto a plane after a face has been cut out; and vice versa it is equally obvious that, if a polyhedron minus a face is stretchable onto a plane, then you can bend it into a round vase which you can then cover with the missing face, thus getting a spherical polyhedron. But our picture-frame can never be inflated into a sphere; but only into a torus.

TEACHER: Good. Now, unlike Delta, I accept this picture-frame as a criticism of the conjecture. I therefore discard the conjecture in its original form as false, but I immediately put forward a modified,

[1] This is a fair exposition of Kepler's view.

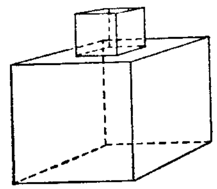


Fig. 12.

restricted version, namely this: the Descartes–Euler conjecture holds good for 'simple' polyhedra, i.e. for those which, after having had a face removed, can be stretched onto a plane. Thus we have rescued some of the original hypothesis. We have: *The Euler characteristic of a simple polyhedron is 2.* This thesis will not be falsified by the nested cube, by the twintetrahedra, or by star-polyhedra – for none of these is 'simple'.

So while the exception-barring method restricted both the domain of the main conjecture and of the guilty lemma to a common domain of safety, thereby accepting the counterexample as criticism both of the main conjecture and of the proof, my method of lemma-incorporation upholds the proof but reduces the domain of the main conjecture to the very domain of the guilty lemma. Or, while a counterexample which is both global and local made the exception-barrer revise both the lemmas and the original conjecture, it makes me revise the original conjecture, but not the lemmas. Do you understand?

ALPHA: Yes, I think I do. To show that I understand, I shall refute you.

TEACHER: My method or my improved conjecture?

ALPHA: Your improved conjecture.

TEACHER: Then you may still not understand my method. But let us have your counterexample.

ALPHA: Consider a cube with a smaller cube sitting on top of it (fig. 12). This complies with all our definitions – *Def. 1, 2, 3, 4, 4′, 5* – so it is a genuine polyhedron. And it is 'simple', in that it can be stretched on to the plane. Thus, according to your modified conjecture, its Euler characteristic should be 2. Nonetheless it has 16 vertices, 24 edges and 11 faces, and its Euler characteristic is $16-24+11 = 3$. It is a gobal counterexample to your improved conjecture and, by the way, also to Beta's first 'exception-barring' theorem. This polyhedron, in spite of having no cavities, tunnels or 'multiple structure', is *not* Eulerian.

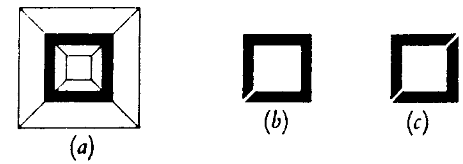


Fig. 13.

DELTA: Let us call this crested cube *Counterexample 6.*[1]

TEACHER: You have falsified my improved conjecture, but you have *not* destroyed my method of improvement. I shall re-examine the proof, and see why it broke down over your polyhedron. There must be another false lemma in the proof.

BETA: Of course there is. I have always suspected the second lemma. It presupposes that in the triangulating process, by drawing a new diagonal edge, you always increase by one the number of edges and of faces. This is false. If we look at the plane network of our crested polyhedron, we shall find a ring-shaped face (fig. 13(*a*)). In this case no single diagonal edge will increase the number of faces (fig. 13(*b*)): we need an increase of two edges to increase the number of faces by one (fig. 13(*c*)).

TEACHER: My congratulations. I certainly must restrict our conjecture further...

BETA: I know what you are going to do. You are going to say that '*Simple polyhedra with triangular faces are Eulerian*'. You will take triangulation for granted; and you will turn this lemma again into a condition.

