EPSILON: I am glad that at least Theta finally caught on. My proof can in fact be cast in a system of which the dominant theory is logic. The conditional statement with all the lemmas incorporated as antecedents can be proved in this system, and we know that (relative to the given stock of formative 'logical' terms) there *are* no counterexamples to any statement which can be proved in this way. No matter how the descriptive terms are re-interpreted, this conditional statement will remain true.

LAMBDA: How do 'we know'?

EPSILON: We don't know for *certain* – it is an informal theorem about logic. But, moreover, we know that, presented with any alleged proof in such a system, we can check completely mechanically using a procedure which is guaranteed to produce an answer in a finite number of steps, whether or not it is indeed a proof. In such systems, then, your 'proof-analysis' reduces to a triviality.

ALPHA: But you would agree, Epsilon, that 'proof-analysis' retains its importance in informal mathematics; and that formal proofs are always translations of informal proofs and that the problems that have been raised about translation are very real.

LAMBDA: But anyway, Epsilon, how do we know that proof checking is always accurate?

EPSILON: Really Lambda, your unquenchable thirst for certainty is becoming tiresome! How many times do I have to tell you that we know nothing for certain? But your desire for certainty is making you raise very boring problems – and is blinding you to the interesting ones.

APPENDIX I

ANOTHER CASE-STUDY IN THE METHOD OF PROOFS AND REFUTATIONS

1. *Cauchy's Defence of the 'Principle of Continuity'*

The method of proofs and refutations is a very general heuristic pattern of mathematical discovery. However, it seems that it was discovered only in the 1840s and even today seems paradoxical to many people; and certainly it is nowhere properly acknowledged. In this appendix I shall try to sketch the story of a proof-analysis in mathematical analysis and to trace the sources of resistance to the understanding and recognition of it. I first repeat the skeleton of the method of proofs and refutations, a method which I have already illustrated by my case-study of the Cauchy proof of the Descartes–Euler conjecture.

There is a simple pattern of mathematical discovery – or of the growth of informal mathematical theories. It consists of the following stages:[1]

(1) *Primitive conjecture.*

(2) *Proof (a rough thought-experiment or argument, decomposing the primitive conjecture into subconjectures or lemmas).*

(3) *'Global' counterexamples (counterexamples to the primitive conjecture) emerge.*

(4) *Proof re-examined: the 'guilty lemma' to which the global counterexample is a 'local' counterexample is spotted. This guilty lemma may have previously remained 'hidden' or may have been misidentified. Now it is made explicit, and built into the primitive conjecture as a condition. The theorem – the improved conjecture – supersedes the primitive conjecture with the new proof-generated concept as its paramount new feature.*[2]

[1] As I have stressed the actual historical pattern may deviate slightly from this heuristic pattern. Also the fourth stage may sometimes precede the third (even in the heuristic order) – an ingenious proof analysis may suggest the counterexample.

[2]* *Editors' note:* In other words this method consists (in part) of producing a series of statements $P_1, \ldots, P_n$ such that P_1 &... & P_n is supposed to be true of some domain of interesting objects and seems to imply the primitive conjecture C. This may turn out not to be the case – in other words we find cases in which C is false ('global counterexamples') but in which P_1 to P_n hold. This leads to the articulation of a new lemma

These four stages constitute the essential kernel of proof analysis. But there are some further standard stages which frequently occur:

(5) *Proofs of other theorems are examined to see if the newly found lemma or the new proof-generated concept occurs in them: this concept may be found lying at cross-roads of different proofs, and thus emerge as of basic importance.*

(6) *The hitherto accepted consequences of the original and now refuted conjecture are checked.*

(7) *Counterexamples are turned into new examples – new fields of inquiry open up.*

I should now like to consider another case-study. Here the *primitive conjecture* is that the limit of any convergent series of continuous functions is itself continuous. It was Cauchy who gave the first proof of this conjecture, whose truth had been taken for granted and assumed therefore not to be in need of any proof throughout the eighteenth century. It was regarded as the special case of the 'axiom' according to which 'what is true up to the limit is true at the limit'.[1] We find the conjecture and its proof in Cauchy's celebrated [1821] (p. 131).

Given that this 'conjecture' had hitherto been regarded as trivially true, why did Cauchy feel the need to prove it? Had someone criticized the conjecture?

