Proofs and Refutations (III)

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PROOFS AND REFUTATIONS (III)*

I. LAKATOS

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

(*a*) *Monsterbarring in defence of the theorem*

GAMMA: I have just discovered that my *Counterexample 5*, the cylinder, refutes not only the naïve 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 urchin? Was that less 'crankish' than my cylinder?[1] *Then* of course you were *criticising* the naïve 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 monsterbarrer! 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

* Part I and Part II appeared in the preceding numbers. The table of contents given with Part I has been slightly altered.

[1] The urchin and the cylinder were discussed previously in Part I, pp. 18 and 24.

[2] Monsterbarring 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], I, 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).

the *first*, which is local but not global—it certainly would not refute the theorem.[1] The *second*, which is both global and local, does not require any action: far from refuting the theorem, it confirms it.[2] 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: $V - E + F = 0$. 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?

ALPHA: Everybody has interpreted 'stretched' as 'stretched in *one piece*', 'stretched *without tear*'. . . . We decided not to incorporate the third lemma because of Epsilon's proof that it followed from the first two.[3] But just have a look at that proof: it hinges on the assumption that the result of the stretching is a *connected* network! Otherwise for the triangulated network $V - E + F$ would not be 1.

GAMMA: Why then didn't you insist on stating it *explicitly?*

ALPHA: Because we took it to be stated *implicitly*.

GAMMA: You, for one, certainly did not. For you proposed that 'simple' stand for 'pumpable into a ball'.[4] The cylinder *can* be pumped into a ball—so according to *your* interpretation it *does* comply with the first lemma.

ALPHA: Well. . . . But you have to agree that it does *not* satisfy the *second* lemma, namely, that '*any face dissected by a diagonal falls into two pieces*'. How will you triangulate the circle or the jacket? Are these faces simply-connected?

[1] Local but not global counterexamples were discussed in Part I, pp. 11-14.

[2] This corresponds to the paradox of confirmation (Hempel [1945]).

[3] See Part II, p. 137

[4] See Part II, p. 130

GAMMA: Of course they are.

ALPHA: But on the cylinder one cannot draw diagonals at all! A diagonal is an edge that connects two non-adjacent vertices. But your cylinder has no vertices!

GAMMA: Don't get upset. If you want to show that the circle is not simply-connected, draw a diagonal which does *not* create a new face.

ALPHA: Don't be funny; you know very well that I cannot.

GAMMA: Then would you admit that 'there is a diagonal of the circle that does not create a new face' is a *false* statement?

ALPHA: Yes, I would. What are you up to now?

GAMMA: Then you are bound to admit that its negation is true, namely, that 'all diagonals of the circle create a new face', or, that 'the circle is simply-connected'.

ALPHA: You cannot give an *instance* of your lemma that 'all diagonals of the circle create a new face'—therefore it is not *true*, but *meaningless*. Your conception of truth is false.

KAPPA [*aside*]: First they quarrelled about what is a polyhedron, now about what is truth![1]

GAMMA: But you already admitted that the negation of the lemma was *false*! Or can a proposition *A* be *meaningless* while *Not-A* is *meaningful and false*? Your conception of meaning does not make sense!

Mind you, I see your difficulty; but we can overcome it by a slight reformulation. Let us call a face simply-connected if '*for all x, if x is a diagonal then x cuts the face into two*'. Neither the circle nor the jacket can have diagonals, so that in their case, whatever *x* is, the antecedent will always be false. Therefore the conditional will be instantiated by any object, and will be both meaningful and true. Or, both the circle and the jacket are simply-connected—the cylinder satisfies the second lemma.

ALPHA: No! If you cannot draw diagonals and thereby triangulate the faces, you will never arrive at a flat triangular network and you will never be able to conclude the proof. How can you then claim that the cylinder satisfies the second lemma? Don't you see that *there must be an existential clause* in the lemma? The correct interpretation of the simply-connectedness of a face must be: '*for all x, if x is a diagonal, then x cuts the face into two; and there is at least one x that is a diagonal*'. Our original formulation may not have spelt it

[1] Gamma's *vacuously true statements* were a major innovation of the nineteenth century. Its problem-background has not yet been unfolded.

out but it was there as an unconsciously made '*hidden assumption*'.[1] All the faces of the cylinder fail to meet it; therefore the cylinder is a counterexample which is *both* global and local, and it does *not* refute the theorem.

GAMMA: First you modified the stretching lemma by introducing 'connectedness', now the triangulating lemma by introducing your existential clause! And all this obscure talk about 'hidden assumptions' only hides the fact that my cylinder made you invent these modifications.

ALPHA: What obscure talk? We already agreed to omit, that is, 'hide', trivially true lemmas.[2] Why then should we state and incorporate trivially *false* lemmas—they are just as trivial and just as boring! Keep them in your mind (*en thyme*) but do not state them. A hidden lemma is not an error: it is shrewd shorthand pointing to our background knowledge.

KAPPA [*aside*]: Background knowledge is where we assume that we know everything but in fact know nothing.[3]

GAMMA: If you did make conscious assumptions, they were that (*a*) removing a face always leaves a connected network and (*b*) any

[1] 'Euclid employs an axiom of which he is wholly unconscious' (Russell [1903], p. 407). 'To make [*sic*] a hidden assumption' is a common phrase among mathematicians and scientists. See also Gamow's discussion of Cauchy's proof ([1953], p. 56) or Eves-Newsom on Euclid ([1958], p. 84).

[2] See Part II, pp. 137–8

[3] Good textbooks in informal mathematics usually specify their 'shorthand', i.e. those lemmas, either true or false, which they regard so trivial as not to be worth mentioning. The standard expression for this is 'we assume *familiarity* with lemmas of type *x*'. The amount of assumed familiarity decreases as criticism turns background knowledge into knowledge. Cauchy, e.g. did not even notice that his celebrated [1821] presupposed 'familiarity' with the *theory of real numbers*. He would have rejected as a monster any counterexample which made lemmas about the nature of irrational numbers explicit. Not so Weierstrass and his school: textbooks of informal mathematics now contain a new chapter on the theory of real numbers where these lemmas are collected. But in their introductions 'familiarity with the *theory of rational numbers*' is usually assumed. (See e.g. Hardy's *Pure Mathematics* from the second edition (1914) onwards—the first edition still relegated the theory of real numbers to background knowledge; or Rudin [1953]). More rigorous textbooks narrow down background knowledge even further: Landau, in the introduction to his famous [1930], assumes familiarity only with '*logical reasoning and German language*'. It is ironical that at the very same time Tarski showed that the absolutely trivial lemmas thus omitted may not only be false but inconsistent—German being a semantically closed language. One wonders when 'the author confesses ignorance about the field *x*' will replace the authoritarian euphemism 'the author assumes familiarity with the field *x*': surely only when it is recognised that knowledge has no foundations.

non-triangular face can be dissected into triangles by diagonals. While they were in your *subconscious*, they were listed as *trivially true*—the cylinder however made them somersault into your *conscious* list as *trivially false*. Before being confronted by the cylinder you could not even conceive that the two lemmas could be false. If you now say that you did, then you are rewriting history to purge it from error.[1]

THETA: Not long ago, Alpha, you ridiculed the 'hidden' clauses which cropped up in Delta's definitions after each refutation. Now it is you who make up 'hidden' clauses in the lemmas after each refutation, it is you who shift your ground and try to hide it to save face. Aren't you embarrassed?

