Proofs and Refutations (IV)

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PROOFS AND REFUTATIONS (IV) *

I. LAKATOS

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7 *The Problem of Content Revisited*

(*a*) *The naiveté of the naive conjecture*

ZETA: I agree with Omega in deploring the fact that monster-barrers, exceptionbarrers and lemma-incorporators all strove for certain truth at the expense of content. But his *Rule 4*,[1] demanding deeper proofs of the same naive conjecture, is not enough. Why should our search for content be delimited by the first naive conjecture we stumble upon? Why should the aim of our enquiry be the 'domain of the naive conjecture'?

OMEGA: I don't follow you. Surely our problem was to discover the domain of truth of $V-E+F=2$?

* Parts I, II and III appeared in the preceding numbers.
[1] See Part III, p. 237

ZETA: It was not! Our problem was to find out the relation between V, E and F for any polyhedron whatsoever. It was a sheer accident that we first got familiar with polyhedra for which $V-E+F = 2$. But a critical inquiry into these 'Eulerian' polyhedra showed us that there are many more non-Eulerian than Eulerian polyhedra. Why not look for the domain of $V-E+F=-6$, $V-E+F=28$ or $V-E+F=0$? Aren't they equally interesting?

SIGMA: You are right. We paid so much attention to $V-E+F = 2$ only because we originally thought it was true. Now we know it is not—we have to find *a new, deeper naive conjecture* . . .

ZETA: . . . that will be less naive. . . .

SIGMA: . . . that will be a relation between V, E, and F for *any* polyhedron.

OMEGA: Why rush? Let us first solve the more modest problem that we set out to solve: to explain why some polyhedra are Eulerian. Until now we have arrived only at partial explanations. For instance, none of the proofs found has explained why a picture-frame with ringshaped faces both in the front and in the back is Eulerian (Fig. 16). It has 16 vertices, 24 edges and 8 faces. . . .

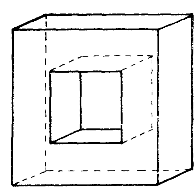


FIG. 16

THETA: It is certainly not a Cauchy-polyhedron: it has a tunnel, it has ringshaped faces. . . .

BETA: And yet Eulerian! How irrational! Is a polyhedron guilty of a single fault—a tunnel without ringshaped faces (Fig. 9)—to be cast out among the goats, yet one which offends in twice as many ways—having also ringshaped faces (Fig. 16)—admitted to the sheep?[1]

OMEGA: You see, Zeta, we have enough puzzles about Eulerian polyhedra. Let us solve them before we go on to a more general problem.

ZETA: No, Omega. 'More questions may be easier to answer than just one question. A new more ambitious problem may be easier to handle than the original problem.'[2] Indeed, I shall show you that your narrow, accidental problem can only be solved by solving the wider, essential problem.

OMEGA: But I want to discover the secret of Eulerianness!

[1] The problem was noticed by Lhuilier ([1812-13], p. 189) and, independently, by Hessel [1832]. In Hessel's paper the figures of the two picture-frames appear next to each other. Also cf. p. 309 footnote 1.

[2] Pólya calls this the 'inventor's paradox' ([1945], p. 110).

ZETA: I understand your resistance. You have fallen in love with the problem of finding out where God drew the firmament dividing Eulerian from non-Eulerian polyhedra. But there is no reason to believe that the term 'Eulerian' occurred in God's blueprint of the universe at all. What if Eulerianness is merely an accidental property of some polyhedra? In this case it would be uninteresting or even impossible to find out the random zigzags of the demarcation line between Eulerian and non-Eulerian polyhedra. Such an admission however would leave rationalism unsullied, for Eulerianness is then not part of the rational design of the universe. So let us forget about it. One of the main points about critical rationalism is that one is always prepared to abandon one's original problem in the course of the solution and replace it by another one.

(b) Induction as the basis of the method of proofs and refutations

SIGMA: Zeta is right. What a disaster!

ZETA: Disaster?

SIGMA: Yes. You now want a new 'naive conjecture' about the relation between V, E and F, for *any* polyhedron, don't you? Impossible! Look at the vast crowd of counterexamples. Polyhedra with cavities, polyhedra with ringshaped faces, with tunnels, joined together at edges, vertices . . . $V-E+F$ can take any value whatsoever! You cannot possibly recognise any order in this chaos! We have left the firm ground of Eulerian polyhedra for a swamp! We have irretrievably lost a naive conjecture and have no hope of getting another one!

ZETA: But . . .

BETA: Why not? Remember the seemingly hopeless chaos in our table of the numbers of vertices, edges and faces even of the most ordinary convex polyhedra:

	Polyhedron	*F*	*V*	*E*
I	cube	6	8	12
II	triangular prism	5	6	9
III	pentagonal prism	7	10	15
IV	square pyramid	5	5	8
V	triangular pyramid	4	4	6
VI	pentagonal pyramid	6	6	10
VII	octahedron	8	6	12
VIII	'tower'	9	9	16
IX	'truncated cube'	7	10	15

We failed so many times to fit them into a formula.[1] But then suddenly the real regularity governing them struck us: $V-E+F=2$.

KAPPA [*aside*]: 'Real regularity'? Funny expression for an utter falsehood.

BETA: All that we have to do now is to complete our table with the data for non-Eulerian polyhedra and look for a new formula: with patient, diligent observation, and some luck, we shall hit on the right one; then we can improve it again by applying the method of proofs and refutations!

ZETA: Patient, diligent observation? Trying one formula after the other? Perhaps you will devise a guessing machine that produces random formulas and tests them against your table? Is this your idea of how science progresses?

BETA: I don't understand your scorn. Surely you agree that our first knowledge, our naive conjectures, can only come from diligent observation and sudden insight, however much our critical method of 'proofs and refutations' takes over once we have *found* a naive conjecture? Any deductive method has to start from an inductive basis!

SIGMA: Your inductive method will never succeed. We only arrived at $V-E+F=2$ because there happened to be no picture-frame or urchin in our original tables. Now that this historical accident. . . .

KAPPA [*aside*]: . . . or God's benevolent guidance. . . .

SIGMA: . . . is no more, you will never 'induce' order from chaos. We started with long observation and lucky insight—and failed. Now you propose to start again with longer observation and luckier insight. Even if we did arrive at a new naive conjecture—which I doubt—we shall only end up in the same mess.

BETA: Perhaps we should give up research altogether? We *have* to start again—first with a new naive conjecture and then going again through the method of proofs and refutations.

ZETA: No, Beta. I agree with Sigma—therefore I shall not start again with a new naive conjecture.

BETA: Then where do you want to *start* if not with an inductive low-level generalisation as a naive conjecture? Or have you an alternative method for starting?

[1] See footnote 3, p. 303. The table has been borrowed from Pólya [1954], Vol. I, p. 36.

(c) Deductive guessing versus naive guessing

ZETA: Start? Why should I *start*? My mind is not empty when I discover (or invent) a problem.

TEACHER: Do not tease Beta. Here is the problem: '*Is there a relation between the number of vertices, edges and faces of polyhedra analogous to the trivial relation between the number of vertices and edges of polygons, namely that* V = E?'[1] How would *you* set about it?

ZETA: First, I have no government grants to conduct an extensive survey of polyhedra, no army of research assistants counting the numbers of their vertices, edges and faces and compiling tables from the data. But even if I had, I should have no patience—or interest—in trying one formula after the other to test whether it fits.

BETA: What then? Will you lie down on your couch, shut your eyes and forget about the data?

ZETA: Exactly. I need an *idea* to start with, but no data whatsoever.

BETA: And where do you get your idea from?

ZETA: It is already there in our minds when we formulate the problem: in fact, it is in the very formulation of the problem.

BETA: What idea?

ZETA: That for a polygon $V = E$.

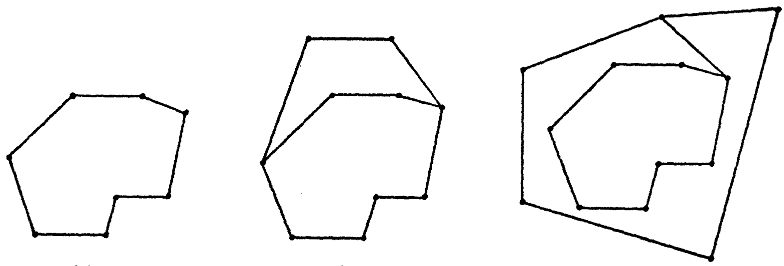
BETA: So what?

ZETA: A problem never comes out of the blue. It is always related to our background knowledge. We know that for polygons $V = E$. Now a polygon is a system of polygons consisting of one single polygon. A polyhedron is a system of polygons consisting of more than a single polygon. But for polyhedra $V \neq E$. At what point did the relation $V = E$ break down in the transition from mono-polygonal systems to polypolygonal systems? Instead of collecting data I trace how the problem grew out of our background knowledge; or, which was the expectation whose refutation presented the problem?

SIGMA: Right. Let us follow your recommendation. For any polygon $E - V = 0$ (Fig. 17*a*). What happens if I fit another polygon to it (not necessarily in the same plane)? The additional polygon has n_1 edges and n_1 vertices; now by fitting it to the original one along a chain of n_1' edges and $n_1' + 1$ vertices we shall increase the number of edges by $n_1 - n_1'$ and the number of vertices by $n_1 - (n_1' + 1)$; that is, in the new 2-polygonal system there will be an excess in the number of

[1] See Part I, p. 7

edges over the number of vertices: $E-V=1$ (Fig. 17*b*; for an unusual but perfectly proper fitting see Fig. 17*c*). 'Fitting' a new face to the system will always increase this excess by one, or, for an F-polygonal system constructed in this way $E-V=F-1$.



(a) (b) (c)

FIG. 17

ZETA: Or, $V-E+F=1$.

LAMBDA: But this is false for most polygonal systems. Take a cube. . . .

SIGMA: But my construction can lead only to 'open' polygonal systems—bounded by a circuit of edges! I can easily extend my thought-experiment to 'closed' polygonal systems, with no such boundary. Such closure can be accomplished by covering an open vase-like polygonal system with a polygon-cover: fitting such a covering polygon will increase F by one without changing V or E. . . .

ZETA: Or, for a closed polygonal system—or closed polyhedron—constructed in this way, $V-E+F+2$: a conjecture which now you have got without 'observing' the number of vertices, edges and faces of a single polyhedron!

LAMBDA: And now you can apply the method of proofs and refutations without an 'inductive starting point'.

ZETA: With the difference that you do not need to devise a proof—the proof is already there! You can go on immediately with refutations, proof-analysis, theorem-formation.

LAMBDA: Then in your method—instead of observations—proof precedes the naive conjecture![1]

ZETA: Well, I shouldn't call a conjecture that has grown out of a proof 'naive'. In my method there is no place for inductive naiveties.

BETA: Objection! You only pushed back the 'naive' inductive start: you start with '$V=E$ for polygons'. Don't you base this on observations?

[1] This is an important qualification to footnote 2, p. 10 in Part I.

ZETA: Like most mathematicians, I cannot count. I just tried to count the edges and vertices of a heptagon: I found first 7 edges and 8 vertices, and then again 8 edges and 7 vertices. . . .

BETA: Joking apart, how *did* you get $V = E$?