As we shall see, the situation was not quite so simple. With the benefit of hindsight we can now see that counterexamples to the Cauchy conjecture had been provided by Fourier's work. Fourier's *Mémoire sur la Propagation de la Chaleur*[2] actually contains an example of what, according to present notions, is a convergent series of continuous functions which tends to a Cauchy discontinuous function, namely:

$$\cos x - \tfrac{1}{3}\cos 3x + \tfrac{1}{5}\cos 5x - \ldots \qquad (1)$$

P_{n+1} which is also refuted by the counterexample ('local counterexample'). The original proof is thus replaced by a new one which can be summed up by the conditional statement

$$P_1 \,\&\ldots\&\, P_n \,\&\, P_{n+1} \to C.$$

The (logical) truth of this conditional statement is no longer impugned by the counterexample (since the antecedent is now false in this case and hence the conditional statement true).

[1] Whewell [1858], I. p. 152. Whewell is in 1858 at least ten years out of date. The principle stems from Leibniz's principle of continuity ([1687], p. 744). Boyer in his [1939], p. 256, quotes a characteristic restatement of the principle from Lhuilier [1786], p. 167.

[2] This *Mémoire* was awarded the *grand prix de mathématiques* for 1812, having been refereed by Laplace, Legendre and Lagrange. It was published only after Fourier's classical *Théorie de la Chaleur* which appeared in 1822, a year after Cauchy's textbook, but the content of the *Mémoire* was then already well known.

Fourier's own attitude to this series is, however, quite clear (and clearly different from this modern one):

(*a*) He states that it is everywhere convergent.

(*b*) He states that its limit function is composed of separate straight lines, each of which is parallel to the x-axis, and equal to the circumference [that is π]. These parallels are situated alternately above and below the axis, with a distance of $\pi/4$, and are joined by perpendiculars which themselves make part of the line.[1]

Fourier's words about the perpendiculars in the graph are telling. He considered these limit functions to be (in some sense) continuous. In fact, Fourier certainly regarded anything as a continuous function if its graph could be drawn with a pencil which is not lifted from the paper. Thus Fourier would not have regarded himself as having constructed counterexamples to Cauchy's continuity axiom.[2] It was only in the light of Cauchy's subsequent characterisation of continuity that the limit functions in some of Fourier's series came to be regarded as discontinuous, and thus that the series themselves came to be seen as counterexamples to Cauchy's conjecture. Given this new, and counter-intuitive definition of continuity, Fourier's innocent continuous drawings seemed to become wicked counterexamples to the old, long established continuity principle.

Cauchy's definition certainly translated the homely concept of continuity into arithmetical language in such a way that 'ordinary

[1] Fourier, *op. cit.*, sections 177 and 178.

[2] After writing this I discovered that the term 'discontinuous' appears in roughly the Cauchy sense in some hitherto unpublished manuscripts of Poisson (1807) and of Fourier (1809), which were being studied by Dr. J. Ravetz, who kindly permitted me to look at his photostats. This certainly complicates my case, though it does not refute it. Fourier obviously had two different notions of continuity in mind at different times, and indeed these two different notions arise quite naturally from two different domains. If we interpret a function like:

$$\sin x - \tfrac{1}{2}\sin 2x + \tfrac{1}{3}\sin 3x - \ldots$$

as the initial position of a string, it will certainly be considered as continuous, and to cut out the perpendicular lines – as was to be required by Cauchy's definition – will seem unnatural. But if we interpret this function as, say, representing temperature along a wire, the function will seem obviously discontinuous. These considerations suggest two conjectures. Firstly, Cauchy's celebrated definition of continuity, which runs counter to the 'string-interpretation' of a function, may have been stimulated by Fourier's investigation of heat phenomena. Secondly, Fourier's insistence on the perpendiculars in the graphs of these (according to the 'heat-interpretation') discontinuous functions may have stemmed from an effort not to come into conflict with the Leibniz principle. **Editors' note*: For further information on Fourier's mathematics, see I. Grattan-Guinness (in collaboration with J. R. Ravetz), *Joseph Fourier, 1768–1830* (M.I.T. Press, 1972).

commonsense' could only be shocked.[1] What sort of continuity is it that implies that if we rotate the graph of a continuous function a little, it turns into a discontinuous one?[2]

So if we replace the intuitive concept of continuity by the Cauchy concept then (and only then!) does the axiom of continuity seem to be contradicted by Fourier's results. This looks like a strong, perhaps decisive, argument against Cauchy's new definitions (not only of continuity, but also other concepts like that of limit). No wonder then that Cauchy wanted to show that he could indeed prove the continuity axiom in his new interpretation of it, thereby providing the evidence that his definition satisfies this most stringent adequacy requirement. He succeeded in providing the proof – and thought he had thereby dealt a mortal blow to Fourier, that talented but woolly and unrigorous dilettante, who had unintentionally challenged his definition.

Of course if Cauchy's proof were correct, then Fourier's examples, despite appearances, could not be real *counter*examples. One way of showing that they were not real counterexamples would be to show that the series apparently converging to functions which were discontinuous in Cauchy's sense were not convergent at all!