[1] *Counterexample 6* was noticed by Lhuilier ([1812–13*a*], p. 186); Gergonne for once admits the novelty of his discovery! But almost fifty years later Poinsot had not heard of it ([1858]) while Matthiessen ([1863]) and, eighty years later, Jonquières ([1890*b*]) treated it as a monster. (Cf. footnotes 4, p. 31, 2, p. 38.) Primitive exception-barrers of the nineteenth century listed it as a curiosity together with other exceptions: 'As an example one is usually shown the case of a three sided pyramid attached to a face of a tetrahedron so that no edges of the former coincide with an edge of the latter. "Oddly enough, in this case $V-E+F = 3$" is what is written in my college notebook. And that ended the matter' (Matthiessen [1863], p. 449). Modern mathematicians tend to forget about ring-shaped faces, which may be irrelevant for the classification of manifolds but can become relevant in other contexts. H. Steinhaus says in his [1960]: 'Let us divide the globe into F countries (we shall consider *seas* and *oceans* as land). Then we shall have $V+F = E+2$, whatever the political situation may be' (p. 273). But one wonders whether Steinhaus would destroy West Berlin or San Marino simply because their existence refutes Euler's theorem. (Though of course he may prevent seas like the Baikal from falling completely in one country by defining them as *lakes*, since he has said that only seas and oceans are to be considered as land.)

TEACHER: No, you are mistaken. Before I point out your mistake concretely, let me enlarge upon my comment on your method of exception-barring. When you restrict your conjecture to a 'safe' domain, you do not examine the proof properly, and, in fact, you do not need to for your purpose. The casual statement that in your restricted domain all the lemmas will be true whatever they are, is enough for your purpose. But this is not enough for mine. I build the very same lemma which was refuted by the counterexample *into* the conjecture, so that I have to spot it and formulate it as precisely as possible, on the basis of a careful analysis of the proof. The refuted lemmas thus will be incorporated in my improved conjecture. Your method does not force you to give a painstaking *elaboration of the proof*, since the proof does not appear in your improved conjecture, as it does in mine. Now I return to your present suggestion. The lemma which was falsified by the ring-shaped face was not – as you seem to think – that '*all faces are triangular*' but that '*any face dissected by a diagonal edge falls into two pieces*'. It is *this* lemma which I turn into a condition. Calling the faces which satisfy it 'simply-connected', I can offer a second improvement on my original conjecture: '*For a simple polyhedron, with all its faces simply-connected,* $V-E+F = 2$.' The reason for your rash mis-statement was that your method did not teach you careful proof-analysis. Proof-analysis is sometimes trivial, but sometimes very difficult indeed.

BETA: I see your point. I should also add a self-critical note to your comment, for it seems to me to reveal a whole continuum of exception-barring attitudes. The worst merely bars some exceptions without looking at the proof at all. Hence the mystification when we have the proof on the one hand and the exceptions on the other. In the mind of such primitive exception-barrers, the proof and the exceptions exist in two completely separate compartments. Some others may now point out that the proof will work only in the restricted domain, and thereby claim to dispel the mystery. But their 'conditions' will still be extraneous to the proof-idea.[1] Better exception-

[1] '...Lhuilier's memoir consists of two *very distinct* parts. In the first the author offers an original proof of Euler's theorem. In the second his aim is to point out the exceptions to which this theorem is subjected.' (Gergonne's editorial comment on Lhuilier's paper in Lhuilier's [1812–13*a*], p. 172, my italics.)

M. Zacharias in his [1914–31] gives an uncritical but faithful description of this compartmentalisation: 'In the 19th century, geometers, besides finding new proofs of the Euler theorem, were engaged in establishing the exceptions which it suffers under certain conditions. Such exceptions were stated, e.g. by Poinsot. S. Lhuilier and F. Ch. Hessel tried to classify the exceptions...' (p. 1052).

barrers will glance quickly at the proof and gain, as I did just now, some inspiration for stating the conditions which determine a safe domain. The best exception-barrers do a careful analysis of the proof and, on this basis, give a very fine delineation of the prohibited area. In fact your method is, in this respect, a limiting case of the exception-barring method...