ZETA: I was deeply shocked when I first realised that for a triangle $V - E = 0$. I knew of course very well that in an edge $V - E = 1$ (Fig. 18*a*). I also knew that fitting new edges will always result in an

(a) (b) (c)

FIG. 18

FIG. 19

increase by one, both in the number of vertices and edges (Figs. 18*b* and 18*c*). Why, in polygonal edge-systems, does $V - E = 0$? Then I realised that this is because of the transition from an open system of edges (which is bounded by two vertices) to a closed system of edges (which has no such boundary): because we 'cover' the open system up by fitting an edge without adding a new vertex. So I proved, not observed, that $V - E = 0$ for polygons.

BETA: Your ingenuity will not help you. You only pushed back the inductive starting point further: now to the statement that $V - E = 1$ for any edge whatsoever. Did you prove or did you *observe* that?

ZETA: I proved it. I knew of course that for a single vertex $V = 1$ (Fig. 19). My problem was to construct an analogous relation. . . .

BETA [*furious*]: Didn't you *observe* that for a point $V = 1$?

ZETA: Did *you*? [*Aside, to Pi*]: Should I tell him that my 'inductive starting point' was empty space? That I began by 'observing' *nothing*?

LAMBDA: Whatever the case, two points have been made. First Sigma argued that *it is due only to historical accidents that one can arrive at naive inductive conjectures*: when one is faced with a real chaos of facts, one will scarcely be able to fit them into a nice formula. Then Zeta showed that *for the logic of proofs and refutations we need no naive conjecture, no inductivist starting point at all.*

BETA: Objection! What about those celebrated conjectures that have *not* been preceded (or even followed) by proofs, such as the four-colour conjecture that says that four colours are enough to colour any map, or the Goldbach conjecture? It is only by historical accidents that proofs can precede theorems, that Zeta's 'deductive guessing' can take place: otherwise naive inductive conjectures come first.

TEACHER: We certainly have to learn *both* heuristic patterns: *deductive guessing* is best, but *naive guessing* is better than no guessing at all. But *naive guessing is not induction: there are no such things as inductive conjectures*!

BETA: But we found the naive conjecture by *induction*! 'That is, it was suggested by observation, indicated by particular instances. . . . And among the particular cases that we have examined we could distinguish two groups: those which preceded the formulation of the conjecture and those which came afterwards. The former *suggested* the conjecture, the latter *supported* it. Both kinds of cases provide some sort of contact between the conjecture and "the facts". . . .'[1] This double contact is the heart of induction: the first makes *inductive heuristic*, the second makes inductive justification, or *inductive logic*.

TEACHER: No! Facts do not suggest conjectures and do not support them either!

BETA: Then what suggested $V-E+F=2$ to *me*, if not the facts, listed in my table?

TEACHER: I shall tell you. You yourself said you failed many times to fit them into a formula.[2] Now what happened was this: you had three or four conjectures which in turn were quickly refuted. Your table was built up in the process of testing and refuting these conjectures. These dead and now forgotten conjectures suggested the facts, not the facts the conjectures. *Naive conjectures are not inductive conjectures: we arrive at them by trial and error, through conjectures and refutations.*[3] But if you—wrongly—believe that you arrived at them inductively, from your tables, if you believe that the longer the table, the more conjectures it will suggest, and later support, you may waste your time compiling unnecessary data. Also, being indoctrinated that the path of discovery is from facts to conjecture, and from

[1] Pólya [1954], Vol. I, pp. 5 and 7

[2] See p. 299

[3] These trials and errors are beautifully reconstructed by Pólya. The first conjecture is that F increases with V. This being refuted, two more conjectures follow: E increases with F; E increases with V. The fourth is the winning guess: $F+V$ increases with E ([1954], Vol. I, pp. 35-37).

conjecture to proof (the myth of induction), you may completely forget about the heuristic alternative: deductive guessing.[1]

Mathematical heuristic is very like scientific heuristic—not because both are inductive, but because both are characterised by conjectures, proofs, and refutations. The—important—difference lies in the nature of the respective conjectures, proofs (or, in science, explanations), and counter-examples.[2]

BETA: I see. Then our naive conjecture was not the *first* conjecture ever, 'suggested' by hard, non-conjectural facts: it was preceded by many 'pre-naive' conjectures and refutations. The logic of conjectures and refutations has no starting point—but the logic of proofs and refutations has: it starts with the first naive conjecture to be followed by a thoughtexperiment.

ALPHA: Perhaps. But then I should not have called it 'naive'![3]

KAPPA [*aside*]: Even in heuristic there is no such thing as perfect *naiveté*!

BETA: The main thing is to get out of the trial-and-error period as soon as possible, to proceed quickly to thoughtexperiments without having too much 'inductive' respect for 'facts'. Such respect may hamper the growth of knowledge. Imagine that you arrive by trial-and-error at the conjecture: $V-E+F=2$, and that it is immediately refuted by the observation that $V-E+F=0$ for the picture-frame. If you have too much respect for facts, especially when they refute your conjectures, you will go on with pre-naive trial-and-error and look for another conjecture. But if you have a better heuristic, you at least *try* to ignore the adverse observational test, and try a *test by thoughtexperiment*: like Cauchy's proof.

[1] On the other hand those who, because of the usual deductive presentation of mathematics, come to believe that the path of discovery is from axioms and/or definitions to proofs and theorems, may completely forget about the possibility and importance of naive guessing. In fact in mathematical heuristic it is deductivism which is the greater danger, while in scientific heuristic it is inductivism.

[2] We owe the revival of mathematical heuristic in this century to Pólya. His stress on the similarities between scientific and mathematical heuristic is one of the main features of his admirable work. What may be considered his only weakness is connected with this strength: he never questioned that science is inductive, and because of his correct vision of deep analogy between scientific and mathematical heuristic he was led to think that mathematics is also inductive. The same thing happened earlier to Poincaré (see his [1902], Introduction) and also to Fréchet (see his [1938]).

[3] See Part II, p. 138

SIGMA: What confusion! Why call Cauchy's *proof* a *test*?

BETA: Why call Cauchy's *test* a *proof*? It was a *test*! Listen. You started with a naive conjecture: $V-E+F=2$ for all polyhedra. Then you drew consequences from it: 'if the naive conjecture is true, after removing a face, for the remaining network $V-E+F=1$'; 'if this consequence is true, $V-E+F=1$ even after triangulation'; 'if this last consequence is true, $V-E+F=1$ will hold while triangles are removed one by one'; 'if this is true, $V-E+F=1$ for one single triangle'. . . .

Now this last conclusion happens to be known to be true. But what if we had concluded that for a single triangle $V-E+F=0$? We would immediately have rejected the original conjecture as false. All that we have done is to test our conjecture: to draw consequences from it. The test seemed to corroborate the conjecture. But corroboration is not proof.

SIGMA: But then our proof proved even less than we thought it did! We then have to reverse the process and try to construct a thoughtexperiment which leads in the opposite direction: from the triangle back to the polyhedron!

BETA: That is right. Only Zeta pointed out that instead of solving our problem by first devising a naive conjecture through trial and error, then testing it, then reversing the test into a proof, we can start straight away with the real proof. Had we realised the possibility of deductive guessing we might have avoided all this pseudo-inductive fumbling!

KAPPA [*aside*]: What a dramatic series of *volte-faces*! Critical Alpha has turned into a dogmatist, dogmatist Delta into a refutationist, and now inductivist Beta into a deductivist!

SIGMA: But wait. If the *test-thoughtexperiment*. . . .

BETA: I shall call it *analysis*. . . .

SIGMA: . . . can be followed up at all by a *proof-thoughtexperiment*. . . .

BETA: I shall call it *synthesis*. . . .[1]

SIGMA: . . . will the 'analytic theorem' be necessarily identical with the 'synthetic theorem'? In going in the opposite direction we might use different lemmas![2]

[1] According to Pappian heuristic, mathematical discovery starts with a conjecture, which is followed by *analysis* and then, provided *analysis* does not falsify the conjecture, by *synthesis*. (Also cf. Part I, p. 10, footnote 2, and Part III, p. 243, footnote 1.) But while our version of *analysis-synthesis improves* the conjecture, the Pappian version only *proves* or *disproves* it.

[2] Cf. Robinson [1936], p. 471

BETA: If they are different, then the synthetic theorem should supersede the analytic one—after all analysis only *tests* while synthesis *proves*.

TEACHER: Your discovery that our '*proof*' was in fact a *test* seems to have shocked the class and diverted their attention from your main argument: that if we have a conjecture that has already been refuted by a counterexample, we should push the refutation aside and try to test the conjecture by a thoughtexperiment: this way, we might hit on a proof, leave the phase of trial-and-error, and switch to the method of proofs and refutations. But it was exactly this which made me say that 'I am willing to set out to "prove" a false conjecture'![1] And Lambda too demanded in his *Rule 1*: 'If you have a conjecture set out to prove it *and* refute it.'

ZETA: That is right. But let me supplement Lambda's rules and Omega's *Rule 4* by

Rule 5. If you have counterexamples of any type, try to find, by deductive guessing, a deeper theorem to which they are not counterexamples any longer.

OMEGA: You now stretch my concept of 'depth'—and you may be right. But what about the actual application of your new rule? Until now it has only given us results that we already knew. It is easy to be wise after the event. Your 'deductive guessing' is just the *synthesis* corresponding to Teacher's original *analysis*. But now you should be honest—you must use your method to find a conjecture which you do not already know about, with the promised increase in content.

ZETA: Right. I start with the theorem generated by *my* thought-experiment: '*All closed normal polyhedra are Eulerian.*'

OMEGA: 'Normal'?

ZETA: I don't want to waste time going through the method of proof and refutations. I just call 'normal' all polyhedra that can be built up from a 'perfect' polygon by fitting to it (*a*) first $F-2$ faces without changing $V-E+F$ (these will be *open* normal polyhedra) and (*b*) then a last closing face which increases $V-E+F$ by 1 (and turns the *open* polyhedron into a *closed* one).

OMEGA: 'Perfect polygon'?

ZETA: By a 'perfect' polygon I mean one that can be built up from one single vertex by fitting to it first $n-1$ edges without changing $V-E$, and then a last closing edge which decreases $V-E$ by 1.

[1] See Part I, p. 25

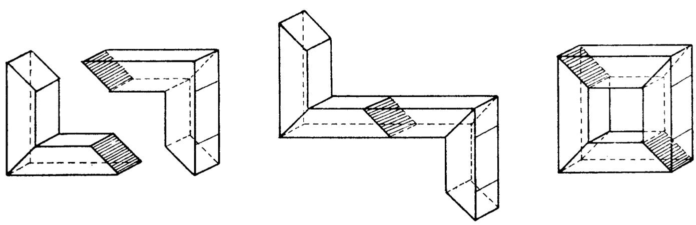
OMEGA: Will your closed normal polyhedra coincide with our Cauchy polyhedra?

ZETA: I do not want to go into that now.

(*d*) *Increasing content by deductive guessing*

TEACHER: Enough of preliminaries. Let us see your deduction.

