Proofs and Refutations (II)

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# PROOFS AND REFUTATIONS (II) ★

I. Lakatos

## (*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 agressive 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

★ Part I appeared in the previous number.

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 monsterbarrer, 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.'[2] 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.'[3] Now I looked at the

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

[2] Hessel [1832], p. 13. Hessel rediscovered Lhuilier's 'exceptions' in 1832. Just after submitting his manuscript he came across Lhuilier's [1812-13]. 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 [1826-27] textbook he 'proved' that Euler's theorem was true for *all* polyhedra (Vol. II, pp. 668-671).

[3] 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 monsterbarring, but—like Rho—by monster-adjustment (cf. footnote 1, p. 135).

'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 monsterbarring, 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'?

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

TEACHER: This debate showed that monsterbarring 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 monsterbarring 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),* $V - E + F = 2$.

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

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',* $V - E + F = 2$.[1]

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 monsterbarring 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. The *adhocness* of your method destroys its chance of achieving certainty.

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?

[1] 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], 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 the method of *experimentum crucis*, which eliminates all but one, *proves* this last one.

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 of inferring from the special to the general'.[2] 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.[3] So instead of barring exceptions piecemeal, I shall draw the borderline modestly, but safely: *All convex polyhedra are Eulerian.*[4] And I hope you will grant that this has nothing conjectural about it: that it is a theorem.

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

[2] Abel [1826]. His criticism seems to be directed against Eulerian inductivism.

[3] 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' ([1809], § 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.

[4] 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. 16), 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.

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 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]

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).

[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

ETA: Thank you, sir, for this argument. Beta's embarrassment clearly displays the superiority of the defamed monsterbarring 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 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.

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. 190-191).

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

[1] L. 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 story (cf. footnote 2, p. 128).

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. . . .[1]

ALPHA: Sir, please explain *your* method, before Rho brainwashes us.[2]

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

[1] 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], Introduction.

[2] 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 Euleriannes, and stated that some of them, like our small stellated dodecahedron, do not comply with Euler's formula ([1809]). Now this very 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 J. C. Becker in his [1869]—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]).

star-polyhedral concept was a misinterpretation. 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]

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. . . .[4]

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?

[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.

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

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, 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 twin-tetrahedra, 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.[1]

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.

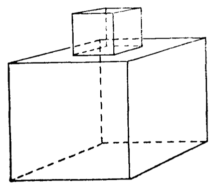

FIG. 12

ALPHA: Consider a cube with a smaller cube sitting on top of it (Fig. 12). This complies with all our definitions—Def. 0, 1, 2, 3, 3a, 4—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 global 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.

DELTA: Let us call this crested cube *Counterexample 6*.[2]

[1] I recall Karl Popper distinguishing three levels of understanding. The lowest was the pleasant feeling of having grasped the argument. The medium level was when one could repeat it. The top level was when one could refute it.

[2] *Counterexample 6* was noticed by Lhuilier ([1812-13], 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, de Jonquières [1890*b*] treated it as a monster. (Cf. footnotes 2, p. 128, 1, p. 135.) 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

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. 13a). In this case no single diagonal edge will increase the number of faces (Fig. 13b): we need an increase of two edges to increase the number of faces by one (Fig. 13c).

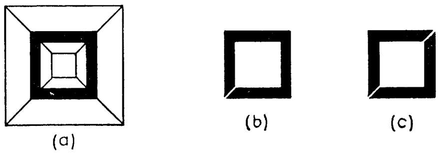


FIG. 13

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.

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

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.)

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-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

[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], 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).

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 heuristical 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 counter-examples.* This is perhaps the background to the approach of the best sort of exceptionbarrers: 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 build up the lemma-incorporating theorem![2]

[1] Hardy, Littlewood, Wilder and Pólya seem to have missed this point (see footnote 1, p. 125).

[2] 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

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.

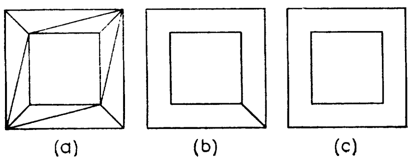


FIG. 14. Three versions of the ring-shaped face: (*a*) de Jonquières, (*b*) Matthiessen, (*c*) the 'untrained eye'.

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 ringshaped face is there! The dissecting edges you are talking about are not there!

RHO: They are just hidden from your untrained eyes.[1]

from fewer assumptions . . . and one should not be satisfied until counterexamples show that one has arrived at the boundary of the possibilities.'

[1] This ' soldering ' of the two polyhedra by hidden edges is argued by de Jonquières ([1890*b*], pp. 171-172), who uses monsterbarring against cavities and tunnels but monster-adjustment against crested cubes and star-polyhedra. The first protagonist 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 de Jonquières' soldering is a complete triangulation of the ring-shaped face, he 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

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 it a cube?

BETA: Certainly.

RHO: A cube has 12 vertices, 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 faces?

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 have turned *all* the lemmas *immediately* into conditions. Why this ' piecemeal engineering '?[2]

' 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 '.

[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.

[2] The students are obviously quite knowledgeable about recent social philosophy. The term was coined by K. R. Popper ([1957], p. 67).

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.[1]

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.

GAMMA: This is just what I meant. We do not turn those lemmas into conditions which can be proved from trivially true principles.

[1] Actually, such a proof was first proposed by H. Reichardt ([1941], p. 23). Also cf. B. L. van der Waerden [1951]. Hilbert and Cohn-Vossen were satisfied that the truth of Beta's assertion is 'easy to see' ([1932], English translation, p. 292).

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 monsterbarring 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 'zip fastener' in a deductive structure goes upwards from the

[1] Pólya ([1945], p. 142)

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

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.

[1] Cf. Part I, footnote 2, p. 10. 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).

(*To be continued*)