KAPPA: Nothing amuses me more than the dogmatist at bay. After donning the militant sceptic's robe to demolish a lesser brand of dogmatism, Alpha becomes frantic when *he* in turn is cornered by the same sort of sceptical arguments. He now plays fast and loose: trying to fight off Gamma's counterexample first with the defence-mechanism he himself had exposed and forbidden (monsterbarring), then by smuggling a reserve of 'hidden lemmas' into the proof and corresponding 'hidden conditions' into the theorem. What is the difference?

TEACHER: The trouble with Alpha was certainly the dogmatist turn in his interpretation of lemma-incorporation. He thought that a careful inspection of the proof would yield a perfect proof-analysis

[1] When it is first discovered, the hidden lemma is considered an error. When J. C. Becker first pionted out a 'hidden' (*stillschweigend*) assumption in Cauchy's proof (he quoted the proof second-hand from Baltzer's [1806–27]), he called it an '*error*' ([1869], pp. 67–68). He drew attention to the fact that Cauchy thought that *all* polyhedra were simple: his lemma was not only hidden but also false. Historians however cannot imagine that great mathematicians should make such errors. A veritable programme of how to falsify history can be found in Poincaré's [1908]: 'A demonstration which is not rigorous is nothingness. I think no one will contest this truth. But if it were taken too literally, we should be led to conclude that before 1820, for example, there was no mathematics; this would be manifestly excessive; the geometers of that time understood voluntarily what we explain by prolix discourse. This does not mean that they did not see it at all; but they passed over it too rapidly, and to see it well would have necessitated taking the pains to say it' (p. 374). Becker's report about Cauchy's 'error' had to be rewritten 1984-wise: 'doubleplusungood refs unerrors rewrite fullwise.' The rewriting was done by E. Steinitz who insisted that 'the fact that the theorem was not generally valid could not possibly remain unnoticed' ([1914–31], p. 20). Poincaré himself applied his programme to the Euler-theorem: 'It is known that Euler proved that $V - E + F = 2$ for *convex* polyhedra' ([1893])—Euler of course stated his theorem for *all* polyhedra.

containing *all* the false lemmas (just as Beta thought he could enumerate *all* the exceptions). He thought that by incorporating them he could attain not only an improved theorem, but a *perfected* theorem,[1] *without bothering about counterexamples.* The cylinder showed him to be wrong but, instead of admitting it, he now wants to call a proof-analysis complete if it contains all the *relevant* false lemmas.

(*c*) *The method of proof and refutations*

GAMMA: I propose to accept the cylinder as a genuine counterexample to the theorem. I invent a new lemma (or lemmas) that will be refuted by it and add the lemma(s) to the original list. This of course is exactly what Alpha did. But instead of 'hiding' them so that they *become* hidden, I announce them publicly.

Now the cylinder which was a puzzling, dangerous global but not local counterexample (the third type) in respect of the old proof-analysis and of the corresponding old theorem, will be a harmless, global *and* local counterexample (the second type) in respect of the new proof-analysis and the corresponding new theorem.

Alpha thought that his classification of counterexamples was absolute—but in fact it was relative to his proof-analysis. As proof-analysis grows, counterexamples of the third type turn into counterexamples of the second type.

LAMBDA: That is right. A proof-analysis is 'rigorous' or 'valid' and the corresponding mathematical theorem true if, and only if, there is no 'third-type' counterexample to it. I call this criterion the *Principle of Retransmission of Falsity* because it demands that global counterexamples be also local: falsehood should be retransmitted from the naïve conjecture to the lemmas, from the consequent of the theorem to its antecedent. If a global but not local counterexample violates this principle, we restore it by adding a suitable lemma to the proof-analysis. The Principle of Retransmission of Falsity is therefore a *regulative principle* for proof-analysis in *statu nascendi*, and a global but not local counterexample is a fermenting agent in the growth of proof-analysis.

GAMMA: Remember, even before finding a single refutation we managed to pick out three suspicious lemmas and go ahead with the proof-analysis!

[1] See Part II, p. 138

LAMBDA: That is true. Proof-analysis may start not only under the pressure of global counterexamples but also when people have already learned to be on guard against 'convincing' proofs.[1]

In the *first case* all global counterexamples appear as counterexamples of the third-type, and all the lemmas start their career as 'hidden lemmas'. They lead us to a gradual build-up of the proof-analysis and so turn one by one into counterexamples of the second-type.

In the *second case*—when we are already in a suspicious mood and look out for refutations—we may arrive at an advanced proof-analysis without any counterexamples. Then there are two possibilities. The *first possibility* is that *we succeed* in refuting—by local counterexamples—the lemmas listed in our proof-analysis. We may very well find that these are also global counterexamples.

ALPHA: This is how I discovered the picture-frame: looking for a polyhedron that, after having a face removed, could not be stretched flat onto a plane.

SIGMA: Then not only do refutations act as fermenting agents for proof-analysis, but proof-analysis may act as a fermenting agent for refutations! What an unholy alliance between seeming enemies!

LAMBDA: That is right. If a conjecture seems very plausible or even self-evident, one should prove it: one may find that it hinges on very sophisticated and dubious lemmas. Refuting the lemmas may lead to some unexpected refutation of the original conjecture.

SIGMA: To proof-generated refutations!

GAMMA: Then 'the virtue of a logical proof is not that it compels belief, but that it suggests doubts'.[2]

LAMBDA: But let me come back to the *second possibility*: when we do *not* find any local counterexamples to the suspected lemmas.

SIGMA: That is, when refutations do not assist proof-analysis! What would happen then?

[1] Our class was a rather advanced one—Alpha, Beta, and Gamma suspected three lemmas when no global counterexamples turned up. In actual history proof-analysis came many decades later: for a long period the counterexamples were either hushed up or exorcised as monsters, or listed as exceptions. The heuristic move from the global counterexample to proof-analysis—the application of the Principle of Retransmission of Falsity—was virtually unknown in the informal mathematics of the early nineteenth century.

[2] H. G. Forder [1927], p. viii. Or: 'It is one of the chief merits of proofs that they instil a certain scepticism as to the result proved.' (Russell [1903], p. 360. He also gives an excellent example.)

LAMBDA: We would be branded cranks. The proof would acquire absolute respectability and the lemmas would shake off suspicion. Our proof-analysis would soon be forgotten.[1] Without refutations one cannot sustain suspicion: the searchlight of suspicion soon switches off if a counterexample does not reinforce it, directing the limelight of refutation onto a neglected aspect of the proof that had scarcely been noticed in the twilight of 'trivial truth'.