IOTA: ...and it displays the fundamental dialectical unity of proof and refutations.

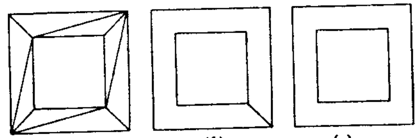
TEACHER: I hope that now all of you see that proofs, even though they may not *prove*, certainly do help to *improve* our conjecture.[1] *The exception-barrers improved it too, but improving was independent of proving. Our method improves by proving. This intrinsic unity between the 'logic of discovery' and the 'logic of justification' is the most important aspect of the method of lemma-incorporation.*

BETA: And of course I now understand your previous puzzling remarks about your not being perturbed by a conjecture being both 'proved' and refuted and about your willingness to 'prove' even a false conjecture.

KAPPA [*aside*]: But why call a 'proof' what in fact is an '*improof*'?

TEACHER: Mind you, few people will share this willingness. Most mathematicians, because of ingrained heuristic dogmas, are incapable of setting out simultaneously to prove *and* refute a conjecture. They would *either* prove it *or* refute it. Moreover, they are particularly incapable of improving conjectures by refuting them if the conjectures happen to be their own. *They want to improve their conjectures without refutations; never by reducing falsehood but by the monotonous increase of truth; thus they purge the growth of knowledge from the horror of counterexamples.* This is perhaps the background to the approach of the best sort of exception-barrers: they *start* by 'playing for safety' by devising a proof for the 'safe' domain and *continue* by submitting it to a thorough critical investigation, testing whether they have made use of each of the imposed conditions. If not, they 'sharpen' or 'generalise' the first modest version of their theorem, i.e. specify the lemmas on which the proof hinges, and incorporate them. For instance, after one or two counterexamples they may formulate the *provisional exception-barring theorem*: 'All convex polyhedra are Eulerian', postponing non-convex instances for a *cura posterior*; next they devise Cauchy's proof and then, discovering that convexity was not really 'used' in the proof, they

[1] Hardy, Littlewood, Wilder and Pólya seem to have missed this point (see footnote 1, p. 29).



(a) (b) (c)

Fig. 14. Three versions of the ring-shaped face: (*a*) Jonquières, (*b*) Matthiessen, (*c*) the 'untrained eye'.

build up the lemma-incorporating theorem![1] There is nothing heuristically unsound about this procedure which combines *provisional* exception-barring with successive proof-analysis and lemma-incorporation.

Beta: Of course this procedure does not abolish criticism, it only pushes it into the background: instead of directly criticising an over-statement, they criticise an under-statement.

Teacher: I am delighted, Beta, that I convinced you. Rho and Delta, how do *you* feel about it?

Rho: I for one certainly think that the problem of 'ring-shaped faces' is a pseudoproblem. It stems from a monstrous interpretation of what constitute the faces and edges of this soldering of two cubes into one – which you called a 'crested cube'.

Teacher: Explain.

Rho: The 'crested cube' is a polyhedron consisting of two cubes *soldered* to one another. Will you agree?

Teacher: I don't mind.

Rho: Now you misinterpreted 'soldering'. 'Soldering' consists of edges connecting the vertices of the bottom square of the small cube to the corresponding vertices of the top square of the large cube. So there is no 'ring-shaped face' at all.

Beta: The ring-shaped face is there! The dissecting edges you are talking about are not there!

Rho: They are just hidden from your untrained eyes.[2]

[1] This standard pattern is essentially the one described in the classic of Pólya and Szegö [1927], p. vii: 'One should scrutinise each proof to see if one has in fact made use of all the assumptions; one should try to get the same consequence from fewer assumptions ...and one should not be satisfied until counterexamples show that one has arrived at the boundary of the possibilities.'