ZETA: Yes, Sir. I take two closed normal polyhedra (Fig. 20*a*) and paste them together along a polygonal circuit so that the two faces that meet disappear (Fig. 20*b*). Since for the two polyhedra $V-E+F=4$, the



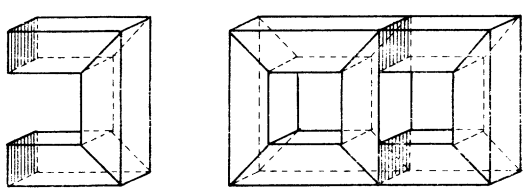
(a) (b) (c)

FIG. 20

disappearance of two faces in the united polyhedron will just restore the Euler formula—no surprise after Cauchy's proof since the new polyhedron can also easily be pumped into a ball. So the formula stands up well to this pasting test. But let us now try a double-pasting test: let us 'paste' the two polyhedra together along two polygonal circuits (Fig. 20*c*). Now 4 faces will disappear and for the new polyhedron $V-E+F=0$.

GAMMA: This is Alpha's *Counterexample 4*, the picture-frame!

ZETA: Now if I 'double-paste' to this picture-frame (Fig. 20*c*) yet another normal polyhedron (Fig. 21*a*), $V-E+F$ will be -2 (Fig. 21*b*). . . .



(a) (b)

FIG. 21

SIGMA: For a monospheroid polyhedron $V-E+F=2$, for a dispheroid polyhedron $V-E+F=0$, for a trispheroid $V-E+F=-2$, for an n-spheroid polyhedron $V-E+F=2-2(n-1)$. . . .

ZETA: . . . which is your new conjecture of unprecedented content, complete with proof, without having compiled a single table.[1]

SIGMA: This is really nice. Not only did you explain the obstinate picture-frame, but you produced an infinite variety of novel counter-examples. . . .

ZETA: Complete with explanation.

RHO: I just arrived at the same result in a different way. Zeta started with two Eulerian examples and turned them into a counter-example in a controlled experiment. I start with a counterexample and turn it into an example. I made the following thoughtexperiment with a picture-frame: 'Let the polyhedron be of some stuff that is easy to cut like soft clay, let a thread be pulled through the tunnel and then through the clay. It will not fall apart. . . .'[2] But it has become a familiar, simple, spheroid polyhedron! It is true, we increase the number of faces by 2, and the numbers of both edges and vertices by m; but since we know that the Euler characteristic of a simple polyhedron is 2, the original must have had the characteristic 0. Now if one needs more, say n, such cuts to reduce the polyhedron to a simple one, its characteristic will be $2-2n$.

SIGMA: This is interesting. Zeta has already shown us that we may not need a conjecture in order to start *proving*, that we may immediately devise a *synthesis*, i.e. a proof-thoughtexperiment from a related proposition that is known to be true. Now Rho shows that we may not need a conjecture even in order to start *testing*, but we may set out—*pretending* that the result is already there—to devise an *analysis*, i.e. a test-thoughtexperiment.[3]

OMEGA: But whichever way you choose, you still leave hordes of polyhedra unexplained! According to your new theorem for all polyhedra $V-E+F$ is an even number, less than 2. But we saw quite a few polyhedra with *odd* Euler characteristics. Take the crested cube (Fig. 12) with $V-E+F=1$. . . .

ZETA: I never said that my theorem applies to *all* polyhedra. It applies only to all n-spheroid polyhedra built up according to my

[1] This was done by Raschig [1891].

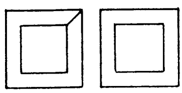
[2] Hoppe [1879], p. 102

[3] This is again part of Pappian heuristic. He calls an *analysis* starting with a conjecture '*theoretical*', and an analysis starting with no conjecture '*problematical*' (Heath [1925], Vol. I, p. 138). The first refers to *problems to prove*, the second to *problems to solve* (or *problems to find*). Also cf. Pólya [1945], pp. 129-136 ('Pappus') and 197-204 ('Working backwards').

construction. My construction as it stands does not lead to ringshaped faces.

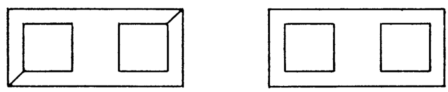
OMEGA: So?

SIGMA: I know! One can also extend it to polyhedra with ringshaped faces: one may construct a ringshaped polygon by deleting in a suitable proof-generated system of polygons an edge without reducing the number of faces (Figs. 22*a* and 22*b*). I wonder, perhaps there are



(a) (b)

FIG. 22



(a) (b)

FIG. 23

also 'normal' systems of polygons, constructed in accordance with our proof, in which we can delete even more than one edge without reducing the number of faces. . . .

GAMMA: That is true. Look at this 'normal' polygonal system (Fig. 23*a*). You can delete two edges without reducing the number of faces (Fig. 23*b*).

SIGMA: Good! Then in general

$$V-E+F = 2-2(n-1)+\sum_{k=1}^{F} e_k$$

for n-spheroid—or n-tuply connected—polyhedra with e_k edges deleted without reduction in the number of faces.

BETA: This formula explains my crested cube (Fig. 12), a monospheroid polyhedron ($n = 1$) with one ringshaped face: e_k are zero, except for e_6 which is 1, or $\sum_{k=1}^{F} e_k = 1$, consequently $V-E+F = 1$.

SIGMA: It also explains your 'irrational' Eulerian freak: the cube with two ringshaped faces and one tunnel (Fig. 16). It is a dispheroid polyhedron ($n = 2$) with $\sum_{k=1}^{F} e_k = 2$. Consequently its characteristic is $V-E+F = 2-2+2 = 2$. Moral order is restored to the world of polyhedra![1]

OMEGA: What about polyhedra with cavities?

[1] The 'order' was restored by Lhuilier with approximately the same formula ([1812–13], p. 189); and by Hessel with clumsy *ad hoc* formulas about different ways of fitting Eulerian polyhedra together ([1832], pp. 19–20). Cf. p. 297, footnote 1.

SIGMA: I know! For them one has to add up the Euler characteristics of each disconnected surface:

$$V - E + F = \sum_{j=1}^{K} \left\{ 2 - 2(n-1) + \sum_{k=1}^{F} e_k \right\}.$$[1]

BETA: And the twin-tetrahedra?

SIGMA: I know! . . .

GAMMA: What is the use of all this precision? Stop this flood of pretentious trivialities![2]

ALPHA: Why should he? Or are the twin-tetrahedra monsters, not genuine polyhedra? A twin-tetrahedron is just as good a polyhedron as your cylinder! But you liked *linguistic* precision.[3] Why do you deride our new precision? We have to make the theorem cover *all* polyhedra—by making it precise we are increasing its content, not decreasing it. This time precision is a virtue!

KAPPA: Boring virtues are just as bad as boring vices! Besides,

[1] Historically Lhuilier—in his [1812-13]—managed to generalise Euler's formula by naive guessing and arrived at the following formula: $V - E + F = 2[(C - T + 1) + (p_1 + p_2 + \ . \ . \ .)]$, where C is the number of cavities, T the number of tunnels and p_1 the number of inner polygons on the *i*th face. He also *proved* it as far as 'inner polygons' were concerned, but tunnels seem to have defeated him. He constructed the formula in an attempt to account for his three kinds of 'exceptions'; but his list of exceptions was incomplete. (Cf. Part II, p. 123, footnote 1.) Moreover, this incompleteness was not the only reason for the falsity of his naive conjecture: for he did not notice the possibility that cavities might be multiply-connected; that one may not be able to determine unambiguously the number of tunnels in polyhedra with a system of branching tunnels; and that it is not 'the number of inner polygons', but the number of ringshaped faces that is relevant (his formula breaks down for two adjacent inner polygons, with an edge in common). For a criticism of Lhuilier's 'inductive generalisation' see Listing [1861], pp. 98–99. Also cf. p. 322, footnote 2.

[2] Quite a few mathematicians of the nineteenth century were confused by such trivial increases in content, and did not really know how to deal with them. Some—like Möbius—used monster-barring definitions (see Part I, p. 17); others—like Hoppe—monster-adjustment. Hoppe's [1879] is particularly revealing. On the one hand he was keen—like many of his contemporaries—to have a *perfectly complete* 'generalised Euler formula' that covers everything. On the other hand he shrank from trivial complexities. So while he claimed that his formula was 'complete, all-embracing', he added confusedly that 'special cases can make the enumeration (of constituents) dubitable' (p. 103). That is, if an awkward polyhedron still defeats his formula, then its constituents were wrongly counted, and the monster should be adjusted by correct vision: *e.g.* the common vertices and edges of twintetrahedra should be seen and counted twice and each twin recognised as a separate polyhedron (*ibid.*). For further examples cf. p. 328, footnote 2.

[3] See Part III, pp. 229-234

you will never achieve *complete* precision. We should stop when it ceases to be interesting to go on.

ALPHA: I have a different point. We started from

(1) one vertex is one vertex.

We deduced from this

(2) $V=E$ for all perfect polygons.

We deduced from this

(3) $V-E+F=1$ for all normal open polygonal systems.

From this

(4) $V-E+F=2$ for all normal closed polygonal systems, i.e. polyhedra.

From this again in turn

(5) $V-E+F=2-2(n-1)$ for normal n-spheroid polyhedra.

(6) $V-E+F=2-2(n-1)+\sum_{k=1}^{F} e_k$ for normal n-spheroid polyhedra with multiply-connected faces.

(7) $V-E+F=\sum_{j=1}^{K}\left\{2-2(n-1)+\sum_{k=1}^{F} e_k\right\}$ for normal n-spheroid polyhedra with multiply-connected faces and with cavities.

Isn't this a miraculous unfolding of the hidden riches of the trivial starting-point? And since (1) is indubitably true, so is the rest.

RHO [*aside*]: Hidden 'riches'? The last two only show how *cheap* generalisations may become![1]

LAMBDA: Do you really think that (1) is the single axiom from which all the rest follows? That deduction increases content?

ALPHA: Of course! Isn't this the miracle of the deductive thought-experiment? If once you have got hold of a little truth, deduction expands it infallibly into a tree of knowledge.[2] If a deduction does

[1] Cf. pp. 328-9

[2] Ancient philosophers did not hesitate to deduce a conjecture from a very trivial consequence of it (see, for example, our synthetic proof leading from the triangle to the polyhedron). Plato thought that 'a single axiom might suffice to generate a whole system'. 'Ordinarily he thought of a single hypothesis as fertile by itself, ignoring in his methodology the other premisses to which he is allying it' (Robinson [1953], p. 168). *This is characteristic of ancient informal logic, that is, of the logic of proof or of thoughtexperiment or of construction; we regard it as enthymematic only through hindsight: it was only later that an increase in content became a sign, not of the power, but of the weakness, of an inference.* This ancient informal logic was strongly advocated by Descartes, Kant and Poincaré; they all despised Aristotelian formal logic and dismissed it as sterile and irrelevant—at the same time extolling the infallibility of fertile informal logic.

not increase the content I would not call it deduction, but 'verification': 'verification differs from true demonstration precisely because it is purely analytic and because it is sterile.'[1]

LAMBDA: But surely deduction cannot increase content! If criticism reveals that the conclusion is richer than the premiss, we have to reinforce the premiss by making hidden lemmas explicit.

KAPPA: And it is these hidden lemmas that contain sophistication and fallibility and ultimately destroy the myth of infallible deduction.[2]

TEACHER: Any other question about Zeta's method?

(e) *Logical versus heuristic counterexamples.*

ALPHA: I like Zeta's *Rule 5*[3]—as I did Omega's *Rule 4*[4]. I liked Omega's method because it looked out for local but not global counterexamples: the ones which Lambda's original three rules[5] ignored as logically harmless, therefore heuristically uninteresting. Omega was stimulated by them to devise new thoughtexperiments: real advances in our knowledge.

