The Logic of Mathematical Discovery Vs. the Logical Structure of Mathematics

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1. Introduction

Mathematics offers us a puzzling contrast. On the one hand it is supposed to be the paradigm of certain and final knowledge: not fixed to be sure, but a steadily accumulating coherent body of truths obtained by successive deduction from the most evident truths. By the intricate combination and recombination of elementary steps one is led incontrovertibly from what is trivial and unremarkable to what can be non-trivial and surprising.

On the other hand, the actual development of mathematics reveals a history full of controversy, confusion and even error, marked by periodic reassessments and occasional upheavals. The mathematician at work relies on surprisingly vague intuitions and proceeds by fumbling fits and starts with all too frequent reversals. In this picture the actual historical and individual processes of mathematical discovery appear haphazard and illogical.

The first view is of course the currently conventional one which descends from the classic work of Euclid. Following Frege, Russell, and Hilbert it has in this century been given a theoretical formulation in terms of the logical analysis of the structure of mathematics. With formal systems as the principal technical object of study, this metamathematics has undergone extraordinarily intensive development.

There is also a more isolated tradition which undertakes to discern patterns in the actual dynamic progress of mathematical thought; it dates back to Pappus and can be traced through the writings of Descartes, Leibniz, and Bolzano. Most notable in our time have been the extensive studies by George Pólya of patterns of plausible reasoning in mathematical problem-solving and demonstration. Imre Lakatos

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has taken this as one point of departure for his "rational reconstruction" of the growth of mathematical knowledge in what he calls the logic of mathematical discovery or heuristic. Lakatos' view of mathematics is philosophically much more sweeping and radical than Pólya's. It is situated within a general account of all rationally gained knowledge which owes its debt to Karl Popper's (so-called) logic of scientific discovery. Lakatos rejects the Euclidean deductivist infallibilist view and replaces it by one of mathematics as a body of fallible knowledge being improved incessantly in response to ongoing critical assaults. To describe this he formulates what is supposed to be a kind of logic of proofs and refutations. At first this was directed by him at the detailed examination of particular problem-situations of both historical and mathematical interest. Later, he turned his attack on the search for "certain and final" foundations of mathematics within global formal systems.

Many of those who are interested in the practice, teaching, and/or history of mathematics will respond with eager sympathy to Lakatos' program. (One may add that it fits well with the increasingly critical and anti-authoritarian temper of these times.) Personally, I have found much to agree with both in his general approach and in his detailed analysis. Clearly, logic as it stands fails to give a direct account either of the historical growth of mathematics or the day-to-day experience of its practitioners. It is also clear that the search for ultimate foundations via formal systems has failed to arrive at any convincing conclusion. Nevertheless, the opinion I reach about Lakatos' own program is that it is far too single-minded and much more limited than he tries to make out. Speaking metaphorically, he plays only one tune on a single instrument--admittedly with a number of satisfying variations--where what is wanted is much greater melodic variety and the resources of a symphonic orchestra.

My plan here is to outline Lakatos' general views together with an indication of how they are elaborated in his case studies. The latter half of the paper is largely taken up with an extensive critique. This is followed by (i) a brief comparison with Pólya's work and (ii) a defense of logic as a means to analyze the underlying structure of mathematics. In conclusion I try to suggest directions for a possible rapprochement or synthesis of the opposing viewpoints. Fittingly, such would be in accord with Lakatos' own dialectical conception of the progress of human understanding.

2. L's Writings

There are two principal sources for Lakatos' writings on the nature of mathematics. One is his relatively well-known book Proofs and Refutations: the Logic of Mathematical Discovery [7][1]. The other consists of the first five essays in volume 2 of his Philosophical Papers [8]. Both of these appeared after his death in 1974, and count among them significant portions which had never been published because--true to his philosophical attitude--Lakatos was not completely satisfied

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with them. He had also planned to improve those parts which had been previously published, including the main body of [7]. Nevertheless, there are ideas and themes which are reiterated with sufficient frequency and emphasis that we can be confident that they were well established in his mind.