[2] This 'soldering' of the two polyhedra by hidden edges is argued by Jonquières ([1890*b*], pp. 171–2), who uses monster-barring against cavities and tunnels but monster-adjustment against crested cubes and star-polyhedra. The first proponent of using monster-adjustment in defence of the Euler theorem was Matthiessen [1863]. He uses monster-adjustment consistently: he succeeds in displaying hidden edges and faces to explain away everything that is non-Eulerian, including polyhedra with tunnels and cavities. While Jonquières' soldering is a complete triangulation of the ring-shaped face, Matthiessen solders with economy, by drawing only the minimal number of edges that split the face into simply-connected sub-faces (fig. 14).

Matthiessen is remarkably confident about his method of turning revolutionary counterexamples into well-adjusted bourgeois Eulerian examples. He claims that 'any polyhedron can be analysed in such a way that it corroborates Euler's theorem...'. He enumerates the alleged exceptions noted by the superficial observer and then states: 'In each such case we can show that the polyhedron has hidden faces and edges, which, if counted, leave the theorem $V-E+F = 2$ untarnished even for these seemingly recalcitrant cases.'

The idea that, by drawing additional edges or faces, some non-Eulerian polyhedra can be transformed into Eulerian ones, stems however not from Matthiessen, but from Hessel. Hessel illustrates this point with three examples using nice figures ([1832], pp. 14–15). But he did not use this method to 'adjust' but, on the contrary, to 'elucidate the exceptions' by showing 'rather similar polyhedra for which Euler's law is valid'.

Beta: Do you expect us to take your argument seriously? What *I* see is superstition, but *your* 'hidden' edges are reality?

Rho: Look at this salt crystal. Would you say this is a cube?

Beta: Certainly.

Rho: A cube has 12 edges, hasn't it?

Beta: Yes, it has.

Rho: But on this cube there are no edges at all. They are hidden. They appear only in your rational reconstruction.

Beta: I shall think about this. One thing is clear. The Teacher criticised my conceited view that my method leads to certainty, and also for forgetting about the proof. These criticisms apply just as much to your 'monster-adjustment' as to my 'exception-barring'.

Teacher: Delta, what about you? How would *you* exorcise the ring-shaped face?

Delta: I would not. You have converted me to your method. I only wonder why you don't make sure and also incorporate the neglected *third* lemma? I propose a fourth, and, I hope, final formulation: 'All polyhedra are Eulerian, which are (*a*) simple, (*b*) have each face simply-connected, and (*c*) are such that the triangles in the plane triangular network, resulting from stretching and triangulating, can be so numbered that, in removing them in the right order, $V-E+F$ will not alter until we reach the last triangle.'[1] I wonder why you did not propose this at once? If you really took your method seriously, you would

[1] This last lemma is unnecessarily strong. It would be enough for the purpose of the proof to replace it by the lemma that 'for the plane triangular network resulting from stretching and triangulating $V-E+F = 1$'. Cauchy does not seem to have noticed the difference.

have turned *all* the lemmas *immediately* into conditions. Why this 'piecemeal engineering'?[1]

ALPHA: Tory turned into revolutionary! Your suggestion strikes me as rather Utopian. For there aren't just *three* lemmas. Why not add, with many others, conditions like '(4) if $1+1 = 2$', and '(5) if all triangles have three vertices and three edges', since we certainly use these lemmas? I propose that we turn only those lemmas into conditions for which a counterexample has been found.

GAMMA: This seems to me too accidental to be accepted as a methodological rule. Let us build in all those lemmas against which we can *expect* counterexamples, i.e. which are not obviously, indubitably true.

DELTA: Well, does our third lemma strike anyone as obvious? Let us turn it into a third condition.

GAMMA: What if the operations expressed by the lemmas of our proof are not all independent? If some of the operations can be performed, it may be that the rest must *necessarily* be able to be performed. I, for one, suspect that *if a polyhedron is simple then there always exists an order of deletion of triangles in the resulting flat network such that $V-E+F$ will not alter*. If there is, then incorporating the first lemma into the conjecture would exempt us from incorporating the third.