All this shows that one cannot put proof and refutations into

[1] It is well known that *criticism* may cast doubt on, and eventually refute, '*a priori* truths' and so turn *proofs* into mere *explanations*. That *lack of criticism or of refutation* may turn implausible conjectures into '*a priori* truths' and so tentative explanations into proofs is not so well-known but just as important. Two major examples of this pattern are the emergence and fall of Euclid and Newton. The story of their fall is well-known, but the story of their emergence is usually misrepresented.

Euclid's geometry seems to have been proposed as a cosmological theory (cf. Popper [1952], pp. 147-148). Both its 'postulates' and 'axioms' (or 'common notions') were proposed as bold, provocative propositions, challenging Parmenides and Zeno, whose doctrines entailed not only the falsity, but even the logical falsity, the inconceivability, of these 'postulates'. Only later were the 'postulates' taken to be indubitably true and the bold anti-Parmenidean 'axioms' (such as 'the whole is greater than the part') taken to be so trivial that they were omitted in later proof-analysis and turned into 'hidden lemmas'. This process started with Aristotle: he branded Zeno a quarrelsome crank, and his arguments 'sophistry'. This story was recently unfolded in exciting detail by Árpád Szabó ([1960], pp. 65-84). Szabó showed that in Euclid's time the word 'axiom'—like 'postulate'—meant a proposition in the critical dialogue (dialectic) put forward to be tested for consequences *without* being admitted as true by the discussion-partner. It is the irony of history that its meaning was turned upside down. The peak of Euclid's authority was reached in the Age of Enlightenment. Clairaut urges his colleagues not to 'obscure proofs and disgust readers' by stating evident truths: Euclid did so only in order to convince 'obstinate sophists' ([1741], pp. x and xi).

Again, *Newton's mechanics and theory of gravitation* was put forward as a daring guess, which was ridiculed and called 'occult' by Leibnitz and suspected even by Newton himself. But a few decades later—in the absence of refutations—his axioms came to be taken as indubitably true. Suspicions were forgotten, critics branded 'eccentric' if not 'obscurantist'; some of his most doubtful assumptions came to be regarded as so trivial that textbooks never even stated them. The debate—from Kant to Poincaré—was no longer about the truth of Newtonian theory but about the nature of its certainty. (This *volteface* in the appraisal of Newtonian theory was first pointed out by Karl Popper—see his [1963], *passim.*)

The analogy between political ideologies and scientific theories is then more far-reaching than is commonly realised: political ideologies which first may be debated (and perhaps accepted only under pressure) may turn into unquestioned background knowledge even in a single generation: the critics are forgotten (and perhaps executed) until a revolution vindicates their objections.

separate compartments. This is why I would propose to rechristen our '*method of lemma-incorporation*' the '*method of proof and refutations*'. Let me state its main aspects in three heuristic rules:

Rule 1. *If you have a conjecture, set out to prove it and to refute it. Inspect the proof carefully to prepare a list of non-trivial lemmas (proof-analysis); find counterexamples both to the conjecture (global counterexamples) and to the suspect lemmas (local counterexamples).*

Rule 2. *If you have a global counterexample discard your conjecture, add to your proof-analysis a suitable lemma that will be refuted by it, and replace the discarded conjecture by an improved one that incorporates that lemma as a condition.*[1] *Do not allow a refutation to be dismissed as a monster.*[2] *Make all 'hidden lemmas' explicit.*[3]

Rule 3. *If you have a local counterexample, check to see whether it is not also a global counterexample. If it is, you can easily apply Rule* 2.

(*d*) *Proof versus proof-analysis. The relativisation of the concepts of theorem and rigour in proof-analysis*

ALPHA: What did you mean by 'suitable' in your *Rule* 2?

GAMMA: It is completely redundant. *Any* lemma which is refuted by the counterexample in question can be added—for *any* such lemma will restore the validity of the proof-analysis.

LAMBDA: What! So a lemma like 'All polyhedra have at least 17 edges' would take care of the cylinder! And any other random *ad hoc* conjecture would do just as well, so long as it happened to be refuted by the counterexample.

GAMMA: Why not?

LAMBDA: We already criticised monster-barrers and exception-barrers for forgetting about proofs.[4] Now you are doing the same, inventing a real monster: *proof-analysis without proof*! The only difference between you and the monsterbarrer is that you would have Delta make his arbitrary definitions explicit and incorporate them into

[1] This rule seems to have been stated for the first time by Ph. L. Seidel ([1847], p. 383).

[2] 'I have the right to put forward any example that satisfies the conditions of your argument and I strongly suspect that what you call bizarre, preposterous examples are in fact embarrassing examples, prejudicial to your theorem' (G. Darboux [1874]).

[3] 'I am terrified by the hoard of implicit lemmas. It will take a lot of work to get rid of them' (G. Darboux [1883]).

[4] See Part II, 125 and 133

the theorem as lemmas. And there is *no* difference between exception-barring and your proof-analysing. The only safeguard against such *ad hoc* methods is to use *suitable* lemmas, i.e. lemmas in accordance with the spirit of the thought-experiment! Or would you drop the beauty of the proofs from mathematics and replace it by a silly formal game?

GAMMA: Better than your 'spirit of the thoughtexperiment'! I am defending the objectivity of mathematics against your psychologism.

ALPHA: Thank you, Lambda, you restated my case: one does not *invent* a new lemma out of the blue to cope with a global but not local counterexample: rather, one inspects the proof with increased care and *discovers* the lemma there. So I did not, dear Theta, 'make up' hidden lemmas, and I did not, dear Kappa, 'smuggle' them into the proof. The proof contains all of them—but a mature mathematician understands the entire proof from a brief outline. We should not confuse *infallible proof* with *inexact proof-analysis*. There is still the irrefutable master-theorem: '*All polyhedra on which one can perform the thought-experiment*, or briefly, *all Cauchy-polyhedra, are Eulerian*.' My approximate proof-analysis drew the borderline of the class of Cauchy-polyhedra with a pencil that—I admit—was not particularly sharp. Now eccentric counterexamples teach us to sharpen our pencil. But first: *no pencil is absolutely sharp* (and if we overdo sharpening it will break); secondly, *pencil-sharpening is not creative mathematics*.

GAMMA: I am lost. What *is* your position? First you were a champion of refutations.

ALPHA: Oh, my growing pains! Mature intuition brushes controversy aside.

GAMMA: Your first mature intuition led you to your 'perfect proof-analysis'. You thought that your 'pencil' was absolutely sharp.

ALPHA: I forgot about the difficulties of linguistic communication—especially with pedants and sceptics. But the heart of mathematics is the thought-experiment—the proof. Its linguistic articulation—the proof-analysis—is necessary for communication but irrelevant. I am interested in polyhedra, you in language. Don't you see the poverty of your counterexamples? They are linguistic, not polyhedral.