Now Zeta is inspired by counterexamples that are both global and local—perfect corroborations from the logical but not from the heuristic point of view: although corroborations, they still call for action. Zeta proposes to extend, sophisticate our original thought-experiment, to turn logical corroborations into heuristic ones, logically satisfactory instances into instances that are satisfactory from both the logical and the heuristic point of view.

Both Omega and Zeta are for new ideas, while Lambda and especially Gamma are preoccupied with linguistic tricks to deal with their irrelevant global but not local counterexamples—the only relevant ones from their crankish point of view.

THETA: So the logical point of view is 'crankish', is it?

ALPHA: *Your* logical point of view, yes. But I want to make another remark. Whether deduction increases content or not—mind you, of course it does—it certainly seems to guarantee the *continuous*

[1] Poincaré [1902], p. 33

[2] The hunt for hidden lemmas, which started only in mid-nineteenth century mathematical criticism, was closely related to the process that later replaced *proofs* by *proof-analyses* and *laws of thought* by *laws of language*. The most important developments in logical theory were usually preceded by the development of mathematical criticism. Unfortunately even the best historians of logic tend to pay exclusive attention to the *changes in logical theory* without noticing their roots in *changes in logical practice*. Cf. also p. 335, footnote 1.

[3] See p. 306 [4] See Part III, p. 237 [5] See Part III, p. 229

growth of knowledge. We start with a vertex and let knowledge grow forcefully and harmoniously to explain the relation between the number of vertices, edges and faces of any polyhedron whatsoever: an undramatic growth without refutations!

THETA [*to Kappa*]: Has Alpha lost all his judgment? One starts with a *problem*, not with a vertex![1]

ALPHA: This piecemeal but irresistibly victorious campaign will lead us to theorems that are 'not by themselves evident, but only deduced from true and known principles by the continuous and uninterrupted action of a mind that has a clear vision of each step in the process'.[2] They could never have been reached by 'unbiased' observation and a sudden flash of insight.

THETA: I am doubtful about this final victory. Such growth will never bring us to the cylinder—for (1) starts with a vertex and the cylinder has none. Also we may never reach onesided polyhedra, or many-dimensional polyhedra. This piecemeal continuous expansion may well stop at some point and you will have to look for a new, revolutionary start. And even this 'peaceful continuity' is full of refutations, criticism! Why do we go on from (4) to (5), from (5) to (6), from (6) to (7) if not under the continuous pressure of counterexamples which are both global and local? Lambda accepted as genuine counterexamples only those which are global but not local: they revealed the *falsehood* of the theorem. Omega's innovation—rightly praised by Alpha—was to regard also counterexamples which are local but not global as genuine counterexamples: they revealed the *poverty of the truth* of the theorem. Now Zeta tells us to recognise even those counterexamples as genuine which are both global and local: they too point to the *poverty of the truth* of the theorem. For example, picture-frames are both global and local counterexamples to Cauchy's theorem: they are of course corroborations as far as *truth* alone is concerned—but they are refutations as far as *content* is concerned. We may call the first (global but not local) counterexamples *logical*, the others *heuristic counterexamples*. But the more we recognise refutations—logical or heuristic—the quicker knowledge grows. Alpha regards logical counterexamples as irrelevant and refuses to call heuristic counterexamples counterexamples at all, because of his obsession with the idea that growth of mathematical knowledge is continuous, and criticism plays no role.

[1] Alpha certainly seems to have slipped into the fallacy of deductive heuristic. Cf. p. 304, footnote 1.

[2] Descartes [1628], Rule III

ALPHA: You expand the concept of refutation and the concept of criticism artificially only to justify your critical theory of the growth of knowledge. Linguistic tricks as tools for a critical philosopher?

PI: I think a discussion of concept-formation may help us to elucidate the issue.

GAMMA: We are all ears.

8 Concept-formation

(a) Refutation by concept-stretching. A reappraisal of monsterbarring—and of the concepts of error and refutation

PI: I would first like to go back to the pre-Zeta, or even pre-Omega period, to the three main methods of theorem-formation: monsterbarring, exceptionbarring, and the method of proofs and refutations. Each started with the same naive conjecture, but ended up with different *theorems* and different *theoretical terms*. Alpha has already outlined some aspects of these differences,[1] but his account is unsatisfactory—especially in the case of monsterbarring and of the method of proofs and refutations. Alpha thought that the monsterbarring theorem 'hides behind the identity of the linguistic expression an essential improvement' on the naive conjecture: he thought that Delta gradually *contracted* the class of 'naive' polyhedra into a class purged of non-Eulerian monsters.

GAMMA: What is wrong with this account?

PI: That it was not the monsterbarrers who *contracted* concepts—it was the refutationists who *expanded* them.

DELTA: Hear, hear!

PI: Let us go back to the time of the first explorers of our subject. They were fascinated by the beautiful symmetry of *regular* polyhedra: they thought that the five regular bodies held the secret of the Cosmos.[2] By the time the Descartes-Euler conjecture was put forward, the concept of polyhedron included all sorts of convex polyhedra and even some concave polyhedra. But it certainly did not include polyhedra which were not simple, or polyhedra with ringshaped faces. For the polyhedra that they had in mind, the conjecture *was* true as it stood and the proof was flawless.[3]

[1] See Part II, p. 138 [2] Cf. Lhuilier [1812–13*a*], p. 233

[3] Fig. 6 in Euler's [1750] is the first concave polyhedron ever to appear in a geometrical text. Legendre talks about convex and concave polyhedra in his [1794]. But before Lhuilier nobody mentioned concave polyhedra that were not simple.

However, one interesting qualification might be added. The first class of polyhedra ever investigated consisted partly of the five ordinary regular polyhedra and

Then came the refutationists. In their critical zeal they stretched the concept of polyhedron, to cover objects that were alien to the intended interpretation. The conjecture was true in its *intended interpretation*, it was only false in an *unintended interpretation* smuggled in by the refutationists. Their 'refutation' revealed no *error* in the original conjecture, no *mistake* in the original proof: it revealed the falsehood of a *new* conjecture which nobody had stated or thought of before.

Poor Delta! He valiantly defended the original interpretation of polyhedron. He countered each counterexample with a new clause to safeguard the original concept. . . .

GAMMA: But wasn't it Delta who shifted his position each time? Whenever we produced a new counterexample, he changed his definition for a longer one which displayed another of his 'hidden' clauses!

PI: What a monstrous appraisal of monsterbarring! He only *seemed* to shift his position. You wrongly accused him of using surreptitious terminological epicycles in the defence of a stubborn idea. His misfortune was that portentous *Definition 1*: 'A polyhedron is a solid whose surface consists of polygonal faces', which the refutationists seized upon immediately. But Legendre meant it to cover *only* his naive polyhedra; that it covered far more was entirely unrealised and unintended by its proposer. The mathematical public was willing to stomach the monstrous content which slowly emerged from this plausible, innocent-looking definition. This is why Delta had to stutter time and time again, 'I meant . . .', and had to keep making his endless 'tacit' clauses explicit: all because the naive concept had never been pinned down, and a simple, but monstrous, unintended definition had superseded it. But imagine a different

quasi-regular polyhedra like prisms and pyramids (cf. Euclid). This class was extended after the Renaissance in two directions. One is indicated in the text: to include all convex and some mildly indented simple polyhedra. The other was Kepler's: he widened the class of regular polyhedra by his invention of regular star-polyhedra. But Kepler's innovation was forgotten, only to be made again by Poinsot (cf. Part I, pp. 18-19). Euler surely did not dream of star-polyhedra. Cauchy knew of them, but his mind was strangely compartmentalised: when he had an interesting idea about star-polyhedra he published it; but he ignored star-polyhedra when presenting counterexamples to his general theorems about polyhedra. Not so the young Poinsot ([1809])—but later he changed his mind (cf. Part II, p. 128).

Thus Pi's statement, although heuristically correct (i.e. true in a rational history of mathematics), is historically false. (This should not worry us: actual history is frequently a caricature of its rational reconstructions.)

situation, where the definition fixed the intended interpretation of 'polyhedron' properly. Then it would have been up to the refutationists to devise ever longer *monster-including definitions* for say, 'complex polyhedra': 'A complex polyhedron is an aggregate of (real) polyhedra such that each two of them are soldered by congruent faces'. 'The faces of complex polyhedra can be complex polygons that are aggregates of (real) polygons such that each two of them are soldered by congruent edges'. This *complex polyhedron* would then correspond to Alpha's and Gamma's refutation-generated concept of *polyhedron*—the first definition allowing also for polyhedra that are not simple, the second also for faces that are not simply-connected. So devising new definitions is not necessarily the task of monsterbarrers or concept-preservers—it can also be that of monster-includers or concept-stretchers.[1]

SIGMA: Concepts and definitions—that is, intended concepts and unintended definitions—can then play funny tricks on each other! I never dreamt that concept-formation might lag behind an unintendedly wide definition!

PI: It might. Monsterbarrers only keep to the original concept, while concept-stretchers widen it; the curious thing is that concept-stretching goes on surreptitiously: nobody is aware of it, and since everybody's 'coordinate-system' expands with the widening concept, they fall prey to the heuristic delusion that monsterbarring *narrows* concepts, while in fact it keeps them invariant.

DELTA: Now who was intellectually dishonest? Who made surreptitious changes in his position?

GAMMA: I admit we were wrong in indicting Delta for surreptitious contractions of his concept of polyhedron: all his six definitions denoted the same good old concept of polyhedron he inherited from his forefathers. *He defined the very same poor concept in increasingly rich theoretical frames of reference, or languages: monsterbarring does not form concepts but only translates definitions.* The monsterbarring theorem is no improvement on the naive conjecture.

DELTA: Do you mean that all my definitions were logically equivalent?

GAMMA: That depends on your logical theory—according to mine they certainly are not.

[1] An interesting example of monster-including definition is Poinsot's re-definition of *convexity*, which brings star-polyhedra into the respectable class of convex regular bodies [1809].

DELTA: This was not a very helpful answer, you will admit. But tell me, did you refute the naive conjecture? You refuted it only by surreptitiously perverting its original interpretation!

GAMMA: Well, we refuted it in a more imaginative and interesting interpretation than you ever dreamt of. This is what makes the difference between *refutations which only reveal a silly mistake* and *refutations which are major events in the growth of knowledge.* If you had found that 'for all polyhedra $V-E+F=1$' because of inept counting, and I had corrected you, I wouldn't call that a 'refutation'.

BETA: Gamma is right. After Pi's revelation we might hesitate to call our 'counterexamples' *logical counterexamples*, since they are after all not inconsistent with the conjecture in its intended interpretation; but they are certainly *heuristic counterexamples* since they spur the growth of knowledge. If we were to accept Delta's narrow logic, knowledge would not grow. Just suppose that somebody with the narrow conceptual framework discovers the Cauchy proof of the Euler conjecture. He finds that all the steps of this thoughtexperiment can easily be performed on *any* polyhedron. He takes the 'fact' that all polyhedra are simple and that all faces are simply-connected as obvious, as indubitable. It never occurs to him to turn his 'obvious' lemmas into conditions in an improved conjecture and so to build up a theorem—because the stimulus of counterexamples, in showing up some 'trivially true' lemmas as false, is missing. Thus he thinks that the 'proof' indubitably establishes the truth of the naive conjecture, that its certainty is beyond doubt. But his 'certainty' is far from being a sign of success, it is only a symptom of lack of imagination, of conceptual poverty. It produces smug satisfaction and prevents the growth of knowledge.[1]

[1] This is in fact Cauchy's case. It is unlikely that if Cauchy had already discovered his revolutionary exception-barring method (cf. Part III, pp. 234-235), he would not have searched for and found some exceptions. But he probably came across the problem of exceptions only later, when he decided to clear up the chaos in analysis. (It was Lhuilier who seems to have first noticed, and faced, the fact that such 'chaos' was not confined to analysis.)