DELTA: You claim that the first condition implies the third. Can you prove this?

EPSILON: I can.[2]

ALPHA: The actual proof, however interesting, will not help us in solving our problem: how far should we go in improving our conjecture? I may admit that you have the proof you claim to have – but that will only decompose this third lemma into some new sub-lemmas. Should we now turn these into conditions? Where should we stop?

KAPPA: There is an infinite regress in proofs; therefore proofs do not prove. You should realise that proving is a game, to be played while you enjoy it and stopped when you get tired of it.

EPSILON: No, this is no game but a serious matter. The infinite regress can be halted by trivially true lemmas, which need not be turned into conditions.

[1] The students are obviously quite knowledgeable about recent social philosophy. The term was coined by K. R. Popper ([1957], p. 64).

[2] Actually, such a proof was first proposed by H. Reichardt ([1941], p. 23). Also cf. B. L. van der Waerden [1941]. Hilbert and Cohn-Vossen were satisfied that the truth of Gamma's assertion is 'easy to see' ([1932], English translation, p. 292).

GAMMA: This is just what I meant. We do not turn into conditions those lemmas which can be proved from trivially true principles. Nor do we incorporate those lemmas which can be proved – possibly with the help of such trivially true principles – from previously specified lemmas.

ALPHA: Agreed. We can then stop improving our conjecture after we have turned the two non-trivial lemmas into conditions. In fact I do think that this method of improvement, by lemma-incorporation, is flawless. It seems to me that it not only improves but *perfects* the conjecture. And I learned something important from it: that it is wrong to assert that 'the aim of a "problem to prove" is to show conclusively that a certain clearly stated assertion is true, or else to show that it is false'.[1] The *real* aim of a 'problem to prove' should be to *improve* – in fact, perfect – the original, '*naive*' *conjecture* into a genuine '*theorem*'.

Our naive conjecture was 'All polyhedra are Eulerian'.

The monster-barring method defends this naive conjecture by reinterpreting its terms in such a way that at the end we have a *monster-barring theorem*: 'All polyhedra are Eulerian.' But the identity of the linguistic expressions of the naive conjecture and the monster-barring theorem hides, behind surreptitious changes in the meaning of the terms, an essential improvement.

The exception-barring method introduced an element which is really extraneous to the argument: convexity. The *exception-barring theorem* was: 'All convex polyhedra are Eulerian.'

The lemma-incorporating method relied on the argument – i.e. on the proof – and on nothing else. It virtually *summed up the proof in the lemma-incorporating theorem*: 'All simple polyhedra with simply-connected faces are Eulerian.'

This shows that (now I use the term 'proving' in the traditional sense) *one does not prove what one has set out to prove*. Therefore no proof should conclude with the words: '*Quod erat demonstrandum*.'[2]

BETA: Some people say that theorems precede proofs in the order of discovery: 'You have to guess a mathematical theorem before you prove it.' Others deny this, and claim that discovery proceeds by drawing conclusions from a specified set of premisses and noting the interesting ones – if you are lucky enough to find any. Or, to use a delightful metaphor of a friend of mine, some say that the heuristic

[1] Pólya ([1945], p. 142).

[2] This last sentence is from Alice Ambrose's interesting paper ([1959], p. 438).

'zip fastener' in a deductive structure goes upwards from the bottom – the conclusion – to the top – the premisses,[1] others say that it goes downwards from the top to the bottom. What is your position?

ALPHA: That your metaphor is inapplicable to heuristic. Discovery does not go up or down, but follows a zig-zag path: prodded by counterexamples, it moves from the naive conjecture to the premisses and then turns back again to delete the naive conjecture and replace it by the theorem. Naive conjecture and counterexamples do not appear in the fully fledged deductive structure: the zig-zag of discovery cannot be discerned in the end-product.