And this was a plausible guess. Fourier himself was doubtful about the convergence of his series in these critical cases. He noticed that the convergence was slow: 'The convergence is not sufficiently rapid to produce an easy approximation, but it suffices for the truth of the equation.'[3]

With hindsight we can see that Cauchy's hope that in these critical cases Fourier's series do not converge (and thus do not represent the function) was also justified in a way by the following fact. Where the limit function is discontinuous, the series tends to $\frac{1}{2}[f(x+0)+f(x-0)]$, and not simply to $f(x)$. It tends to $f(x)$ only if $f(x) = \frac{1}{2}[f(x+0)+f(x-0)]$. But this was not known before 1829, and in fact general opinion was at

[1] That is string-commonsense or graph-commonsense.

[2]* *Editors' note:* What is violated here is, perhaps, not our intuitive notion of continuity, but rather our belief that any graph representing a function would still represent some function when slightly rotated. Fourier's curve is continuous from an intuitive point of view, and this intuition can still be accounted for by the ϵ, δ definition of continuity (with which Cauchy is usually credited); for Fourier's curve, complete with perpendiculars, is parametrically representable by two continuous functions.

[3] *Op. cit.*, section 177. This remark, of course, is a far cry from the discovery that the convergence is in these places infinitely slow, which was made only after 40 years experience in calculating Fourier series. And this discovery could not possibly be made before Dirichlet's decisive improvement on Fourier's conjecture showing that only those functions can be represented by Fourier series whose value at the discontinuities is $\frac{1}{2}[f(x+0)+f(x-0)]$.

first behind Fourier rather than Cauchy. Fourier's series seemed to work and when Abel, in 1826, five years after the publication of Cauchy's proof, mentioned in a footnote of his [1826*b*],[1] that there are 'exceptions' to Cauchy's theorem, this constituted a rather intriguing double victory: Fourier series were accepted, but so was Cauchy's startling definition of continuity and the theorem he had proved using it.

It was precisely in view of this double victory that it now seemed that there must be *exceptions* to the specific version of the principle of continuity we are considering, even though Cauchy had flawlessly proved it.

Cauchy must have reached the same conclusion as Abel for in the same year he gave, without of course giving up his characterisation of continuity, a proof of the convergence of the Fourier series.[2] He must have been very ill at ease with the situation however. The second volume of the *Cours d'Analyse* was never published. And, which is still more suspicious, he produced no further editions of the first volume, allowing his pupil Moigno, when the pressure for a textbook had become too great, to publish his notes of his lectures.[3]

Given that Fourier's examples were now interpreted as counterexamples, the puzzle was evident: how could a proved theorem be false, or 'suffer exceptions'? We have already discussed how people in the same period were puzzled by the 'exceptions' to the Euler theorem despite the fact that it had been proved.

2. *Seidel's Proof and the Proof-Generated Concept of Uniform Convergence*

Everybody felt that this Cauchy–Fourier case was not just a harmless puzzle, but a fatal blemish on the whole of the new 'rigorous' mathematics. Dirichlet in his celebrated papers about Fourier series,[4] while preoccupied with showing exactly *how* convergent series of continuous functions represent discontinuous functions, and while obviously very much aware of the Cauchy version of the continuity principle, did not mention the obvious contradiction at all.

It was left to Seidel at last to solve the riddle by spotting the guilty hidden lemma in Cauchy's proof.[5] But this happened only in 1847. Why did it take so long? To answer this question we shall have to look at Seidel's celebrated discovery a little more closely.

[1] Abel [1826*b*], p. 316.

[2] Cauchy [1826]. The proof is based on an incorrigibly false assumption (see e.g. Riemann, [1868]).

[3] Moigno [1840–1].

[4] Dirichlet [1829].

[5] Seidel [1847].

Let $\Sigma f_n(x)$ be a convergent series of continuous functions and, for any n, define $S_n(x) = \sum_{m=0}^{n} f_m(x)$ and $r_n(x) = \sum_{m=n+1}^{\infty} f_n(x)$. Then the gist of Cauchy's proof is the inference from the premise:

Given any $\epsilon > 0$:

(1) there is δ such that for any b, if $|b| < \delta$, then $|S_n(x+b) - S_n(x)| < \epsilon$ (there is such a δ because of the continuity of $S_n(x)$);

(2) there is an N, such that $|r_n(x)| < \epsilon$ for all $n \geqslant N$ (there is such an N because of the convergence of $\Sigma f_n(x)$);

(3) there is an N' such that $|r_n(x+b)| < \epsilon$ for all $n \geqslant N'$ (there is such an N' because of the convergence of $\Sigma f_n(x+b)$);

to the conclusion that:

$$\begin{aligned} |f(x+b) - f(x)| &= |S_n(x+b) + r_n(x+b) - S_n(x) - r_n(x)| \\ &\leqslant |S_n(x+b) - S_n(x)| + |r_n(x)| + |r_n(x+b)| \\ &< 3\epsilon, \text{ for all } b < \delta \end{aligned}$$

Now the global counterexamples provided by series of continuous functions which converge to Cauchy-discontinuous functions show that something is wrong with this (roughly stated) argument. But where is the guilty lemma?