The body of [7] consists of a case study in the rational reconstruction of mathematical progress. It is a presentation in dialogue form of the amazingly tangled history--spanning most of the 19th century--surrounding the (Descartes-) Euler conjecture for polyhedra. This is a very simple equation (namely, $V - E + F = 2$) which expresses an invariable relationship between the number of vertices (V), edges (E), and faces (F) of any polyhedron. The choice of this example as an object of heuristic study (originally suggested to Lakatos by Pólya), has much to recommend it, besides its surprising ins-and-outs. For one thing, the concepts reach back to Greek geometry and, in the form established by Poincaré, bring us forward to the very doorsteps of modern combinatorial topology. In addition, the concepts involved are relatively elementary and the logic of the situation can be followed by anyone having a modicum of appreciation of mathematical proofs. The dialogue is often a delight to read and the entire presentation is a brilliantly sustained *tour de force*. Eventually, though, the relentless examination and re-examination of concepts, putative results, criticisms, and counter-examples is extremely fatiguing; one must be rather determined to see it through to the end, with little additional insight as reward.

I think one gets a clearer and quicker idea of Lakatos' general views and program by reading the two appendices to [7] and the first two essays of [8]. Though these contain illustrations from mathematics of a less elementary conceptual character than the Euler conjecture, they could hardly discourage anyone seriously interested in their subject matter.

3. A Summary of L's General Views

(In presenting the gist of these I shall move freely between [7] and [8], quoting liberally as well as paraphrasing.)

Modern mathematical philosophy is deeply embedded in general epistemology and is only to be understood with reference to its basic controversy: that is between the *dogmatists*--who claim that we can know--and the *skeptics*--who claim that we cannot know, or at least cannot know what it is that we know. The skeptical argument that it is hopeless to find foundations for knowledge is based on infinite regress both for meaning and for truth. Three major *rationalist* (dogmatist) enterprises have been developed to try to stop these twin infinite regresses: (1) the *Euclidean program*, (2) the *Empiricist program*, and (3) the *Inductive program*. The first of these is normally associated with mathematics, the latter two with scientific theories. Each organizes knowledge within (not necessarily formal)

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deductive systems. In a Euclidean system truth "flows downwards through the deductive channels" from the indubitably true axioms, while in an Empiricist system falsity "flows upwards" from those basic statements which turn out to be untrue. The Inductive program also attempts to find conditions for truth to flow upwards from the basic statements; this will not concern us further here.

None of the rationalist programs can withstand the criticism of the skeptics. However, there is a fourth program which can answer them, namely Popper's critical fallibilism. This takes infinite regress in proofs and concepts seriously and does not pretend to stop them. In a Popperian theory "we never know, we only guess." But guesses can be criticized and then improved. The old problems of reduction and justification of knowledge become pseudo-problems. Instead of asking How do you know? one asks How do you improve your guesses? There is now no concern if the skeptic complains that you cannot know the answer to that, since in fact your answer itself is only a guess. "There is nothing wrong with an infinite regress of guesses."

The Euclidean program for mathematics is hereby abandoned (indeed rejected), but mathematics can be regarded as a quasi-empirical theory under the new stance. By such is meant an empirical theory whose basic statements are not of a singular spatio-temporal character. For example, they may be elementary arithmetical statements or even entire bodies of already accepted informal statements about which one has developed some confidence. Individual mathematical conjectures and whole mathematical theories can be tested for their consequences among such basic statements and be modified or even rejected. A quasi-empirical theory is always conjectural, at best well-corroborated. As in empirical theories, the axioms are used to explain those basic statements which appear as consequences. Euclidean theories are rigid and anti-speculative; by contrast, the quasi-empirical approach is uninhibitedly speculative and advocates a proliferation of "bold, imaginative" hypotheses.

4. The Logic of Proofs and Refutations (Lakatos' ideas, cont'd.)

Instead of growing through the steady accumulation of indubitably established theorems, mathematics grows through the "incessant improvement of guesses by speculation and criticism," by the logic of proofs and refutations. While this is a very general pattern of mathematical discovery, it was itself only discovered in the 1840's. Naive conjectures and concepts must pass through the crucible of proofs and refutations. The results are improved conjectures (theorems) and improved (proof-generated or theoretical) concepts. The logic of mathematical discovery "is neither psychology nor logic, it is an independent discipline" also called heuristic. While mathematics is a product of human activity, it acquires a certain autonomy with its own laws of growth, its own dialectic. This is the subject of heuristic, which it studies through history and the rational reconstruction of history.