TEACHER: Very good. But let us add a note of caution. The theorem does not *always* differ from the naive conjecture. We do not necessarily improve by proving. Proofs improve when the proof-idea discovers unexpected aspects of the naive conjecture which then appear in the theorem. But in *mature* theories this might not be the case. It is certainly the case in young, *growing* theories. This intertwining of discovery and justification, of improving and proving is primarily characteristic of the latter.

KAPPA [*aside*]: Mature theories can be rejuvenated. Discovery always supersedes justification.

SIGMA: This classification corresponds to mine! My first type of propositions was the mature type, the third the growing type...

GAMMA [*interrupts him*]: The theorem is false! I found a counterexample to it.

5. *Criticism of the Proof-Analysis by Counterexamples which are Global but not Local. The Problem of Rigour*

(*a*) *Monster-barring in defence of the theorem*

GAMMA: I have just discovered that my *Counterexample 5*, the cylinder, refutes not only the naive conjecture but also the theorem. Although it satisfies both lemmas, it is not Eulerian.

ALPHA: Dear Gamma, do not become a crank. The cylinder was a joke, not a counterexample. No serious mathematician will take the cylinder for a polyhedron.

GAMMA: Why didn't you protest against my *Counterexample 3*, the

[1] Cf. footnote 1, p. 9. The metaphor of the 'zip fastener' was invented by R. B. Braithwaite; however, he talks only of 'logical' and 'epistemological' zip fasteners, but not of 'heuristic' ones ([1953], esp. p. 352).

urchin? Was that less 'crankish' than my cylinder?[1] *Then* of course you were *criticising* the naive conjecture and welcomed refutations. *Now* you are *defending* the theorem and abhor refutations! *Then*, when a counterexample emerged, your question was: *what is wrong with the conjecture? Now* your question is: *what is wrong with the counterexample?*

DELTA: Alpha, you have turned into a monster-barrer! Aren't you embarrassed?[2]

(*b*) *Hidden lemmas*

ALPHA: I am. I may have been a bit rash. Let me think. There are *three possible types of counterexamples*. We have already discussed the *first*, which is local but not global – it certainly would not refute the theorem.[3] The *second*, which is both global and local, does not require any action: far from refuting the theorem, it confirms it. Now there may be a *third* type, which is global but not local. This would refute the theorem. I did not think that this was possible. Now Gamma claims that the cylinder is one. If we do not want to reject it as a monster, we have to admit that it is a global counterexample: $V-E+F = 1$. But is it not of the second harmless type? I bet it does not satisfy at least one of the lemmas.

GAMMA: Let us check. It certainly satisfies the first lemma: if I remove the bottom face, I can easily stretch the rest on to the blackboard.

ALPHA: But if you happen to remove the jacket, the thing falls into two pieces!

GAMMA: So what? The first lemma required that the polyhedron be 'simple', i.e. 'after having had a face removed, it can be stretched on to a plane'. The cylinder satisfies this requirement even if you start by removing the jacket. What you are claiming is that the cylinder should satisfy an *additional* lemma, namely that *the resulting plane network also be connected.* But who has ever stated *this* lemma?

[1] The urchin and the cylinder were discussed above, pp. 16 and 31.

[2] Monster-barring in defence of the theorem is an important pattern in informal mathematics: 'What is wrong with the examples in which Euler's formula fails? Which geometrical conditions, rendering more precise the meaning of F, V, and E, would ensure the validity of Euler's formula?' (Pólya [1954], 1, exercise 29). The cylinder is given in exercise 24. The answer is: '...an edge...should terminate in corners...' (p. 225). Pólya formulates this generally: 'The situation, not infrequent in mathematical research is this: A theorem has been already formulated but we have to give a more precise meaning to the terms in which it is formulated in order to render it strictly correct' (p. 55).

[3] Local but not global counterexamples were discussed on pp. 10–12.