GAMMA: Then refuting a theorem only betrays our failure to grasp the hidden lemmas in it? So a 'theorem' is meaningless unless we understand its proof?

ALPHA: Since the vagueness of language makes the *rigour of proof-analysis* unattainable, and turns theorem-formation into an unending process, why bother about the theorem? Working mathematicians certainly do not. If yet another petty ' counterexample ' is produced they do not admit that their theorem is refuted, but at most that its ' domain of validity ' should be suitable narrowed down.

LAMBDA: So you are not interested either in counterexamples, or in proof-analysis, or in lemma-incorporation?

ALPHA: That is right. I reject all your rules. I propose one single rule instead: *Construct rigorous (crystal-clear) proofs.*

LAMBDA: You argue that the *rigour of proof-analysis* is unattainable. Is *the rigour of proof* attainable? Cannot ' crystal-clear ' thought-experiments lead to paradoxical or even contradictory results?

ALPHA: Language is vague, but thought can achieve absolute rigour.

LAMBDA: But surely ' at each stage of the evolution our fathers also thought they had reached it? If they deceived themselves, do we not likewise cheat ourselves? '[1]

ALPHA: ' Today absolute rigour is attained.'[2]

[*Giggling in the classroom.*[3]]

GAMMA: This theory of ' crystal-clear ' proof is sheer psychologism![4]

[1] Poincaré [1905], p. 214

[2] *Ibid.* p. 216. Changes in the criterion of ' rigour of the proof ' engender major revolutions in mathematics. Pythagoreans held that rigorous proofs can only be arithmetical. They however discovered a rigorous proof that $\sqrt{2}$ was ' irrational '. When this scandal eventually leaked out, the Criterion was changed: arithmetical ' intuition ' was discredited and geometrical intuition took its place. This meant a major and complicated reorganisation of mathematical knowledge (e.g. the theory of proportions). In the eighteenth century ' misleading ' figures brought geometrical proofs into disrepute, and the nineteenth century saw arithmetical intuition re-enthroned with the help of the cumbersome theory of real numbers. Today the main dispute is about what is rigorous and what not in set-theoretical and meta-mathematical proofs, as shown by the well-known discussions about the admissibility of Zermelo's and Gentzen's thoughtexperiments.

[3] As was already pointed out, the class is very advanced.

[4] The term ' psychologism ' was coined by Husserl ([1900]). For an earlier ' criticism ' of psychologism see Frege [1893], pp. xv-xvi. Modern intuitionists (unlike Alpha) openly embrace psychologism: ' A mathematical theorem expresses a purely empirical fact, namely the success of a certain construction . . . mathematics is a study of certain functions of the human mind ' (Heyting [1956], pp. 8 and 10). How they reconcile psychologism with certainty is their well-kept secret.

ALPHA: Better than the logico-linguistic pedantry of your proof-analysis![1]

LAMBDA: Swearwords apart, I too am sceptical about your conception of mathematics as 'an essentially languageless activity of the mind'.[2] How can an activity be true or false? Only *articulated* thought can try for truth. Proof cannot be enough: we also have to state what the proof proved. The proof is only a stage of the mathematician's work which has to be followed by proof-analysis and refutations and concluded by the rigorous theorem. We have to *combine* the 'rigour of proof' with the 'rigour of proof-analysis'.

ALPHA: Are you still hoping that at the end you will arrive at a perfectly rigorous proof-analysis? If so, tell me why you did not start by formulating your new theorem 'stimulated' by the cylinder? You only indicated it. Its length and clumsiness would have made us laugh in despair. And this only after the *first* of your new counterexamples! You replaced our original theorem by a succession of ever more precise theorems—but only *in theory*. What about the *practice* of this relativisation? Ever more eccentric counterexamples will be countered by ever more trivial lemmas—yielding a 'vicious infinity'[3] of ever longer and clumsier theorems.[4] If criticism was felt to be invigorating while it seemed to lead to truth, now it is certainly frustrating when it destroys any truth whatsoever and drives us

[1] That even if we had perfect knowledge we could not perfectly articulate it, was a commonplace for ancient sceptics (see Sextus Empiricus [*c.* 195], I. 83-87), but was forgotten in the Enlightenment. It was rediscovered by the intuitionists: they accepted Kant's philosophy of mathematics but pointed out that 'between the perfection of mathematics proper and the perfection of mathematical language no clear connection can be seen' (Brouwer [1952], p. 140). 'Expression by spoken or written word—though necessary for communication—is never adequate . . . The task of science is not to study languages, but to create ideas' (Heyting [1939], pp. 74-75).

[2] Brouwer [1952], p. 141

[3] English has the term '*infinite regress*', but this is only a *special* case of 'vicious infinity' (*schlechte Unendlichkeit*) and would not apply here. Alpha obviously coined this phrase with '*vicious circle*' in mind.

[4] Usually mathematicians avoid long theorems by the alternative device of long definitions, so that in the theorems only the defined terms (e.g. 'ordinary polyhedron') appear—this is more economical since one definition abbreviates many theorems. Even so, the definitions take up enormous space in 'rigorous' expositions, though the monsters which lead to them are seldom mentioned. The definition of an '*Euler polyhedron*' (with the definitions of some of the defining terms) takes about 25 lines in Forder [1927] (pp. 67 and 29); the definition of '*ordinary polyhedron*' in the 1962 edition of the *Encyclopaedia Britannica* fills 45 lines.

endlessly without purpose. I stop this vicious infinity in *thought*—you will never stop it in *language*.

GAMMA: But I never said that there have to be *infinitely many* counterexamples. At a certain point we may reach truth and then the flow of refutations will stop. But of course we shall not know when. Only refutations are conclusive—proofs are a matter of psychology.[1]

LAMBDA: I still trust that the light of absolute certainty will flash up when refutations peter out!

KAPPA: But will they? What if God created polyhedra so that all true universal statements about them—formulated in human language—are infinitely long? Is it not blasphemous anthropomorphism to assume that (divine) true theorems are of finite length?

Be frank: for some reason or other you are all bored with refutations and piecemeal theorem-formation. Why not call it a day and stop the game? You already gave up '*Quod erat demonstrandum*'. Why not give up '*Quod erat demonstratum*' too? Truth is only for God.

THETA [*aside*]: A religious sceptic is the worst enemy of science!

SIGMA: Let's not overdramatise! After all, only a narrow penumbra of vagueness is at stake. It is simply that, as I said before, *not all propositions are true or false*. There is a third class which I would now call '*more or less rigorous*'.

THETA [*aside*]: Three-valued logic—the end of critical rationality!

SIGMA: . . . and we state their domain of validity with a rigour that is more or less adequate.

ALPHA: Adequate for what?

SIGMA: Adequate for the solution of the problem which we want to solve.

THETA [*aside*]: Pragmatism! Has everybody lost interest in *truth*?