A slightly more careful proof analysis (using the same symbols as before, but making explicit the functional dependencies of some of the quantities) produces the following inference:

(1′) $|S_n(x+b) - S_n(x)| < \epsilon$ if $b < \delta(\epsilon, x, n)$

(2′) $|r_n(x)| < \epsilon$ if $n > N(\epsilon, x)$

(3′) $|r_n(x+b)| < \epsilon$ if $n > N(\epsilon, x+b)$

therefore

$$|S_n(x+b) + r_n(x+b) - S_n(x) - r_n(x)| = |f(x+b) - f(x)| < 3\epsilon$$

if $n > \max_z N(\epsilon, z)$ and $b < \delta(\epsilon, x, n)$.

The hidden lemma is that this maximum, $\max_z N(\epsilon, z)$, should exist for any fixed ϵ. This is what came to be called the requirement of *uniform convergence*.

There were probably three major impediments in the way of making this discovery.

The *first* was Cauchy's loose usage of 'infinitely small' quantities.[1] The *second* was that even if some mathematicians had noticed that the

[1] This prevented Cauchy from giving a clear critical appraisal of his old proof and even from formulating his theorem clearly in his [1853] (pp. 454–9).

assumption of the existence of a maximum of an infinite set of Ns is involved in this proof, they may very well have made it without a second thought. Existence proofs in maximum problems occur first in the Weierstrass school. But the *third* and main obstacle was the prevalence of Euclidean methodology – this good and evil spirit of early nineteenth century mathematics.

But before discussing this in general let us see how Abel solves the problem posed for the Cauchy theorem by the Fourier counter-examples. I shall show that he solves it (or rather 'solves' it) by the primitive 'exception-barring' method.[1]

3. *Abel's Exception-Barring Method*

Abel states the problem, which I claim to be the basic background problem of his celebrated paper on the binomial series,[2] only in a footnote. He writes: 'It seems to me that there are some exceptions to Cauchy's theorem', and immediately gives the example of the series

$$\sin\phi - \tfrac{1}{2}\sin 2\phi + \tfrac{1}{3}\sin 3\phi - \ldots$$ [3]

Abel adds that 'as it is known, there are many more examples like this'. His response to these counterexamples is to start guessing: 'What is the safe domain of Cauchy's theorem?'

His answer to this question is this: the domain of validity of the theorems of analysis in general, and that of the theorems about the continuity of the limit function in particular, is restricted to power series. All the known exceptions to this basic continuity principle were trigonometrical series, and so he proposed to withdraw analysis to within the safe boundaries of power series, thus leaving behind Fourier's cherished trigonometrical series as an uncontrollable jungle – where exceptions are the norm and successes miracles.

In a letter to Hansteen dated 29 March 1826, Abel characterised 'miserable Eulerian induction' as a method which leads to false and unfounded generalisations and he asks what the reason is for such procedures having in fact led to so *few* calamities. His answer is

To my mind the reason is that in analysis one is largely concerned with functions that can be represented by power-series. As soon as other functions enter – and this happens but rarely – then [induction] does not work any more and an

[1] See above, pp. 24–30.

[2] Abel [1826b], p. 316.

[3] Abel fails to mention that precisely this example had already been mentioned in this context by Fourier.

infinite number of incorrect theorems arise from these false conclusions, one leading to the others. I have investigated several of these and I was lucky enough to solve the problem...[1]

In Abel's paper, we find his famous theorem – which, I claim, stemmed from his grappling with the classical metaphysical principle of Leibniz – in the following restricted form:

If the series

$$f\alpha = v_0 + v_1\alpha + v_2\alpha^2 + \ldots + v_m\alpha^m + \ldots$$

is convergent for a given value δ of α, it will also converge for every value smaller than δ, and for steadily decreasing values of β, the function $f(\alpha-\beta)$ will approach the limit $f\alpha$ indefinitely, provided that α is smaller than or equal to δ.[2]

Modern rationalist historians of mathematics who consider the history of mathematics as the history of a homogeneous growth of knowledge on the basis of unchanging methodology, assume that anyone who discovers a global counterexample and proposes a new conjecture which is not subject to refutation by the counterexample in question, has automatically discovered the corresponding hidden lemma and proof-generated concept. In this way such students of history attribute the discovery of uniform convergence to Abel. So in the authoritative *Encyclopädie der Mathematischen Wissenschaften*, Pringsheim says that Abel 'demonstrated the existence of the property today called uniform convergence'.[3] Hardy shares Pringsheim's view. In his [1918] paper he says that 'the idea of uniform convergence is