Historians, e.g. Steinitz in his [1914-31], usually say that Cauchy, noticing that his theorem was not universally valid, stated it for *convex* polyhedra only. It is true that in his proof he uses the expression 'the convex surface of a polyhedron' ([1811], p. 81), and in his [1812] he restates Euler's theorem under the general head: 'Theorems on solid angles and *convex polyhedra*'. But probably to counteract this title, he gives particular stress to the *universal* validity of Euler's theorem for *any* polyhedron (Theorem XI, p. 94), while stating three other theorems (Theorem XIII and its two

(*b*) *Proof-generated versus naive concepts. Theoretical versus naive classification*

PI: Let me return to the proof-generated theorem: 'All simple *polyhedra* with simply-connected faces are Eulerian'. This formulation is misleading. It should read: 'All simple *objects* with simply-connected faces are Eulerian.'

GAMMA: Why?

corollaries) explicitly for *convex* polyhedra (pp. 96 and 98).

Why Cauchy's sloppy terminology? Cauchy's concept of polyhedron *almost* coincided with the concept of convex polyhedron. But it did not coincide exactly: Cauchy knew about concave polyhedra, which can be obtained by slightly pushing in the side of convex polyhedra, but he did not discuss what seemed to be irrelevant further *corroborations*—not *refutations*—of his theorem. (*Corroborations never compare with counterexamples, or even 'exceptions', as catalysts for the growth of concepts.*) This is the reason for Cauchy's casual use of 'convex': it was a failure to realise that concave polyhedra might give counterexamples, not a conscious effort to *eliminate* these counterexamples. In the very same paragraph, he argues that Euler's theorem is an 'immediate consequence' of the lemma that $V-E+F=1$ for *flat* polygonal networks, and states that 'for the validity of the theorem $V-E+F=1$ it has no significance whatever whether the polygons lie in the same plane or in different planes, since the theorem is concerned only with the number of polygons and the number of their constituents' (p. 81). This argument is perfectly correct within Cauchy's narrow conceptual framework, but incorrect in a wider one, in which 'polyhedron' refers also to, say, picture-frames. The argument was frequently repeated in the first half of the nineteenth century (e.g. Olivier [1826], p. 230, or Grunert [1827], p. 367, or R. Baltzer [1860-62], Vol. II, p. 207). It was criticised by J. C. Becker ([1869], p. 68).

Often, as soon as concept-stretching refutes a proposition, the refuted proposition seems such an elementary mistake that one cannot imagine that great mathematicians could have made it. This important characteristic of concept-stretching refutation explains why respectful historians, because they do not understand that concepts *grow*, create for themselves a maze of problems. After saving Cauchy by claiming that he 'could not possibly miss' polyhedra which are not simple and that therefore he '*categorically*' (!) *restricted* the theorem to the domain of convex polyhedra, the respectful historian now has to explain why Cauchy's borderline was 'unnecessarily' narrow. Why did he *ignore* non-convex Eulerian polyhedra? Steinitz's explanation is this: the *correct* formulation of the Euler-formula is in terms of connectivity of surfaces. Since in Cauchy's period this concept was not yet 'clearly grasped', 'the simplest way out' was to assume convexity (p. 20). So Steinitz explains away a mistake that Cauchy never made.

Other historians proceed in a different way. They say that before the point where the correct conceptual framework (i.e. the one they know) was reached there was only a 'dark age' with 'seldom, if ever, sound' results. This point in the theory of polyhedra is Jordan's proof (1866) according to Lebesgue ([1923], pp. 59-60); it is Poincaré's (1895) according to Bell ([1945], p. 460).

PI: The first formulation suggests that the class of simple polyhedra that occurs in the theorem is a subclass of the class of ' polyhedra ' of the naive conjecture.

SIGMA: Of course the class of simple polyhedra is a subclass of polyhedra! The concept of ' simple polyhedron ' *contracts* the original wide class of polyhedra by restricting them to those on which the first lemma of our proof is performable. The concept of ' simple polyhedron with simply-connected faces ' indicates a further contraction of the original class. . . .

PI: No! The original class of polyhedra contained only polyhedra that were simple and whose faces were simply-connected. Omega was wrong when he said that lemma-incorporation reduces content.[1]

OMEGA: But doesn't each incorporation of lemmas rule out a counterexample?

PI: Of course it does: but a counterexample that was produced by concept-stretching.

OMEGA: So lemma-incorporation *conserves* content, just like monster-barring?

PI: No. Lemma-incorporation *increases* content: monster-barring does not.

OMEGA: What? Do you really want to convince me not only that lemma-incorporation does not *reduce* content, but also that it *increases* it? That instead of *contracting* concepts it *stretches* them?

PI: Exactly. Just listen. Was a globe, with a political map drawn on it, an element of the original class of polyhedra?

OMEGA: Certainly not.

PI: But it became one after Cauchy's proof. For you can perform Cauchy's proof on it without the slightest difficulty—if only there are no ringshaped countries or seas on it.[2]

GAMMA: That is right! Pumping the polyhedron up into a ball and distorting edges and faces will not perturb us in the least in performing the proof—so long as the distortion does not alter the *number* of vertices, edges and faces.

SIGMA: I see your point. Then the proof-generated ' simple polyhedron ' is not just a contraction, a specification, but also a *generalisation*, an *expansion* of the naive ' polyhedron '.[3] The idea of

[1] See Part III, p. 236 [2] Cf. Part II, p. 132, footnote.

[3] Darboux, in his [1874], came close to this idea. Later it was clearly formulated by Poincaré: ' Mathematics is the art of giving the same name to different

generalising the concept of polyhedron so that it should include crumpled, *curvilinear* ' polyhedra ' with *curved* faces could hardly have occurred to anybody before Cauchy's proof; even if it had, it would have been dismissed as crankish. But now it is a natural generalisation, since the operations of our proof can be interpreted for them just as well as for ordinary naive polyhedra with straight edges and flat faces.[1]

Pi: Good. But you have to make one more step. *Proof-generated concepts* are neither ' specifications ', nor ' generalisations ' of naive concepts. The impact of proofs and refutations on naive concepts is much more revolutionary than that: they *erase* the crucial naive concepts completely and *replace* them by proof-generated concepts.[2]

things . . . . If one chooses the right language, one is surprised to learn that the proofs made for a known object apply immediately to many new objects, without the slightest change—one can even retain the names ' ([1908], p. 375). Fréchet calls this ' an extremely useful principle of generalisation ', and formulates it as follows: ' When the set of properties of a mathematical entity used in the proof of a proposition about this entity does not determine this entity, the proposition can be extended to apply to a more general entity ' ([1928], p. 18). He points out that such generalisations are not trivial and ' may require very great efforts ' (*ibid.*).

[1] Cauchy did not notice this. His proof differed from the one given by the Teacher in one important respect: Cauchy in his [1811-12] did *not* imagine the polyhedron to be made of rubber. The novelty of his proof-idea was to imagine the polyhedron as a *surface*, and not as a *solid*, as Euclid, Euler and Legendre did. But he imagined it as a *solid* surface. When he removed one face and mapped the remaining spatial polygonal network into a flat polygonal network, he did not conceive his mapping as a *stretching* that might *bend* faces or edges. The first mathematician to notice that Cauchy's proof could be performed on polyhedra with *bent faces* was Crelle ([1826-27], pp. 671-2), but he still carefully stuck to *straight edges*. For Cayley however it seemed recognisable ' *at first sight* ' that ' the theory would not be materially altered by allowing the edges to be curved lines ' ([1861], p. 425). The same remark was made independently in Germany by Listing ([1861], p. 99) and in France by Jordan ([1866], p. 39).

[2] *This theory of concept-formation weds concept-formation to proofs and refutations.* Pólya weds it to *observations*: ' When the physicists started to talk about " electricity," or the physicians about " contagion," these terms were vague, obscure, muddled. The terms that the scientists use today, such as " electric charge," " electric current," " fungus infection," " virus infection," are incomparably clearer and more definite. Yet what a tremendous amount of observation, how many ingenious experiments lie between the two terminologies, and some great discoveries too. Induction changed the terminology, clarified the concepts. We can illustrate also this aspect of the process, the inductive clarification of concepts, by suitable mathematical examples ' ([1954], Vol. I, p. 55). But even this mistaken inductivist theory of concept-formation is preferable to the attempt to make concept-formation *autonomous*, to make ' *clarification* ' or ' *explication* ' of concepts a *preliminary* to any scientific discussion.

The naive term 'polyhedron', even after being stretched by refutationists, denoted something that was crystal-like, a solid with 'plane' faces, straight edges. The proof-ideas swallowed this naive concept and fully digested it. In the different proof-generated theorems we have nothing of the naive concept. That disappeared without trace. Instead each proof yields its characteristic proof-generated concepts, which refer to stretchability, pumpability, photographability, projectability and the like. The old problem disappeared, new ones emerged. After Columbus one should not be surprised if *one does not solve the problem one has set out to solve.*

SIGMA: So the 'theory of solids', the original 'naive' realm of the Euler conjecture, dissolves, and the remodelled conjecture reappears in projective geometry if proved by Gergonne, in analytical topology if proved by Cauchy, in algebraic topology if proved by Poincaré. . . .

PI: Quite right. And now you will understand why I formulated the theorems not, like Alpha or Beta, as: 'All Gergonne-polyhedra are Eulerian', 'All Cauchy-polyhedra are Eulerian', and so on, but rather as: 'All Gergonnian objects are Eulerian', 'All Cauchy objects are Eulerian', and so on.[1] *So I find it uninteresting to quarrel not only about the exactness of naive concepts but also about the truth or falsehood of naive conjectures.*

BETA: But surely we can retain the term 'polyhedron' for our favourite proof-generated term, say, 'Cauchy-objects'?

PI: If you like, but remember that *your term no longer denotes what it set out to denote*: that its naive meaning has disappeared and that now it is used. . . .

BETA: . . . for a more general, improved concept!

THETA: No! For a totally different, novel concept.

SIGMA: I think your views are paradoxical!

PI: If you mean by paradoxical 'an opinion not yet generally received',[2] and possibly inconsistent with some of your ingrained naive ideas, never mind: you only have to replace your naive ideas with the paradoxical ones. This may be a way to 'solve' paradoxes. But what particular view of mine do you have in mind?

SIGMA: You remember, we found that some star-polyhedra are Eulerian while some others are not. We were looking for a proof that would be deep enough to explain the Eulerianness both of ordinary and star-polyhedra. . . .

[1] See Part III, p. 245

[2] Hobbes [1656], Animadversions upon the Bishop's Reply No. xxi.