KAPPA: Or adequate for the *Zeitgeist*! 'Sufficient unto the day is the rigour thereof.'[2]

THETA: Historicism! [*Faints*].

ALPHA: Lambda's rules for '*rigorous proof-analysis*' deprive mathematics of its beauty, present us with the hairsplitting pedantry of long, clumsy theorems filling dull thick books, and will eventually land us in vicious infinity. Kappa's escape-route is convention, Sigma's mathematical pragmatism. What a choice for a rationalist!

GAMMA: So a rationalist ought to relish Alpha's '*rigorous proofs*', inarticulate intuition, 'hidden lemmas', derision of the Principle of

[1] Logic makes us reject certain arguments, but it cannot make us believe any argument' (Lebesgue [1928], p. 328).

[2] E. H. Moore [1902], p. 411

Retransmission of Falsity, and elimination of refutations? Should mathematics have no relation to criticism and logic? [1]

BETA: Whatever the case, I am fed up with all this inconclusive verbal quibble. I want to do mathematics and I am not interested in the philosophical difficulties of justifying its foundations. Even if reason fails to provide such justification my natural instinct reassures me.[1]

I understand Omega has an interesting collection of alternative proofs—I would rather listen to him.

OMEGA: But I shall put them into a ' philosophical ' framework!

BETA: I don't mind packing if there is something else in the packet.

Note. In this section I have tried to show how the emergence of mathematical criticism has been the driving force in the search for the ' foundations ' of mathematics.

The distinction that we made between *proof* and *proof-analysis* and the corresponding distinction between the *rigour of proof* and the *rigour of proof-analysis* seems to be crucial. About 1800 the *rigour of proof* (crystal-clear thought experiment or construction) was contrasted with muddled argument and inductive generalisation. This was what Euler meant by ' *rigida demonstratio* ', and Kant's idea of infallible mathematics too was based on this concept (see his paradigm case of a mathematical proof in his [1781], pp. 716-717). It was also thought that one proves what one has set out to prove. It did not occur to anybody that the verbal articulation of a thought-experiment involves any real difficulty. Aristotelian formal logic and mathematics were two completely separate disciplines—mathematicians considered the former as utterly useless. The proof or thoughtexperiment carried full conviction without any deductive pattern or ' logical ' structure.

In the early nineteenth century the flood of counterexamples brought confusion. Since proofs were crystal-clear, refutations had to be miraculous freaks, to be completely segregated from the indubitable proofs. *Cauchy's revolution of rigour* rested on the heuristic innovation that the mathematician should not stop at the proof: he should go on and find out what he has proved by enumerating the exceptions, or rather by stating a safe domain where the proof is valid. *But Cauchy—or Abel—did not see any connection*

[1] ' Nature confutes the sceptics, reason confutes the dogmatists ' (Pascal [1654], 432). Few mathematicians would confess—like Beta—that reason is too weak to justify itself. Most of them adopt some brand of dogmatism, historicism or confused pragmatism and remain curiously blind to its untenability; for example: ' Mathematical truths are in fact *the prototype of the completely incontestable*. . . . But the rigor of maths is not absolute; it is in a process of continual development; the *principles of maths have not congealed once and for all* but have a life of their own and may even be the subject of scientific quarrels '(A. D. Alexandrov [1947], p. 7). (This quotation may remind us that dialectic tries to account for change without using criticism: truths are in ' in continual development ' but always ' completely incontestable '.)

between the two problems. It never occurred to them that if they discover an exception, they should have another look at the proof. (Others practised monster-barring, monster-adjustment or even ' turning a blind eye '—but all agreed that the proof was taboo and had nothing to do with the ' exceptions '.)

The nineteenth century union of logic and mathematics had two main sources: Non-Euclidean geometry and the *Weierstrassian revolution of rigour.* They brought about the integration of proof (thoughtexperiment) and refutations and started to develop *proof-analysis,* gradually introducing deductive patterns in the proof-thoughtexperiment. What we called the ' method of proof and refutations ' was their heuristic innovation: *it united logic and mathematics for the first time.* Weierstrassian rigour triumphed over its reactionary monster-barring and lemma-hiding opponents who used slogans like ' the dullness of rigour ', ' artificiality versus beauty ', etc. *The rigour of proof-analysis superseded the rigour of proof*; but most mathematicians put up with its pedantry only so long as it promised them complete certainty.

Cantor's set-theory—with yet another crop of unexpected refutations of ' rigorous ' theorems—turned many of the Weierstrassian Old Guard into dogmatists, ever ready to combat the ' anarchists ' by barring the new monsters or referring to ' hidden lemmas ' in their theorems which represented ' the last word in rigour ' while still chastising the older type ' reactionaries ' for like sins.

Then some mathematicians realised that the drive for rigour of proof-analysis in the method of proofs and refutations leads to vicious infinity. An ' intuitionist ' counter-revolution began: the frustrating logico-linguistic pedantry of *proof-analysis* was condemned, and new extremist standards of rigour were invented for *proofs*; mathematics and logic were divorced once more.

Logicists tried to save the marriage and foundered on the paradoxes. Hilbertian rigour turned mathematics into a cobweb of *proof-analyses* and claimed to stop their infinite regresses by crystal-clear consistency *proofs* of his intuitionistic metatheory. The ' foundational layer ', the region of uncriticisable familiarity, was shifted into the thoughtexperiments of metamathematics. (Cf. Lakatos [1962], pp. 179-184).

By each ' revolution of rigour ' proof-analysis penetrated deeper into the proofs down to the *foundational layer* of ' familiar background knowledge ' (also cf. footnote 3, p. 224), where crystal-clear intuition, the rigour of the proof, reigned supreme and criticism was banned. Thus, *different levels of rigour differ only about where they draw the line between the rigour of proof-analysis and the rigour of proof,* i.e. *about where criticism should stop and justification should start.* ' Certainty is never achieved '; ' foundations ' are never found—but the 'cunning of reason' turns each increase in *rigour* into an increase in *content*, in the scope of mathematics. But this story is beyond our present investigation.

6 *Return to the Criticism of the Proof by Counterexamples which are Local but not Global. The Problem of Content.*

(*a*) *Increasing Content by Deeper Proofs*

OMEGA: I like Lambda's method of proof and refutations and I share his faith that somehow we shall finally arrive at a rigorous proof-analysis and thereby at a certainly true theorem. But even so, our very method creates a new problem: *proof-analysis, when increasing certainty, decreases content.* Each new lemma in the proof-analysis, each corresponding new condition in the theorem, reduces its domain. Increasing rigour is applied to a decreasing number of polyhedra. Does lemma-incorporation not repeat the mistake Beta made in playing for safety? Could we too 'have withdrawn too radically, leaving lots of Eulerian polyhedra outside the walls?' [1] In both cases we may throw the baby out with the bathwater. *We should have a counterweight against the content-decreasing pressure of rigour.*

We have already made a few steps in this direction. Let me remind you of two cases and re-examine them.