[1] Letter to Hansteen ([1826a]). The rest of the letter is also interesting and reflects Abel's exception-barring method: 'When one proceeds by a general method, it is not too difficult; but I have had to be very circumspect, for propositions once accepted without rigorous proof (i.e. without any proof) are so rooted within me that I at each moment risk using them without further examination.' Thus Abel checked these general conjectures one after the other and tried to guess the domain of their validity.

This Cartesian self-imposed restriction to the absolutely clear power-series explains Abel's particular concern about the rigorous treatment of the Taylor-expansion: 'Taylor's theorem, the basis of all the infinitesimal calculus is not better founded. I have only found one rigorous demonstration and that is M. Cauchy's in his *Résumé des leçons sur le calcul infinitésimal*, where he demonstrated that one will have

$$\phi(x+a) = \phi(x) + a\phi'(x) + a^2\phi''(x) + \ldots$$

as long as the series is convergent; but one employs it without attention in all cases.' (Letter to Holmboë [1825].)

[2] Abel [1826b], I. p. 314. The text is a retranslation from German, (Crelle translated the original French into German).* *Editors' note:* It seems that Abel forgot the modulus sign around α.

[3] Pringsheim [1916], p. 34.

present implicitly in Abel's proof of his celebrated theorem'.[1] Bourbaki is even more explicitly false: according to him, Cauchy

did not at first perceive the distinction between simple convergence and uniform convergence, and considered himself able to demonstrate that every convergent series of continuous functions has as its sum a continuous function. The error was almost as soon revealed by Abel, who proved at the same time that every complete [?] series is continuous in the interior of its interval of convergence by the reasoning which has become classical and which uses essentially, in this particular, the idea of uniform convergence. It only remained to disentangle the latter in a general manner, which was done independently by Stokes and Seidel in 1847–8 and by Cauchy himself in 1853.[2]

So many sentences, so many mistakes. Abel did not reveal Cauchy's mistake in identifying the two sorts of convergences. His proof does not exploit the concept of uniform convergence any more than does Cauchy's. Abel's and Seidel's results are not in the relation of 'special' and 'general' – they are on quite different levels. Abel did not even notice that it is not the domain of eligible functions which has to be restricted, but rather the way they converge! *In fact for Abel there is only one sort of convergence, the simple one*; and the secret of the sham certainty of his proof lies in his cautious (and lucky) *zero-definitions*:[3] as we now know, in the case of power series, simple convergence coincides with uniform convergence![4]

[1] Hardy [1918], p. 148.

[2] Bourbaki [1949], p. 65 and [1960], p. 228.

[3] Cf. above, pp. 24–30.

[4] There were two mathematicians who noticed that Abel's proof was not quite flawless. One was Abel himself, who comes to grips with the problem again – without success – in his posthumously published paper 'Sur les Séries' ([1881], p. 202). The other was Sylow, the coeditor of the second edition of Abel's Collected Works. He added a critical footnote to the theorem, in which he pointed out that we have to require uniform convergence in the proof and not simple convergence, as Abel does. But he did not use the term 'uniform convergence' about which he did not seem to know, (the second edition of Jordan's *Cours d'Analyse* had not then appeared) and he referred instead to a later generalisation of du Bois-Reymond, which only shows that even he did not see clearly the nature of the flaw. Reiff, in his [1889], rejected Sylow's criticism with the naive argument that Abel's theorem is valid. Reiff says that while Cauchy was the founder of the theory of convergence, Abel was the founder of the theory of the continuity of series:

> Briefly summarizing the achievement of Cauchy and of Abel, we can say: Cauchy discovered the theory of the convergence and divergence of infinite series in his *Analyse Algébrique*, and Abel discovered the theory of the continuity of series in his *Treatise on the Binomial Series*. ([1889], pp. 178–9.)

To say this in 1889 was certainly a piece of pompous ignorance.

But of course the validity of Abel's theorem is due to the very narrow zero-definition, and not to the proof. Abel's paper was later published in *Ostwald's Klassiker* (Nr. 71), Leipzig, 1895. In the notes Sylow's remarks are reproduced without any comment.

Whilst I am criticizing the historians I should just mention that the first counterexample to Cauchy's theorem has generally been attributed to Abel. That it occurs in Fourier was noticed only by Jourdain. But he, in the ahistorical spirit already noted, draws from this fact the consequence that Fourier, for whom Jourdain had a great admiration, came close to discovering the concept of uniform convergence.[1] The point that a counterexample may have to fight for recognition, and when recognised it still may not lead automatically to the hidden lemma and thereby to the proof-generated concept in question, has been missed by all historians so far.