EPSILON: I have it.[1]

SIGMA: I know. But just for the sake of argument let us imagine that there is no such proof, but that somebody offers, in addition to Cauchy's proof for Eulerian 'ordinary' polyhedra, a corresponding but altogether different proof for Eulerian star-polyhedra. Would you then, Pi, because of these two different proofs, propose to split into two what we formerly classified as one? And would you have two completely different things united under one name just because somebody finds a common explanation for some of their properties?

PI: Of course I would. I certainly wouldn't call a whale a fish, a radio a noisy box (as aborigines may do), and I am not upset when a physicist refers to glass as a liquid. Progress indeed replaces *naive classification* by *theoretical classification*, that is, by theory-generated (proof-generated, or if you like, explanation-generated) classification. Conjectures and concepts both have to pass through the purgatory of proofs and refutations. *Naive conjectures and naive concepts are superseded by improved conjectures (theorems) and concepts (proof-generated or theoretical concepts) growing out of the method of proofs and refutations.* And as theoretical ideas and concepts supersede naive ideas and concepts, theoretical language supersedes naive language.[2]

[1] See Part III, p. 244, footnote 1.

[2] It is interesting to follow the gradual changes from the rather naive classification of polyhedra to the highly theoretical one. The first naive classification which covers not only simple polyhedra comes from Lhuilier: a classification according to the number of *cavities, tunnels* and '*inner polygons*' (see p. 310, footnote 1).

(*a*) *Cavities.* Euler's first proof and, incidentally, Lhuilier's own ([1812-13], pp. 174-177), rested on the decomposition of the *solid*, either by cutting off its corners one by one, or by decomposing it into pyramids from one or more points in the inside. Cauchy's proof-idea however—Lhuilier did not know about it—rested on the decomposition of the polyhedral *surface*. When the theory of polyhedral surfaces finally superseded the theory of polyhedral solids, cavities became uninteresting: *one* 'polyhedron with cavities' turns into a whole *class* of polyhedra. Thus our old monster-barring *Definition 2* (Part I, p. 16) became a proof-generated, theoretical definition, and the taxonomical concept of 'cavity' disappeared from the mainstream of growth.

(*b*) *Tunnels.* Already Listing pointed to the unsatisfactoriness of this concept (see p. 310, footnote 1). The replacement came not from any 'explication' of the 'vague' concept of tunnel, as a Carnapian might be tempted to expect, but from trying to prove and refute Lhuilier's naive conjecture about the Euler-characteristic of polyhedra with tunnels. In the course of this process the concept of polyhedron with n tunnels disappeared and proof-generated 'multiply-connectedness' (what we called 'n-spheroidness') took its place. In some papers we find the naive term retained for the new proof-generated concept: Hoppe defines the number of 'tunnels'

OMEGA: In the end we shall arrive from naive, accidental, merely nominal classification to the final true, real, classification, to perfect language! [1]

(c) *Logical and heuristic refutations revisited*

PI: Let me take up again some of the issues which have arisen in connection with deductive guessing. First let us take the problem of heuristic versus logical counterexamples as raised in the discussion between Alpha and Theta.

My exposition has shown, I think, that even the so-called 'logical' counterexamples were heuristic. In the originally intended interpretation there is no inconsistency between

(*a*) All polyhedra are Eulerian

and

(*b*) The pictureframe is not Eulerian.

If we keep to the tacit semantical rules of our original language our counterexamples are not counterexamples. They are turned into logical counterexamples only by changing the rules of the language by concept-stretching.

by the number of cuts that leave the polyhedron connected ([1879], p. 102). For Ernst Steinitz the concept of tunnel is already so theory-impregnated that he is unable to find any 'essential' difference between Lhuilier's naive classification according to the number of tunnels and the proof-generated classification according to multiply-connectedness; therefore he regards Listing's criticism of Lhuilier's classification as 'largely unjustified' ([1914-31], p. 22).

(*c*) '*Inner polygons*'. This naive concept too was soon replaced, first by ring-shaped, then by multiply-connected, faces (also cf. p. 310, footnote 1), (*replaced*, not 'explicated', for 'ring-shaped face' is surely not an explication of 'inner polygon'). When, however, the theory of polyhedral surfaces was superseded on the one hand by the topological theory of surfaces, and on the other hand by graph-theory, the problem of how multiply-connected faces influence the Euler-characteristic of a polyhedron lost all its interest.

Thus, out of the three key concepts of the first naive classification, only one was left', and even that in a hardly recognisable form—the generalised Euler formula was, for the moment, reduced to $V-E+F=2-2n$. (For further developments cf. p. 329, footnote 1.)

[1] As far as naive classification is concerned, nominalists are close to the truth when claiming that the only thing that polyhedra (or, to use Wittgenstein's favourite example, games) have in common is their name. But after a few centuries of proofs and refutations, as the theory of polyhedra (or, say, the theory of games) develops, and theoretical classification replaces naive classification, the balance changes in favour of the realist. The problem of universals ought to be reconsidered in view of the fact that, as knowledge grows, languages change.

GAMMA: Do you mean that *all* interesting refutations are heuristic?

PI: Exactly. You cannot separate refutations and proofs on the one hand and changes in the conceptual, taxonomical, linguistic framework on the other. Usually, when a 'counterexample' is presented, you have a choice: either you refuse to bother with it, since it is not a counterexample at all in your *given* language L_1, or you agree to change your language by concept-stretching and accept the counterexample in your new language L_2. . . .

ZETA: . . . and *explain* it in L_3!

PI: According to traditional static rationality you would have to make the first choice. Science teaches you to make the second.

GAMMA: That is, we may have two statements that are consistent in L_1, but we switch to L_2 in which they are inconsistent. Or, we may have two statements that are inconsistent in L_1, but we switch to L_2 in which they are consistent. As knowledge grows, languages change. 'Every period of creation is at the same time a period in which the language changes.'[1] The growth of knowledge cannot be modelled in any given language.

PI: That is right. Heuristic is concerned with language-dynamics, while logic is concerned with language-statics.

(*d*) *Theoretical versus naive concept-stretching. Continuous versus critical growth*

GAMMA: You promised to come back to the question whether or not deductive guessing offers us a continuous pattern of the growth of knowledge.

PI: Let me first sketch some of the many *historical* forms which this *heuristic* pattern can take.

The *first main pattern* is when naive concept-stretching outstrips theory by far and produces a vast chaos of counterexamples: our naive

[1] Félix [1957], p. 10. According to logical positivists, the *exclusive* task of philosophy is to construct 'formalised' languages in which artificially congealed states of science are expressed (see our quotation from Carnap in Part I, p. 2). But such investigations scarcely get under way before the rapid growth of science discards the old 'language system'. Science teaches us not to respect any given conceptual-linguistic framework lest it should turn into a conceptual prison—language analysts have a vested interest in at least slowing down this process, in order to justify their linguistic therapeutics, that is, to show that they have an all-important feedback to, and value for, science, that they are not degenerating into 'fairly dried-up pettyfoggery' (Einstein [1953]). Similar criticisms of logical positivism have been made by Popper: see e.g. his [1934], p. 128, footnote *3.

concepts are loosened but no theoretical concepts replace them. In this case deductive guessing may catch up—piecemeal—with the backlog of counterexamples. This is, if you like, a continuous 'generalising' pattern—but do not forget that it starts with refutations, that its continuity is the piecemeal explanation by a growing theory of the heuristic refutations of its first version.

GAMMA: Or, 'continuous' growth only indicates that refutations are miles ahead!

PI: That is right. But it may happen that each single refutation or expansion of naive concepts is *immediately* followed by an expansion of the theory (and theoretical concepts) which explains the counter-example; 'continuity' then gives place to an exciting alternation of concept-stretching refutations and ever more powerful theories, of *naive concept-stretching* and explanatory *theoretical concept-stretching*.

SIGMA: Two accidental historical variations on the same heuristic theme!

PI: Well, there is not really much difference between them. In both of them *the power of the theory lies in its capacity to explain its refutations in the course of its growth*. But there is a *second main pattern* of deductive guessing. . . .

SIGMA: Yet another accidental variation?

PI: Yes, if you like. In this variation however the growing theory not only *explains* but *produces* its refutations.

SIGMA: What?

PI: In this case theoretical growth overtakes—and, indeed, eliminates—naive concept-stretching. For example, one starts with, say, Cauchy's theorem, without a single counterexample on the horizon. Then one tests the theorem by transforming the polyhedron in all possible ways: cutting it into two, cutting off pyramidal corners, bending it, distorting it, pumping it up. . . . Some of these test-ideas will lead to proof-ideas [1] (by arriving at something known to be true and then turning back, that is, by following the Pappian analysis-synthesis pattern), but some—like Zeta's 'double-pasting test'—will lead us, not back to something already known, but to real novelty, to some heuristic refutation of the tested proposition—*not by extending a naive concept, but by extending the theoretical framework*. This sort of refutation is self-explanatory. . . .

[1] Pólya discriminates between 'simple' and 'severe' tests. 'Severe' tests may give 'the first hint of a proof' ([1954], Vol. I, pp. 34-40).

IOTA: How dialectical! Tests turning into proofs, counterexamples that become examples by the very method of their construction. . . .

PI: Why dialectical? The test of one proposition turns into the proof of *another*, deeper proposition, counterexamples of the first into examples of the second. Why call confusion dialectic? But let me come back to my point: I do not think that my second main pattern of deductive guessing could be regarded—as Alpha would have it—as continuous growth of knowledge.

ALPHA: Of course it can. Compare our method with Omega's idea of replacing one proof-idea with a radically different, deeper one. Both methods increase content, but while in Omega's method one *replaces* operations of the proof that are applicable in a narrow domain by operations which are applicable in a wider domain, or, more radically, replaces the whole proof by one that is applicable in a wider domain—deductive guessing *extends* the given proof by adding operations which widen its applicability. Is this not continuity?

SIGMA: That is right! We deduce from the theorem a chain of ever wider theorems! From the special case ever more general cases! Generalisation by deduction! [1]

PI: But full of counterexamples, once you recognise that *any* increase of content, *any* deeper proof follows or generates heuristic refutations of the previous poorer theorems. . . .

ALPHA: Theta expanded ' counterexample ' to cover heuristic counterexamples. You now expand it to cover heuristic counterexamples that never actually exist. Your claim that your ' second pattern ' is full of counterexamples is based on the expansion of the concept of counterexample to counterexamples with zero life-time, whose discovery coincides with their explanation! But why should all intellectual activity, every struggle for increased content in a unified theoretical framework, be ' critical '? Your dogmatic ' critical attitude ' is obscuring the issue!

TEACHER: The issue between you and Pi is certainly obscure—for your ' continuous growth ' and Pi's ' critical growth ' are perfectly consistent. I am more interested in the *limitations*, if any, of deductive guessing, or ' continuous criticism '.

[1] In *informal* logic there is nothing wrong with the ' fact, so usual in mathematics and still so surprising to the beginner, or to the philosopher who takes himself for advanced, that the general case can be logically equivalent to a special case ' (Pólya [1954], Vol. I, p. 17). Also cf. Poincaré [1902], pp. 31-33.

(*e*) *The limits of the increase in content. Theoretical versus naive refutations*

PI: I think that sooner or later 'continuous' growth is bound to reach a dead-end, a *saturation point* of the theory.

GAMMA: But surely I can always stretch some of the concepts!