The method of proofs and refutations is also called proof-analysis. Its skeleton is as follows ([7], pp. 127-128): There is

(1) a primitive conjecture and

(2) an informal proof.

The latter is a thought-experiment or argument which decomposes the primitive conjecture into subconjectures or lemmas. Subsequently

(3) "global" counterexamples emerge,

i.e., counterexamples to the primitive conjecture. Finally,

(4) the proof is re-examined for a hidden lemma to which the global counterexample is a local counterexample. This is built into the improved conjecture (theorem); its principal new feature is the proof-generated concept.

The method frequently has further ramifications. The lemma may be hidden in other proofs and the new concept may be used to improve them; counterexamples open up into new fields of inquiry, and so on.

The method of proof-analysis might not improve a proof. This only happens when the analysis turns up unexpected aspects of the naive conjecture. That might not be the case in mature theories but it is always the case in young, growing theories.

It should also be noted that other strategies than proof-analysis have been used historically to deal with the problems presented by counterexamples. These are the methods of monster barring and exception barring. The former attempts to restrict concepts involved specifically to exclude pathological cases. In the second, one searches for a "safe" domain of objects for which the conjecture is valid, without seeking the most general domain of validity.

5. Getting Down to Cases

There are only two cases that Lakatos presents in any detail to support his thesis, though features of a number of other cases are taken up too. These are the Euler conjecture which, as has already been mentioned, is treated at length in the main body of [7] and Cauchy's theorem on limits of series of continuous functions ([7], App. 1). Actually, the latter serves to illustrate the method of proof-analysis in a somewhat clearer way than the former, so we look at it here.

The external history of this theorem runs briefly as follows. Cauchy took the first successful steps to give a rigorous foundation for the calculus without using the troublesome concept of infinitesimal. These became well-known through publication of his book Cours d'Analyse in 1821. (Actually, much the same achievement had been made by Bolzano several years earlier, but his 1817 publication seems to have received no attention in the mathematical world at that time.[2]) The first concepts which needed re-examination were those of <u>limit</u> and <u>continuous function</u>, for which Cauchy provided new definitions. These are not as precise as the 'ε,δ' definitions we use today and which we owe instead to Weierstrass (around mid-century). Indeed, there is a kind of ambiguity about them which is disturbing by today's standards of rigor.

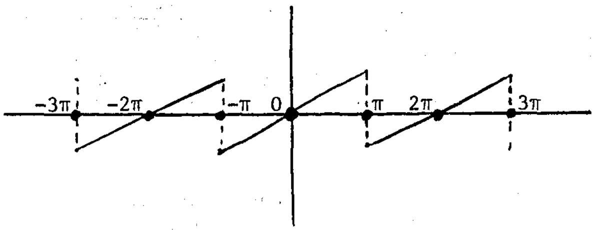
Cauchy stated and presented an argument for the following theorem: <u>Suppose</u> $\Sigma_{n=1}^{\infty} f_n(x)$ <u>converges to</u> $f(x)$ <u>for each</u> x <u>and that each</u> $f_n(x)$ <u>is continuous; then</u> $f(x)$ <u>is continuous.</u>
The hypothesis is expressed in terms of limits by

$$\lim_{n \to \infty} s_n(x) = f(x) \quad \text{where} \quad s_n(x) = \Sigma_{m=1}^{n} f_m(x) \ .$$

According to our present-day interpretation of limits and continuity this theorem is false and there are many simple counterexamples. The curious part of this history is that a series which could serve as a counterexample was already known to Cauchy and not recognized as such. This was

$$\sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \frac{1}{4}\sin 4x + \ldots \left(\text{or } \Sigma_{n=1}^{\infty} \frac{(-1)^{n+1}}{n} \sin nx \right)$$

which appeared in the famous 1807 memoir by Fourier on the propagation of heat. It was shown there to converge to a function with the broken straight line graph:



(At the points $n\pi$ the series converges to 0 .)