4. *Obstacles in the Way of the Discovery of the Method of Proof-Analysis*

But now let us return to the main problem. Why did the leading mathematicians from 1821 to 1847 fail to find the simple flaw in Cauchy's proof and improve both the proof-analysis and the theorem?

The first reply is that they did not know about the method of proofs and refutations. They did not know that after the discovery of a counterexample they had to analyse their proof carefully and try to find the guilty lemma. They dealt with global counterexamples with the help of the heuristically sterile exception-barring method.

In fact, Seidel discovered the proof-generated concept of uniform convergence and the method of proofs and refutations at one blow. He was fully conscious of his methodological discovery[2] which he stated in his paper with great clarity:

> Starting from the certainty just achieved, that the theorem is not universally valid, and hence that its proof must rest on some extra hidden assumption, one then subjects the proof to a more detailed analysis. It is not very difficult to discover the hidden hypothesis. One can then infer backwards that this condition expressed by the hypothesis is not satisfied by series which represent discontinuous functions, since only thus can the agreement between the otherwise correct proof sequence, and what has been on the other hand established, be restored.[3]

What prevented the generation before Seidel from discovering this? The main reason (which we already mentioned) was the prevalence of Euclidean methodology.

[1] Jourdain [1912], 2, p. 527.

[2] Rationalists doubt that there are methodological discoveries at all. They think that method is unchanging, eternal. Indeed methodological discoverers are very badly treated. Before their method is accepted it is treated like a cranky theory; after, it is treated as a trivial commonplace.

[3] Seidel [1847], p. 383.

The Cauchy revolution of rigour was motivated by a conscious attempt to apply Euclidean methodology to the Calculus.[1] He and his followers thought that this was how they could introduce light to dispel the 'tremendous obscurity of analysis'.[2] Cauchy proceeded in the spirit of Pascal's rules: he first set out to define the obscure terms of analysis – like limit, convergence, continuity, etc. – in the perfectly familiar terms of arithmetic, and then he went on to prove everything that had not previously been proved, or that was not perfectly obvious. Now in the Euclidean framework there is no point trying to prove what is false, so Cauchy had first to improve the extant body of mathematical conjectures by jettisoning the false rubbish. In order to improve the conjectures, he applied the method of looking out for exceptions and restricting the domain of validity of the original, rashly stated conjectures to a safe field, i.e. he applied the exception-barring method.[3]

A writer in the 1865 edition of the *Larousse* (probably Catalan) rather sarcastically characterised Cauchy's search for counterexamples. He wrote:

> He has introduced into science only negative doctrines...it is in fact almost always the negative aspect of the truth which he came to discover, that he takes care to make evident: if he had found some gold in whiting, he would have announced to the world that chalk is not *exclusively* formed of carbonate of lime.

A part of a letter which Abel wrote to Holmboë is further evidence of this new heartsearching mood of the Cauchy school:

> I have begun to examine the most important rules which (at present) we ordinarily sanction in this respect, and to show in which cases they are not proper. This goes well enough and interests me infinitely.[4]

What was considered by the rigourists to be hopeless rubbish, such as conjectures about sums of divergent series, was duly committed to the flames.[5] 'Divergent series are', wrote Abel, 'the work of the devil'. They only cause 'calamities and paradoxicalities'.[6]

But while constantly endeavouring to improve their conjectures by

[1] 'As for methods, I have had to give them all the rigour that one demands in geometry, so as never to resort to reasons drawn from the generality of algebra.' (Cauchy [1821], Introduction.)

[2] Abel [1826*a*], p. 263.

[3] 'To bring useful restrictions to too extended assertions.' (Cauchy, [1821].)

[4] Abel [1825], p. 258.

[5] Contemporaries certainly regarded this purge as 'a little harsh'. (Cauchy, [1821], Introduction.)

[6] Abel [1825], p. 257.

exception-barring, the idea of *improving* by proving never occurred to them. The two activities of guessing and proving are rigidly separated in the Euclidean tradition. The idea of a proof which deserves its name and still is not conclusive was alien to the rigourists. Counterexamples were regarded as grave and disastrous blemishes: they showed that a conjecture was wrong and that one had to start proving again from scratch.