PI: Of course. *Naive* concept-stretching may go on—but *theoretical* concept-stretching has limits! Refutations by naive concept-stretching are only gadflies that prod us to catch up by theoretical concept-stretching. So there are two sorts of refutations. We *stumble* on the first sort by coincidence or good fortune, or by an arbitrary expansion of some concept. They are like miracles, their 'anomalous' behaviour is unexplained; we accept them as *bona fide* counterexamples only because we are used to accepting concept-stretching criticism. I shall call these *naive* counterexamples or *freaks*. Then there are the *theoretical counterexamples*: these are either originally produced by proof-stretching or, alternatively, they are freaks which are reached by stretched proofs, explained by them, and thereby raised to the status of theoretical counterexamples. Freaks have to be looked upon with great suspicion: they may not be genuine counter-examples, but instances of a quite different theory—if not outright mistakes.

SIGMA: But what shall we do when we get stuck? When we cannot turn our naive counterexamples into theoretical ones by expanding our original proof?

PI: We may probe again and again whether or not our theory still has some hidden capacity for growth. Sometimes, however, we have good reason to give up. For instance, as Theta rightly pointed out, if our deductive guessing starts from a vertex we cannot very well ever expect it to explain the vertexless cylinder.

ALPHA: So after all, the cylinder was not a monster, but a freak!

THETA: But freaks should not be played down! They are the *real* refutations: they cannot be fitted into a pattern of continuous 'generalisations', and may actually force us to revolutionise our theoretical framework. . . . [1]

[1] Cayley [1861] and Listing [1861] took the stretching of the basic concepts of the theory of polyhedra seriously. Cayley defined *edge* as 'the path from a summit to itself, or to any other summit' but allowed edges to degenerate into vertexless closed curves, which he called 'contours' (p. 426). Listing had one term for edges, whether with two, one, or no vertices: '*lines*' (p. 104). Both realised that a completely new theory was needed to explain the 'freaks' which they naturalised with their liberal conceptual framework—Cayley invented the '*Theory of Partitions of a Close*', Listing, one of the great pioneers of modern topology, the '*Census of Spatial Complexes*'.

OMEGA: Good! One may get to a *relative saturation point* of a *particular* chain of deductive guessing—but then one finds a revolutionary, new, deeper proof-idea that has more explanatory power. At the end one still gets to a *final* proof—without limit, without saturation point, without freaks to refute it!

PI: What? A single unified theory to explain *all* the phenomena of the universe? Never! Sooner or later we shall approach something like an *absolute saturation point.*

GAMMA: I don't really mind whether we do or not. If a counterexample can be explained by a cheap, *trivial* extension of the proof, I would already regard it as a freak. I repeat: I really do not see any point in generalising 'polyhedron' to include a polyhedron with cavities: this is not one polyhedron, but a class of polyhedra. I would also forget about 'multiply-connected faces'—why not draw the missing diagonals? As to the generalisation that includes twin-tetrahedra, I would reach for my gun: it only serves for making up complicated, pretentious formulas for nothing.

RHO: At last you rediscover my method of monster-adjustment![1] It relieves you of shallow generalisation. Omega should not have called content 'depth'; *not every increase in content is also an increase in depth:* think of (6) and (7)! [2]

[1] See Part II, pp. 127-130 and pp. 135-136

[2] Quite a few mathematicians cannot distinguish the trivial from the non-trivial. This is especially awkward when a lack of feeling for relevance is coupled with the illusion that one can construct a *perfectly complete* formula that covers all conceivable cases (cf. p. 310, footnote 2). Such mathematicians may work for years on the 'ultimate' generalisation of a formula, and end up by extending it with a few trivial corrections. The excellent mathematician, J. C. Becker, provides an amusing example: after many years' work he produced the formula $V-E+F=4-2n+q$ where n is the number of cuts that is needed to divide the polyhedral surface into simply-connected surfaces for which $V-E+F=1$, and q is the number of diagonals that one has to add to reduce all the faces to simply-connected ones ([1869], p. 72). He was very proud of his achievement, which—he claimed—shed 'completely new light', and even 'brought to a conclusion' 'a subject in which people like Descartes, Euler, Cauchy, Gergonne, Legendre, Grunert, and von Staudt, took interest' before him (p. 65). But three names were missing from his reading list: Lhuilier, Jordan and Listing. When he was told about Lhuilier, he published a sad note, admitting that Lhuilier knew all this more than fifty years before. As for Jordan, he was not interested in ring-shaped faces, but happened to take an interest in open polyhedra with boundaries, so that in his formula m, the number of boundaries, figures in addition to n ([1866*a*], p. 86). So Becker—in a new paper [1869*a*]—combined Lhuilier's and Jordan's formulas into $V-E+F=2-2n+q+m$ (p. 343). But in his embarrassment he was too hasty, and had not digested Listing's long paper. So he sadly

ALPHA: So you would stop at (5) in my series?

GAMMA: Yes. (6) and (7) are not growth, but degeneration! Instead of going on to (6) and (7), I would rather find and explain some *exciting* new counterexample! [1]

ALPHA: You may be right after all. But who decides *where* to stop? Depth is only a matter of taste.

GAMMA: Why not have mathematical critics just as you have literary critics, to develop mathematical taste by public criticism? We may even stem the tide of pretentious trivialities in mathematical literature.[2]

SIGMA: If you stop at (5) and turn the theory of polyhedra into a theory of triangulated spheres with *n* handles, how can you, if the need arises, deal with trivial anomalies like those explained in (6) and (7)?

MU: Child's play!

THETA: Right. Then we stop at (5) for the moment. *But can we stop*? Concept-stretching may refute (5)! We may ignore the stretching of a concept if it yields a counterexample that shows up the poverty of the content of our theorem. But if the stretching yields a counterexample that shows up its plain falsehood, what then? We may refuse to apply our content-increasing *Rule 4* or *Rule 5* to explain a freak, but we have to apply our content-preserving *Rule 2* to ward off refutation by a freak.

concluded his [1869*a*] with 'Listing's generalisation is still wider'. (By the way, he later tried to extend his formula also to star-polyhedra ([1874]; cf. Part II, p. 128, footnote 2.)

[1] Some people may entertain philistine ideas about *a law of diminishing returns in refutations*. Gamma, for one, certainly does not. We shall not discuss *one-sided* polyhedra (Möbius, [1865]) or *n-dimensional* polyhedra (Schläfli, [1852]). These would confirm Gamma's expectation that totally unexpected concept-stretching refutations may always give the whole theory a new—possibly revolutionary—push.

[2] Pólya points out that shallow, cheap, generalisation is 'more fashionable nowadays than it was formerly. It dilutes a little idea with a big terminology. The author usually prefers to take even that little idea from somebody else, refrains from adding any original observation, and avoids solving any problem except a few problems arising from the difficulties of his own terminology. It would be very easy to quote examples, but I don't want to antagonize people' ([1954], Vol. I, p. 30). Another of the greatest mathematicians of our century, John von Neumann, also warned against this 'danger of degeneration', but thought it would not be so bad 'if the discipline is under the influence of men with an exceptionally well-developed taste' ([1947], p. 196). One wonders, though, whether the 'influence of men with an exceptionally well-developed taste' will be enough to save mathematics in our 'publish or perish' age.

GAMMA: That is it! We may dismiss cheap '*generalisations*', but we can hardly dismiss 'cheap' *refutations*.

SIGMA: Why not build up a monster-barring definition of 'polyhedron', adding a new clause for each freak?

THETA: In both cases our old nightmare, vicious infinity, is back again.

ALPHA: While you are increasing content, you develop ideas, do mathematics; after it you clarify concepts, do linguistics. Why not stop altogether when one stops increasing content? Why be trapped in vicious infinities?

MU: Not mathematics versus linguistics again! Knowledge never profits from such disputes.

GAMMA: The term 'never' soon turns into 'soon'. I am all for taking up our old discussion again.

MU: But we already ended up in a deadlock! Or does anybody have anything new to say?

KAPPA: I think I have.

9 *How Criticism may turn Mathematical Truth into Logical Truth*

(*a*) *Unlimited concept-stretching destroys meaning and truth*

KAPPA: Alpha already said that our 'old method' leads to vicious infinity.[1] Gamma and Lambda answered with the hope that the stream of refutations might peter out:[2] but now that we understand the mechanism of refutational success—concept-stretching—we know that theirs was a vain hope. For any proposition there is always some sufficiently narrow interpretation of its terms, such that it turns out true, and some sufficiently wide interpretation such that it turns out false. Which interpretation is intended and which unintended depends of course on our intentions. The first interpretation may be called the *dogmatist, verificationist or justificationist interpretation*, the second the *sceptical, critical or refutationist interpretation*. Alpha called the first a conventionalist stratagem[3]—but now we see that the second is one too. You all ridiculed Delta's dogmatist interpretations of the naive conjecture[4] and then Alpha's dogmatist interpretation of the theorem.[5] But concept-stretching will refute *any* statement, and will leave no true statement whatsoever.

[1] See Part III, p. 232

[2] See Part III, p. 233

[3] Alpha in fact did not use this Popperian term explicitly; see Part I, p. 23.

[4] See Part I §4,(*b*)

[5] See Part III, §5

Gamma: Wait. True, we stretched ' polyhedron '—then tore it up and threw it away: as Pi pointed out, the naive concept ' polyhedron ' does not figure in the theorem any more.

Kappa: But then you will start stretching a term in the theorem—a theoretical term, won't you? You yourself chose to stretch ' simply-connected face ' to include the circle and the jacket of the cylinder.[1] You implied that it was a matter of intellectual honesty to stick one's neck out, to achieve the respectable status of refutability, i.e. to make the refutationist interpretation possible. But because of concept-stretching, refutability means refutation. So you slide onto the infinite slope, refuting *each* theorem and replacing it by a more ' rigorous ' one—by one whose falsehood has not been ' exposed ' yet! But *you never get out of falsehood.*

Sigma: What if we stop at a certain point, adopt justificationist interpretations, and don't budge either from the truth or from the particular linguistic form in which that truth was expressed?

Kappa: Then you will have to ward off concept-stretching counter-examples with monster-barring definitions. Thus you will slide on to another infinite slope: you will be forced to admit of each ' particular linguistic form ' of your true theorem that it was not precise enough, and you will be forced to incorporate in it more and more ' rigorous ' definitions couched in terms whose vagueness has not been exposed yet! But *you never get out of vagueness.*

Theta [*aside*]: What is wrong with a heuristic where vagueness is the price we pay for growth?

Alpha: I told you: precise concepts and unshakable truths do not dwell in language, but only in thought!

Gamma: Let me challenge you, Kappa. Take the theorem as it stood, after we took account of the cylinder: ' For all simple objects with simply-connected faces such that the edges of the faces terminate in vertices $V-E+F=2$.' How would you refute *this* by the method of concept-stretching?

Kappa: First I go back to the defining terms and spell out the proposition in full. Then I decide which concept to stretch. For instance, ' simple ' stands for ' stretchable onto a plane after having had a face removed '. I shall stretch ' stretching '. Take the already discussed twin-tetrahedra—the pair with an *edge* in common (Fig. 6*a*). It is simple, its faces are simply-connected, but $V-E+F=3$. So our theorem is false.

[1] See Part III, pp. 221-225

GAMMA: But this twin-tetrahedron is *not* simple!