There had been much controversy over Fourier's memoir and his assertion about the representability of "arbitrary" functions by trigonometric series. It seems that Cauchy's theorem, which would hardly have been considered worth stating before, was designed to put Fourier's work definitively into question (this is the view of Grattan-Guinness [3], p. 78). Be that as it may, no one protested the theorem until 1826, when Abel pointed out that there were "exceptions" such as the series above. But instead of examining Cauchy's putative proof to see where it broke down, Abel took the exception-barring route: he restricted attention to the "safe" domain of power-series functions, for

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which he obtained definitive convergence results. It was not until 1847 that Seidel (a student of Dirichlet's) re-examined Cauchy's argument and found the "hidden" lemma which makes the conclusion correct. This is analyzed by Lakatos as follows, writing out the concepts involved in modern terms. We assume:

(1) (convergence) for each x and $\varepsilon > 0$ there exists N such that $|f(x) - s_n(x)| < \varepsilon$ for all $n \geq N$, and

(2) (continuity of each f_n, hence of each s_n) for each x and $\varepsilon > 0$ there exists $\delta > 0$ such that $|y-x| < \delta$ implies $|s_n(y) - s_n(x)| < \varepsilon$.

The desired conclusion is

(3) (continuity of f) for each x and $\varepsilon > 0$ there exists $\delta > 0$ such that $|y-x| < \delta$ implies $|f(y) - f(x)| < \varepsilon$.

The proof idea is to relate $|f(y) - f(x)|$ to $|s_n(y) - s_n(x)|$ for suitably large n. By the "triangle inequality" we have

$$|f(y) - f(x)| \leq |f(y) - s_n(y)| + |s_n(y) - s_n(x)| + |s_n(x) - f(x)| .$$

Thus $|f(y) - f(x)| < \varepsilon$ if each of $|f(y) - s_n(y)|$, $|s_n(y) - s_n(x)|$ and $|f(x) - s_n(x)|$ is less than $\varepsilon/3$. For any n the second can be arranged using (2). By (1) we can choose N such that $|f(x) - s_n(x)| < \varepsilon/3$ for $n \geq N$ and for any y we can choose N_1 such that $|f(y) - s_n(y)| < \varepsilon/3$ for $n \geq N_1$. But while x in (3) is conceived to be fixed, y must be variable, and the N_1 associated with y may vary too. What is needed to carry through the proof is a uniform choice of N independent of x (hence applicable to any y) in (1):

(1)' (uniform convergence) for each $\varepsilon > 0$ there exists N such that $|f(x) - s_n(x)| < \varepsilon$ for all x and all $n \geq N$.

This assumption was hidden in Cauchy's argument. The improved theorem is that (1)' and (2) implies (3). The Fourier series which was a counter-example to Cauchy's formulation of the theorem now becomes a counter-example to the "guilty lemma" that $\Sigma_{n=1}^{\infty} f_n(x)$ converges uniformly to $f(x)$ if it converges: indeed, it converges but not uniformly. The proof-generated concept of uniform convergence is incorporated as the principal feature of the improved theorem.

Lakatos credits the method of proof-analysis to Seidel. He says that "Seidel discovered the proof-generated concept of uniform convergence and the method of proof-analysis at one blow. He was fully conscious of his methodological discovery which he stated with great clarity."[3,4]

There is no time here to go into the treatment of the Descartes-Euler conjecture in [7]. It should be remarked that a difficulty with the dialogue form of presentation used there is that one is never sure which of the participants' views are shared by Lakatos.[5] Fortunately, Lakatos has provided many (scholarly) footnotes which parallel the text as well as suppplementary comments.

6. A Critical Examination of Lakatos' Views and Program

The details of this and other examples spelled out (or indicated) in [7] would seem to show that one must take Lakatos' analysis of mathematical progress rather seriously. Nevertheless, I have a number of questions to raise and criticisms to make.[6] These will concern both what Lakatos tells us and matters about which he says nothing at all.