One was when we first came across local but not global counterexamples.[2] Gamma refuted the third lemma in our first proof-analysis (that 'in removing triangles from the flat triangulated network we have only two possibilities: either we remove an edge or we remove two edges and a vertex'). He removed a triangle from the middle of the network without removing a single edge or vertex.

We then had two possibilities.[3] The *first* was to incorporate the false lemma into the theorem. This would have been a perfectly proper procedure as far as certainty is concerned, but would have reduced the domain of the theorem so drastically that it would have applied only for the tetrahedron. Together with the counterexamples we would have thrown out all the examples but one.

This was the rationale behind our adoption of the alternative: instead of *narrowing* the domain of the theorem by lemma-incorporation, we *widened* it by replacing the falsified lemma by an unfalsified one. But this vital pattern for theorem-formation was soon forgotten and Lambda did not bother to formulate it as a heuristic rule. It should be:

[1] Part II, p. 125 [2] For the discussion of this first case see Part I, pp. 11-14.

[3] Omega seems to ignore a third possibility: Gamma may very well claim that since local but not global counterexamples do not show up any violation of the principle of retransmission of falsity, there is no action to be taken.

Rule 4. *If you have a counterexample which is local but not global try to improve your proof-analysis by replacing the refuted lemma by an unfalsified one.*

Counterexamples of the first type (local but not global) may provide an opportunity of *increasing* the content of our theorem which is constantly being *reduced* under the pressure of counterexamples of the third type (global but not local).

GAMMA: *Rule* 4 shows up again the weakness of Alpha's now discarded ' perfect proof-analysing intuition '.[1] He would have listed the suspicious lemmas, incorporated them immediately and—without caring for counterexamples—formed near-empty theorems.

TEACHER: Omega, let us hear the second example you promised.

OMEGA: In Beta's proof-analysis the second lemma was that ' *all faces are triangular* '.[2] This can be falsified by a number of local but not global counterexamples, e.g. by the cube or the dodecahedron. Therefore you, Sir, replaced it by a lemma which is not falsified by them, namely that ' *any face dissected by a diagonal edge falls into two pieces* '. But instead of invoking *Rule* 4 you rebuked Beta for ' careless proof-analysis '. You will admit that *Rule* 4 is better advice than just ' be more careful '.

BETA: You are right, Gamma, and you also make me understand better ' the method of the best sort of exceptionbarrers '.[3] They start with a cautious, ' safe ' proof-analysis and systematically applying *Rule 4* they gradually build up the theorem without uttering a falsehood. After all, it is a matter of temperament whether one approaches truth through ever false overstatements or through ever true understatements.

OMEGA: That may be right. But one can interpret *Rule 4* in two ways. Hitherto we considered only the first, weaker interpretation: ' one *easily* elaborates, improves the proof by replacing the false lemma by a *slightly modified* one which the counterexample will not refute ';[4] all that one needs for this is a ' more careful ' inspection of the proof and a ' trifling observation '.[5] On this interpretation *Rule 4* is just local patching *within the framework of the original proof.*

I allow also for the alternative, radical interpretation: to replace the lemma—or possibly all the lemmas—not only by trying to squeeze out the last drop of content from the given proof, but possibly by inventing a completely different, more embracing, *deeper* proof.

TEACHER: For example?

[1] Cf. pp. 225–6. [2] For the discussion of this second case cf. Part II, pp.132–4
[3] See *Part II*, pp. 134–135 [4] Part I, p. 12 [5] *Ibid.*

Omega: I discussed the Descartes-Euler conjecture earlier with a friend who immediately offered a proof, as follows: let us imagine the polyhedron to be hollow, with a surface made of any rigid material, say cardboard. The edges must be clearly painted on its inside. Let the inside be well illuminated, and let one of the faces be the lens of an ordinary camera—that face from which I can take a snapshot showing all edges and vertices.

Sigma [*aside*]: A camera in a mathematical proof?

Omega: So I get a picture of a plane network, which can be dealt with just like the plane network in your proof. Also in the same way, I can show that, if the faces are simply-connected, $V - E + F = 1$, and adding the lens-face which is invisible on the photo, I get Euler's formula. The main lemma is that there is a face of the polyhedron which, if transformed into the lens of a camera, photographs the inside of the polyhedron so that all the edges and all the vertices are on the film. Now I introduce the following abbreviation: instead of 'a polyhedron which has at least one face from which we can photograph *all* the inside', I shall say 'a quasi-convex polyhedron'.

Beta: So your theorem will be: All quasi-convex polyhedra with simply-connected faces are Eulerian.

Omega: For brevity and to give credit to the inventor of this particular proof-idea I would rather say: '*All Gergonne-polyhedra are Eulerian*'.[1]

Gamma: But there are many simple polyhedra which, although perfectly Eulerian, are so badly indented that they have no face from which the whole of the inside can be photographed! Gergonne's proof is not deeper than Cauchy's—it is Cauchy's that is deeper than Gergonne's!

Omega: Of course! I suppose Teacher knew about Gergonne's proof, found out that it was unsatisfactory by some local but not

[1] Gergonne's proof is to be found in Lhuilier [1812–13], pp. 177–9. In the original it could not of course contain photographic devices. It says: 'Take a polyhedron, one of its faces being transparent; and imagine that the eye approaches this face from the outside, so closely, that it can perceive the inside of all the other faces . . .' Gergonne points out modestly that Cauchy's proof is deeper, it 'has the valuable advantage that it does not assume convexity at all'. (It does not occur to him however to ask what it *does* assume.) Jacob Steiner later rediscovered essentially the same proof ([1826]). His attention was then called to Gergonne's priority, so he read Lhuilier's paper with the list of exceptions but this did not prevent him from concluding his proof with the 'theorem': '*All polyhedra are Eulerian*'. (It was Steiner's paper that provoked Hessel—the Lhuilier of the Germans—to write his [1832]).

global counterexample and replaced the optical—photographing—lemma by the wider topological—stretching—lemma. Thereby, he arrived at the *deeper* Cauchy proof, not by a 'careful proof-analysis' followed by a slight alteration, but by a radical, imaginative innovation.

TEACHER: I accept your example—but I did not know about Gergonne's proof. But if you did, why did you not tell us about it?

OMEGA: Because I immediately refuted it by non-Gergonnian polyhedra that were Eulerian.

GAMMA: As I have just said, I too found such polyhedra. But is that a reason for scrapping the proof altogether?

OMEGA: I think so.

TEACHER: Have you heard of Legendre's proof? Would you scrap that too?