This was understandable in view of the fact that in the eighteenth century pieces of shabby inductive reasoning were called proofs.[1] But there was no way of improving *these* 'proofs'. They were rightly scrapped as 'not rigorous proofs – that means, no proofs at all'.[2] *Inductive argument was fallible – therefore it was committed to the flames. Deductive argument took its place – because it was held to be infallible.* 'I make all uncertainty disappear', announced Cauchy.[3] It is against this background that the refutation of Cauchy's 'rigorously' proved theorem has to be appreciated. And this refutation was not an isolated case. Cauchy's rigorous proof of the Euler formula was, as we have seen, followed likewise by papers stating the well known 'exceptions'.

There were only two ways out: either to revise the whole infallibilist philosophy of mathematics underlying the Euclidean method, or somehow to hush up the problem. Let us first see what would be involved in revising the infallibilist approach. One would certainly have to give up the idea that all mathematics can be reduced to indubitably true trivialities, that there are statements about which our truth-intuition cannot possibly be mistaken. One had to give up the idea that our deductive, inferential intuition is infallible. Only these two admissions could open the way to the free development of the method of proofs and refutations and its application to the critical appraisal of deductive argument and to the problem of dealing with counterexamples.[4]

[1] The eighteenth-century 'formalism' was sheer inductivism. Cf. p. 133. Cauchy rejects in the Preface of his [1821] inductions which are only 'appropriate to sometimes present the truth'.

[2] Abel, [1826*a*], p. 263. For Cauchy and Abel 'rigorous' means deductive, as opposed to inductive.

[3] Cauchy [1821], Introduction.

[4*] *Editors' note:* This passage seems to us mistaken and we have no doubt that Lakatos, who came to have the highest regard for formal deductive logic, would himself have changed it. First order logic has arrived at a characterisation of the validity of an inference which (relative to a characterisation of the 'logical' terms of a language) does make valid inference essentially infallible. Thus, one need make only the first of the two admissions mentioned by Lakatos. By a sufficiently good 'proof analysis' all the doubt can be thrown onto the *axioms* (or antecedents of the theorem) leaving none on the *proof* itself. The method of proofs and refutations is by no means invalidated (as is suggested

As long as a counterexample was a blemish not only to a theorem but to the mathematician who advocated it, as long as there were only proofs or non-proofs, but no sound proofs with weak spots, mathematical criticism was barred. It was the infallibilist philosophical background of Euclidean method that bred the authoritarian traditional patterns in mathematics, that prevented publication and discussion of conjectures, that made impossible the rise of mathematical criticism. Literary criticism can exist because we can appreciate a poem without considering it to be perfect; mathematical or scientific criticism cannot exist while we only appreciate a mathematical or scientific result if it yields perfect truth. A proof is a proof only if it proves; and it either proves or it does not. The idea – expressed so clearly by Seidel – that a proof can be respectable without being flawless, was a revolutionary one in 1847, and, unfortunately, still sounds revolutionary today.

It is no coincidence that the discovery of the method of proofs and refutations occurred in the 1840s, when the breakdown of Newtonian optics (through the work of Fresnel in the 1810s and 1820s), and the discovery of non-Euclidean geometries (by Lobatschewsky in 1829 and Bolyai in 1832) shattered infallibilist conceit.[1]

in the text) by refusing to make the second of these admissions: indeed it may be by this method that proofs are improved so that all the assumptions that have to be made in order that the proof be valid, are made explicit.

[1] In the same decade Hegel's philosophy offered both a radical break with its infallibilist predecessors and a powerful start for a thoroughly novel approach to knowledge. (Hegel and Popper represent the only fallibilist traditions in modern philosophy, but even they both made the mistake of reserving a privileged infallible status for mathematics.) A passage from de Morgan shows the new fallibilist mood of the forties:

'A disposition sometimes appears to reject all that offers any difficulty, or does not give all its conclusions without any trouble in examination of apparent contradictions. If by this it be meant that nothing should be permanently used, and implicitly trusted, which is not true to the full extent of the assertion made, I, for one, should offer no opposition to so rational a course. But if it be implied that nothing should be produced to the student, with or without warning, which cannot be understood in all its generality, I should, with deference, protest against a restriction which would tend, in my opinion, not only to give false views of what is actually known, but to stop the progress of discovery. It is not true, out of geometry, that the mathematical sciences are, *in all their parts*, those models of finished accuracy which many suppose. The extreme boundaries of *analysis* have always been as imperfectly understood as the tract beyond the boundaries was absolutely unknown. But the way to enlarge the settled country has not been by keeping within it, [this remark is against the exception-barring method] but by making voyages of discovery, and I am perfectly convinced that the *student* should be exercised in this manner; that is, that he should be taught how to examine the boundary, as well as how to cultivate the interior. I have therefore never scrupled, in the latter part of the work, to use methods which I will not call doubtful, because they are presented as unfinished, and because the doubt is that of an expectant learner, not of an unsatisfied critic. Experience has often shown that the defective conclusion has been rendered