KAPPA: Of course it is simple. Removing any face, I can stretch it on to a plane. I just have to be careful, when I get to the critical edge, that I do not tear anything there when opening the second tetrahedron along that edge.

GAMMA: But this is not stretching! You *tear*—or *split*—the edge into two edges! You certainly cannot map one point onto two: *stretching is a bicontinuous one-one mapping*!

KAPPA: *Def. 9*? I am afraid this narrow, dogmatist interpretation of 'stretching' does not appeal to *my* common sense. For instance,

(a) (b)

FIG. 24

I can well imagine stretching a square (Fig. 24*a*) into two nested squares by stretching the boundary lines (Fig. 24*b*). Would you call this stretch a tear or a split, just because it is not a 'bicontinuous one-one mapping'? By the way, I wonder why you did not define stretching as a transformation that leaves V, E and F unaltered, and have done with it?

GAMMA: Right, you win again. I either have to agree to your refutationist interpretation of 'stretching' and expand my proof, or find a deeper one, or incorporate a lemma—or I have to introduce a new monsterbarring definition. Yet in any of these cases I shall always make my defining terms clearer and clearer. Why should I not arrive at a point where the meanings of the terms will be so crystal clear that there will only be one single interpretation, as is the case with $2+2=4$? There is nothing elastic about the meaning of these terms and nothing refutable about the truth of this proposition, which shines for ever in the natural light of reason.

KAPPA: Dim light!

GAMMA: Stretch, if you can.

KAPPA: But this is child's play! In certain cases two and two make five. Suppose we ask for the delivery of two articles each weighing two pounds; they are delivered in a box weighing one pound; then in this package two pounds and two pounds will make five pounds!

GAMMA: But you get five pounds by adding *three* weights, 2 and 2 and 1!

KAPPA: True, our operation '2 and 2 make 5' is not an addition in the originally intended sense. But we can make the result hold true by a simple stretching of the meaning of addition. Naive addition is a very special case of packing where the weight of the covering material is zero. We have to build this lemma into the conjecture as a condition: our improved conjecture will be: '$2+2=4$ for "weightless" addition'.[1] The whole story of algebra is a series of such concept- and proof-stretchings.

GAMMA: I think you take 'stretching' a bit far. Next time you will interpret 'plus' as 'times' and consider it a refutation! Or you will interpret 'all' as 'no' in 'All polyhedra are polyhedra'! You stretch the concept of concept-stretching! We have to demarcate refutation by *rational stretching* from 'refutation' by *irrational stretching*. We cannot allow you to stretch any term you like just as you like.

We must pin down the concept of counterexample in crystal-clear terms!

DELTA: Even Gamma has turned into a monsterbarrer: now he wants a monsterbarring definition of concept-stretching refutation. *Rationality, after all, depends on inelastic, exact, concepts*![2]

KAPPA: *But there are no such concepts! Why not accept that our ability to specify what we mean is nil, therefore our ability to prove is nil?* If you want mathematics to be meaningful, you must resign of certainty. If you want certainty, get rid of meaning. You cannot have both. *Gibberish is safe from refutations, meaningful propositions are refutable by concept-stretching.*

GAMMA: Then your last statements can also be refuted—and you know it. 'Sceptics are not a sect of people who are persuaded of what they say, but a sect of liars.'[3]

KAPPA: Swear-words: the last resort of reason!

(b) *Mitigated concept-stretching may turn mathematical truth into logical truth*

THETA: I think Gamma is right about the need for demarcating rational from irrational concept-stretching. For concept-stretching

[1] Cf. Félix [1957], p. 9

[2] Gamma's demand for a crystal-clear definition of 'counterexample' amounts to a demand for crystal-clear, inelastic concepts in the metalanguage as a condition of rational discussion.

[3] Arnauld [1724], pp. xx–xxi

has come a long way, and has changed from a mild, rational activity to a radical, irrational one.

Originally, criticism concentrates exclusively on the *slight* stretching of *one particular* concept. It has to be *slight*, so that we do not notice it; if its real—stretching—nature were discovered, it might not be accepted as legitimate criticism. It concentrates on *one particular* concept, as in the case of our rather unsophisticated universal propositions: '*All A's are B's*'. Criticism then means finding a slightly stretched *A* (in our case *polyhedron*) that is not *B* (in our case *Eulerian*).

But Kappa sharpened this in two directions. First, to submit *more than one* constituent of the proposition under attack to concept-stretching criticism. Second, to turn concept-stretching from a surreptitious and rather modest activity into *open deformation* of the concept, like the deformation of 'all' into 'no'. Here any meaningful translation of the terms under attack that renders the theorem false is accepted as refutation. I would then say that *if a proposition cannot be refuted with respect to the constituents a, b, . . ., then it is logically true with respect to these constituents.*[1] Such a proposition is the end-result of a long critical-speculative process in the course of which the meaning-load of some terms is completely transferred to the remaining terms and to the form of the theorem.

Now all that Kappa says is that there are no propositions which are logically true with respect to *all* their constituents. But there may be logically true propositions with respect to *some* constituents, so that the stream of refutations can only be opened up again if new stretchable constituents are added. If we go the whole hog, we end up in irrationalism—but we need not. Now where should we draw the borderline? We may very well allow concept-stretching only for a distinguished subset of constituents which become the prime targets of criticism. Logical truth will not depend on their meaning.

SIGMA: So after all we took Kappa's point: we made truth independent of the meaning of at least *some* of the terms!

THETA: That is right. But if we want to defeat Kappa's scepticism, and escape his vicious infinities, we certainly have to stop concept-stretching at the point where it ceases to be a tool of growth and becomes a tool of destruction: we may have to find out which are

[1] This is a slightly paraphrased version of Bolzano's definition of logical truth ([1837], § 147). Why Bolzano, in the 1830's, proposed his definition, is a puzzling question, especially since his work anticipates the concept of model, one of the greatest innovations in nineteenth-century mathematical philosophy.

those terms whose meaning can be stretched only at the cost of destroying the basic principles of rationality.[1]

KAPPA: Can we stretch the concepts in your theory of critical rationality? Or will that be manifestly true, formulated in unstretchable, exact terms which do not need to be defined? Will your theory of criticism end in a ' retreat to commitment ': is everything criticisable except for your theory of criticism, your ' metatheory '?[2]

OMEGA [*to Epsilon*]: I do not like this shift from Truth to rationality. *Whose* rationality? I sense conventionalist infiltration.

BETA: What are you talking about? I understand Theta's ' mild pattern ' of concept-stretching. I also understand that concept-stretching may attack more than one term: we saw this when Kappa stretched ' stretching ' or when Gamma stretched ' all '. . . .

SIGMA: Surely Gamma stretched ' simply-connected '!

BETA: But no. ' Simply-connected ' is an abbreviation—he only stretched the term ' all ' that occurred among the defining terms.[3]

THETA: Come back to the point. You are unhappy about ' open ', radical concept-stretching?

[1] Nineteenth-century mathematical criticism stretched more and more concepts, and shifted the meaning-load of more and more terms onto the *logical form* of the propositions and onto the meaning of the few (as yet) unstretched terms. In the 1930's this process seemed to slow down and the demarcation line between unstretchable (' logical ') terms and stretchable (' descriptive ') terms seemed to become stable. A list, containing a small number of logical terms came to be widely agreed upon, so that a general definition of logical truth became possible; logical truth was no longer ' with respect to ' an *ad hoc* list of constituents. (Cf. Tarski [1935].) Tarski was however puzzled about this demarcation and wondered whether, after all, he would have to return to a relativised concept of counterexample, and consequently, of logical truth (p. 420)—like Bolzano's, of which, by the way, Tarski did not know. The most interesting result in this direction was Popper's [1947-48] from which it follows that one cannot give up further logical constants without giving up some basic principles of rational discussion.

[2] ' Retreat to commitment ' is Bartley's expression [1962]. He investigates the problem of whether a rational defence of critical rationalism is possible mainly with respect to *religious* knowledge—but the problem-patterns are very much the same with respect to *mathematical* knowledge.

[3] See Part III, pp. 221-225. Gamma did, in fact, want to remove some meaning-load from ' all ', so that it no longer applied only to non-empty classes. The modest stretching of ' all ' by removing ' existential import ' from its meaning and thereby turning the empty set from a monster into an ordinary *bourgeois* set was an important event—connected not only with the Boolean set-theoretical re-interpretation of Aristotelian logic, but also with the emergence of the concept of vacuous satisfaction in mathematical discussion.

BETA: Yes. Nobody would accept this last brand as genuine refutation! I quite see that the mild concept-stretching trend of heuristic criticism that Pi uncovered is a most important vehicle of mathematical growth. But mathematicians will never accept this last, wild form of refutation!

TEACHER: You are wrong, Beta. They *did* accept it, and their acceptance was a turning point in the history of mathematics. *This revolution in mathematical criticism changed the concept of mathematical truth, changed the standards of mathematical proof, changed the patterns of mathematical growth*! [1] But now let us close our discussion for the time being: we shall discuss this new stage some other time.

SIGMA: But then nothing is settled. We can't stop *now*.

TEACHER: I sympathise. This latest stage will have important feed-backs to our discussion.[2] But a scientific inquiry ' begins and ends with problems '.[3] [*Leaves the classroom*].

BETA: But I had no problems at the beginning! And now I have nothing *but* problems!

(*Concluded*)

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[1] The concepts of criticism, counterexample, consequence, truth, and proof are inseparable; when they change, *the primary change occurs in the concept of criticism* and changes in the others follow.

[2] Cf. Lakatos [1962]

[3] Popper [1963*b*], p. 968

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ERRATA IN I. LAKATOS 'PROOFS AND REFUTATIONS' PARTS I-III

Part I

p. 2, last line:
Instead of 'Part III' *read* 'Part IV'
p. 24, last line of the main text:
Instead of 'very small corner.' *read* 'very small corner.[2]'

Part II

p. 134, last line but one:
Instead of '[1927]' *read* '[1925]'
p. 137, last line but one:
Instead of '[1951]' *read* '[1941]'

Part III

p. 222, eighth line from the top:
Instead of '$V—E+F=0$' *read* '$V—E+F=1$'
p. 225, third line of the footnote:
Instead of '[1806-27]' *read* '[1826-27]'
p. 231, sixth line from the top:
Instead of 'suitable' *read* 'suitably'
p. 231, first line of the footnotes:
Instead of 'p. 214' *read* 'p. 216'
p. 233, second line of the footnotes:
Instead of '[*c.* 195]' *read* '[*c.* 190]'
p.234, first two lines of the footnote:
Instead of 'Pascal [1654), 432' *read* 'Pascal [1654]. Cf. *Oeuvres complètes de Pascal, texte établi et annoté par Jacques Chevalier*, Paris, 1954, pp. 1206-7.'
p. 234, second line from the top:
Instead of 'criticism and logic?[1]' *read* 'criticism and logic?'
p. 235, fourth line from the bottom:
Instead of "'Certainty is never achieved'" *read* "'Certainty' is never achieved"
p. 237, twenty-first line from the top:
Instead of 'Gamma' *read* 'Omega'
p. 244, first line of the footnotes:
Instead of '[1900]' *read* '[1899]'
p. 244, third footnote, eighteenth line from the bottom:
Instead of '[1751])' *read* '([1858])'
p. 245, tenth line from the top:
Instead of 'Beta's' *read* 'Zeta's'