OMEGA: I certainly would. It is still less satisfactory: its content is even poorer than Gergonne's proof. His thought-experiment started by mapping the polyhedron with a central projection on to a sphere containing the polyhedron. The radius of the sphere he chose as 1. He chose the centre of the projection so that the sphere will be covered completely, once but only once, by a network of spherical polygons. So his first lemma was that such a point exists. His second lemma was that for the polyhedral network on the sphere $V - E + F = 2$—but this he succeeded in decomposing into trivially true lemmas of spherical trigonometry. But a point from which such a central projection is possible exists only in convex and a few decent 'almost-convex' polyhedra—a class narrower even than that of 'quasi-convex' polyhedra. But this theorem: '*All Legendre-polyhedra are Eulerian*'[1] differs *completely* from that of Cauchy, but

[1] *Legendre's proof* can be found in his [1794], but not the proof-generated *theorem*, since proof-analysis and theorem-formation were virtually unknown in the 18th century. Legendre first defines polyhedra as solids whose surface consists of polygonal faces (p. 161). Then he proves $V - E + F = 2$ *in general* (p. 228). But there is an exception-barring amendment in a note in fine print on p. 164, saying that only *convex* polyhedra will be considered. He ignored the almost convex fringe. Poinsot was first, in his [1809], to notice when commenting on Legendre's proof, that the Euler formula 'is valid not only for ordinary convex solids, namely, for those whose surface is cut by a straight line in no more than two points: it also holds for polyhedra with re-entrant angles, provided one can find a point in the interior of the solid which serves as the centre of a sphere on to which one can project the faces of the polyhedron by lines leading from the centre, so that the projected faces do not overlap. This applies to an infinity of polyhedra with re-entrant angles. In fact, Legendre's proof applies, as it stands, to all these additional polyhedra' (p. 46).

only for the worse. It is ' unfortunately incomplete '.[1] It is a ' vain effort which presupposes conditions on which the Euler theorem does not depend at all. It has to be scrapped and one has to look for more general principles '.[2]

BETA: Omega is right. ' Convexity is to a certain extent accidental for Eulerianness. A convex polyhedron might be transformed, for example by a dent or by pushing in one or more of the vertices, into a non-convex polyhedron with the same configurational numbers. Euler's relation corresponds to something more fundamental than convexity.'[3] And you will never capture that by your ' almost ' and ' quasi- ' frills.

OMEGA: I thought Teacher had captured it in the topological principles of the Cauchy proof in which *all* the lemmas of Legendre's proof are replaced by completely new ones. But then I stumbled upon a polyhedron that refuted even this proof which is certainly the deepest hitherto.

TEACHER: Let us hear about it.

OMEGA: You all remember Gamma's ' urchin' (Fig. 7). That was of course non-Eulerian. But not all star-polyhedra are non-Eulerian!

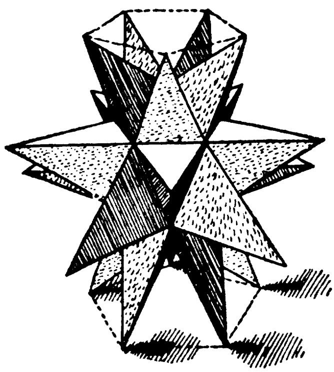

FIG. 15

Take for instance the 'great stellated dodecahedron ', (Fig. 15). It consists, like the ' small stellated dodecahedron ' of pentagrams, but

[1] E. de Jonquières goes on, again lifting an argument from Poinsot's [1858]: ' In invoking Legendre, and like high authorities, one only fosters a widely spread prejudice that has captured even some of the best intellects: that the domain of validity of the Euler theorem consists only of convex polyhedra ' ([1890a], p. 111).

[2] This is from Poinsot ([1858], p. 70).

[3] D. M. Y. Sommerville ([1929], pp. 143-4)

differently arranged. It has 12 faces, 30 edges and 20 vertices, so that $V - E + F = 2$.[1]

TEACHER: Do you then reject our proof?

OMEGA: I do. The satisfactory proof has to explain the Eulerianness also of the ' great stellated dodecahedron '.

RHO: Why not admit that your ' great stellated dodecahedron ' is triangular? Your difficulties are imaginary.

DELTA: I agree. But they are imaginary for a different reason. I have taken to star-polyhedra now: they are fascinating. But they are, I am afraid, essentially different from ordinary polyhedra: therefore one cannot possibly conceive a proof that would explain the Eulerian character of, say, the cube, *and* of the ' great stellated dodecahedron ' by one single idea.

OMEGA: Why not? You have no imagination. Would you have insisted after Gergonne's and before Cauchy's proof that concave and convex polyhedra are essentially different: therefore one cannot possibly conceive of a proof that would explain the Eulerian character of convex and concave polyhedra by one single idea? Let me quote from Galileo's *Dialogues*:

> SAGREDO: So as you see, all planets and satellites—let us call them all ' planets '—are moving in ellipses.
>
> SALVIATI: I am afraid there are planets moving in parabolas. Look at this stone. I throw it away: it moves along a parabola.
>
> SIMPLICIO: But this stone is not a planet! These are two quite separate phenomena!
>
> SALVIATI: Of course this stone is a planet, only thrown with a less mighty hand than that one which launched the Moon.
>
> SIMPLICIO: Nonsense! How can you dare to pool under one head heavenly and earthly phenomena? One has nothing to do with the other! Of course both may be explained by proofs, but I surely expect the two explanations to be totally different! I cannot imagine a proof which should explain the course of a planet in heaven and a projectile on the earth by one single idea!
>
> SALVIATI: You cannot imagine it but I can devise it . . .[2]

TEACHER: Never mind projectiles and planets, Omega, have you succeeded in finding a proof to embrace both ordinary Eulerian polyhedra and Eulerian star-polyhedra?

[1] This ' great stellated dodecahedron ' has already been devised by Kepler ([1619], p. 53), then independently, by Poinsot ([1809]), who first tested if for Eulerianness. Fig. 15 is copied from Kepler's book.

[2] I was unable to trace this quotation.

OMEGA: I have not. But I shall.[1]

LAMBDA: Say you do—what is the matter with Cauchy's proof? You must explain why you reject one proof after the other.

(b) Drive towards final proofs and corresponding sufficient and necessary conditions.

OMEGA: You criticised proof-analyses for the breakdown of the *retransmission of* falsity by counterexamples of the *third* type. Now I criticise them for the breakdown of the *transmission* of falsity (or what amounts to the same, the *retransmission of truth*) by counterexamples of the *second* type. A proof must explain the phenomenon of Eulerianness in its entire range.

My quest is not only for *certainty* but also for *finality*. The theorem has to be certain—there must not be any counterexamples *within* its domain; but it has also to be *final*: there must not be any examples *outside* its domain. I want to draw a dividing line between examples and counterexamples, and not just between a safe domain of a few examples on the one hand and a mixed bag of examples and counterexamples on the other.

LAMBDA: Or, you want the conditions of the theorem to be not only sufficient, but also necessary!

KAPPA: Let us imagine then, for the sake of the argument, that you found such a master-theorem: '*All master-polyhedra are Eulerian*'. Do you realise that this theorem will only be 'final' if the converse theorem: '*All Eulerian polyhedra are master-polyhedra*' is certain?

OMEGA: Of course.

KAPPA: That is, if certainty gets lost in vicious infinity, so will finality? You will find at least one Eulerian polyhedron outside the domain of each of your ever deeper proofs.