Before the discovery of the method of proofs and refutations the problem posed by the succession of counterexamples to a 'rigorously proved' theorem could be 'solved' only by the exception-barring method. *The proof proves the theorem, but it leaves the question open of what is the theorem's domain of validity. We can determine this domain by stating and carefully excluding the 'exceptions' (this euphemism is characteristic of the period). These exceptions are then written into the formulation of the theorem.*

The dominance of the exception-barring method shows how the Euclidean method can, in certain crucial problem situations, have deleterious effects on the development of mathematics. Most of these problem situations occur in growing mathematical theories, where growing concepts are the vehicles of progress, where the most exciting developments come from exploring the boundary regions of concepts, from stretching them, and from differentiating formerly undifferentiated concepts. In these growing theories intuition is inexperienced, it stumbles and errs. There is no theory which has not passed through such a period of growth; moreover, this period is the most exciting from the historical point of view and should be the most important from the teaching point of view. These periods cannot be properly understood without understanding the method of proofs and refutations, without adopting a fallibilist approach.

This is why Euclid has been the evil genius particularly for the history of mathematics and for the teaching of mathematics, both on the introductory and the creative levels.[1]

intelligible and rigorous by persevering thought, but who can give it to conclusions which are never allowed to come before him? The effect of exclusive attention to those parts of mathematics which offer no scope for the discussion of doubtful points is a distaste for modes of proceedings which are absolutely necessary to the extension of analysis. If the cultivation of the higher parts of mathematics were left to persons trained for the purpose, there might be some show of reason for keeping out of the ordinary student's reach, not only the unsettled, but even the purely speculative parts of the abstract sciences; reserving them for those persons whose business it would then be to render the former clear and the latter applicable. As it is, however, the few in this country who pay attention to any difficulty of mathematics for its own sake come to their pursuit through the casualties of taste or circumstances; and the number of such casualties should be increased by allowing all students whose capacity will let them read on the higher branches of applied mathematics, to have each his chance of being led to the cultivation of those parts of analysis on which rather depends its future progress than its present use in the sciences of matter.' (de Morgan [1842], p. vii).

[1] According to R. B. Braithwaite, 'the good genius of mathematics and of unselfconscious science, Euclid has been the evil genius of philosophy of science – and indeed of metaphysics'. (Braithwaite [1953], p. 353.) This statement, however, originates in a static logicist conception of mathematics.

Note: In this appendix the supplementary stages 5, 6, and 7 (cf. p. 128) of the method of proofs and refutations have not been discussed. I would just mention here that a methodical hunt for uniform convergence in other proofs (stage 5) would very quickly have yielded the refutation and improvement of another theorem proved by Cauchy: the theorem that the integral of the limit of any convergent series of continuous functions is the limit of the sequence of the integrals of the terms, or briefly, that in the case of series of continuous functions, the limit and the integral-operations can be interchanged. This had been uncontested throughout the eighteenth century, and even Gauss applied it without giving it a second thought. (See Gauss [1813], Knopp [1928] and Bell [1945].)

Now it did not occur to Seidel, who discovered uniform convergence in 1847, to look at other proofs to see if it had been implicitly assumed there. Stokes, who discovered uniform convergence in the same year – though not with the help of the method of proofs and refutations – uses in this same paper the false theorem about integration of series, referring to Moigno (Stokes [1848]). (Stokes made another mistake: he thought he had proved that uniform convergence was not only sufficient but necessary for the continuity of the limit function.)

This delay in discovering that the proof that the integration of series also depends on the assumption of uniform convergence may have been due to the fact that this primitive conjecture was refuted by a concrete counterexample only in 1875 (Darboux [1875]), by which date proof-analysis had already traced uniform convergence in the proof without the analysis being catalysed by a counterexample. The hunt for uniform convergence once fully under way with Weierstrass at its head soon discovered the concept in proofs concerning term by term differentiation, double limits, etc.

The *sixth stage* is to check the hitherto accepted consequences of the refuted primitive conjecture. Can we rescue these consequences, or does the refutation of the lemma lead to a disastrous holocaust? Term by term integration, for instance, was a cornerstone of the Dirichlet proof of Fourier's conjecture. Du Bois-Reymond describes the situation in dramatic terms: the theory of trigonometric series is 'cut to the heart', its two key theorems 'have had the ground cut from under them' and

with one blow the general theory was pushed back to the state in which it had been before Dirichlet, back even before Fourier.

(du Bois-Reymond [1875], p. 120.) It makes an intriguing study to see how the 'lost ground' has been regained.

In this process a spate of counterexamples was unearthed. But their study – the *seventh stage* of the method – started only in the last years of the century. (E.g. Young's work on the classification and distribution of points of non-uniform convergence; Young [1903–4].)