OMEGA: Of course I know that I cannot solve the problem of finality without solving the problem of certainty. I am sure we shall solve both. We shall stop the infinite spate of counterexamples both of the first and the third types.

TEACHER: Your search for increasing content is very important. But why not accept your second criterion of satisfactoriness—finality—as *mandatory* but not *obligatory*? Why reject interesting proofs that do not contain both sufficient and necessary conditions? Why regard them as refuted?

[1] Cf. footnote 1, p. 244.

OMEGA: Well . . .[1]

LAMBDA: Whatever the case, Omega certainly convinced me that a single proof may not be enough for the critical improvement of a naive conjecture. Our method should include the radical version of his *Rule* 4, and then it should be called the method of '*proofs and refutations*' instead of '*proof and refutations*'.

MU: Excuse my butting in, I have just translated the results of your discussion into quasi-topological terms: The lemma-incorporating method yielded a contracting sequence of the nested *domains of successive improved theorems*; these domains shrank under the continued attack of global counterexamples in the course of the emergence of hidden lemmas and converged to a *limit*: let us call this limit the '*domain of the proof-analysis*'. If we apply the weaker version of *Rule* 4, this domain can be widened under the continued pressure of local counterexamples. This expanding sequence again will have a limit: I shall call it the '*domain of the proof*'. The discussion then has shown that even this limit domain may be too narrow (perhaps even empty). We may have to devise *deeper* proofs whose domains will form an *expanding* sequence, including more and more recalcitrant Eulerian polyhedra which were local counterexamples to previous proofs. These domains, themselves limit-domains, will converge to the double limit of the '*domain of the naive conjecture*'—which is after all the aim of the inquiry.

The topology of this heuristic space will be a problem for mathematical philosophy: will the sequences be infinite, will they converge at all, attain the limit, may the limit be the empty set?

EPSILON: I found a deeper proof than Cauchy's which explains also the Eulerianness of Omega's 'great stellated dodecahedron'! [*Passes a note to the Teacher.*]

OMEGA: The final proof! The true essence of Eulerianness will now be revealed!

[1] The answer is in the celebrated Pappian heuristic of antiquity which applied only to the discovery of 'final', 'ultimate' truths, i.e. to theorems which contained both necessary and sufficient conditions. For 'problems to prove' the main rule of this heuristic was: 'If you have a conjecture, derive consequences from it. If you arrive at a consequence known to be false, the conjecture was false. If you arrive at a consequence known to be true, reverse the order and, if the conjecture can be thus derived from this true consequence, then it was true.' (Cf. Heath [1925], I, pp. 138-139.) The principle '*causa aequat effectu*' and the quest for theorems with necessary and sufficient conditions were both in this tradition. It was only in the seventeenth century—when all the efforts to apply Pappian heuristic to modern science had failed—that the quest for certainty came to prevail over the quest for finality.

TEACHER: I am sorry, time is running short: we shall have to discuss Epsilon's very sophisticated proof some other time.[1] All I do see is that it will not be final in Omega's sense. Yes, Beta?

(c) *Different proofs yield different theorems*

BETA: The most interesting point I have learned from this discussion is that different proofs of the same naive conjecture lead to quite different theorems. *The one Descartes-Euler conjecture is improved by each proof into a different theorem.* Our original proof yielded: '*All Cauchy-polyhedra are Eulerian.*' Now we have learned about two completely different theorems: '*All Gergonne-polyhedra are Eulerian*' and '*All Legendre-polyhedra are Eulerian*'. Three proofs, three theorems with one common ancestor.[2] The usual expression '*different proofs of the Euler theorem*' is then confusing, for it conceals the vital role of proofs in theorem-formation.[3]

[1] The proof is Poincaré's (cf. his [1893] and [1900]).

[2] There are many other proofs of the Euler conjecture. For a detailed heuristic discussion of Euler's, Jordan's and Poincaré's proofs see Lakatos [1961].

[3] Poinsot, Lhuilier, Cauchy, Steiner, Crelle all thought that the different proofs prove the same theorem: the '*Euler-theorem*'. To quote a characteristic sentence from a standard textbook: 'The theorem stems from Euler, the first proof from Legendre, the second from Cauchy' (Crelle [1827], II, p. 671).

Poinsot came very near to noticing the difference when he observed that Legendre's proof applied to more than just ordinary convex polyhedra. (See footnote 1 on p. 239.) But when he then compared Legendre's proof with Euler's proof (that one which was based on cutting off pyramidal corners of the polyhedron and arriving at a final tetrahedron without changing the Euler-characteristic [1751]) he gave preference to Legendre's on the ground of 'simplicity'. 'Simplicity' stands here for the eighteenth-century idea of rigour: clarity in the thoughtexperiment. It did not occur to him to compare the two proofs for *content*: then Euler's proof would have turned out to be superior. (As a matter of fact, there is nothing wrong with Euler's proof. Legendre applied the subjective standard of contemporary rigour and neglected the objective one of content).

Lhuilier—in a surreptitious criticism of this passage (he does not mention Poinsot)—points out that Legendre's simplicity is only 'apparent', for it presumes considerable background knowledge in spherical trigonometry ([1812-13], p. 171). But Lhuilier too believes that Legendre '*proved the same theorem*' as Euler (ibid. p. 170).

Jacob Steiner joins him in the appraisal of Legend e's proof and in assuming that all proofs prove the same theorem ([1826]). The only difference is that while according to Steiner all the different proofs prove that '*all polyhedra are Eulerian*', according to Lhuilier all the different proofs prove that '*all polyhedra that have no tunnels, cavities and ringshaped faces are Eulerian*'.

Cauchy wrote his [1811] on polyhedra when he was in his early twenties, years before his revolution of rigour and one cannot take it amiss that he repeats Poinsot's

PI: The difference between the different proofs goes much deeper. Only the naive conjecture is about polyhedra. The theorems are about Cauchy-objects, Gergonnian objects, Legendrian objects respectively, but not any more about polyhedra.

BETA: Are you trying to be funny?

PI: No, I shall explain my point. But I would do this in a wider context—I want to discuss *concept-formation* in general.

ZETA: We should rather first discuss *content.* I found Omega's *Rule* 4 very weak—even in his radical interpretation.[2]

TEACHER: Right. Let us then first hear Beta's approach to the problem of content and then wind up our debate with a discussion of concept-formation.

comparison of Euler's and Legendre's proofs in the introduction to the second part of his treatise. He—like most of his contemporaries—did not grasp the difference in depth of different proofs and so could not appreciate the real power of his own proof. He thought he had just given yet *another proof of the very same theorem*—but he was rather eager to stress that he had arrived at a rather trivial generalisation of the Euler-formula to certain aggregates of polyhedra.

Gergonne was the first to appreciate the unrivalled depth of Cauchy's proof (Lhuilier [1812-13], p. 179).

[2] See p. 237.

(*To be continued*)