(i) What happened before 1847? According to the quotation given just a moment ago, the method of proof analysis appears to be a relative late-comer in the history of mathematics (1847). But this is said to be the same as the method of proofs and refutations, which is the only theoretical pattern offered by Lakatos to account for the progress of mathematics. It would seem then that Lakatos has nothing to tell us about the growth of mathematics prior to 1847. Actually, he has various things to say: for example, the methods of monster-barring and exception-barring were practiced before that date as moves to respond to criticism. Shouldn't a logic which is supposed to account for changes in a fallible body of knowledge account for any significant kinds of changes? A related question is whether the method of proofs and refutations is supposed to be descriptive or normative. It seems at best that it could be descriptive of progress since 1847. But much of the tenor of the discussion leads one to view it as normative.

(ii) Is the method most appropriate to describe mathematics in transitional foundational periods? The example from Cauchy's rigorization of analysis would seem to suggest that; witness also the statement that the method is more appropriate to young, growing theories. But the example of Euler's conjecture is not of this character, though it turned out that the concepts were less clear than one had imagined. On the other hand, there were a number of foundational moves which took place without response to specific criticism or counter-examples, e.g., those establishing the use of imaginary numbers or points at infinity (in projective geometry) or continuous probability measures. Finally, the method tells us nothing about progress by internal organizational foundational moves. These proceed by finding suitable abstract concepts around which to wind large parts of the subject in an understandable way. They do not arise as responses to critical examination of fallacious proofs. Examples are: linear algebra, linear analysis, point-set topology, group theory, etc.

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(iii) How does this "logic of mathematical discovery" relate to working experience? Most mathematicians throughout the history of theoretical mathematics work at a safe distance from troublesome foundational questions. This is not to say that the concepts used at any given time are all clearly understood (e.g., the nature of geometrical objects, infinitesimals, imaginary numbers, sets, etc.). Rather, the mathematician is usually engaged in a project "mid-stream" which seems hardly affected by foundational considerations. That project usually consists in developing conjectures and seeking proofs of those conjectures. The tests for whether one has succeeded in obtaining such a proof are informal but fairly decisive.[7] It is common experience that proof-attempts proceed by fits and starts and involve reversals; (self-) critical examination is an essential element, but this does not necessarily mean that counter-examples form their principal feature.

(iv) Is there no end to guessing? Again what Lakatos suggests here does not square with ordinary experience. The professional mathematician knows rather well what sort of thing will work for certain kinds of problems and what won't. So guesswork is minimized from the outset. Moreover, the guesswork finishes with the mathematician's successful struggle to solve a problem or complete a proof. It is true that results are viewed in changing perspective over historical periods. Their significance is reassessed, they are generalized and understood in wider settings. (A marvelous example is provided by Pythagoras' Theorem.) But this is quite a different picture from that given by Lakatos of endless guesswork.

(v) What constitutes improvement in a proof? Lakatos gives no theoretical criterion for this. He merely produces examples and shows the change which takes place in the situation in response to criticism and/or counter-examples. Evidently--both for him and for us--improvement has taken place. It is my contention, which I shall elaborate below, that in fact we have informal criteria for what constitutes an adequate proof and that these criteria can be explained in logical terms; improvement is described in the passage from inadequate proofs to adequate ones. It seems to me that Lakatos must implicitly accept this, or something like it. I believe further that he refuses to say anything explicitly in this direction since doing so would undermine his sweeping rejection of the deductivist account of mathematics. In connection with both this and the preceding point, recall Lakatos' statement that not all proofs can be improved, especially not those in mature theories. In other words, one reaches resting points where there comes an end to improvement of proofs under criticism. Of course there is always the possibility of improvement of results by generalization, which is quite a different matter (as just described above).

(vi) What constitutes an initial proof? Where does it come from? Lakatos tells us that this is a thought-experiment or naive proof-idea. But in the examples he gives us the idea for the proof is already well-advanced. It has significant structure and steps; it pretends to be a rational chain from hypotheses to conclusion. We cannot imagine that such a proof springs full blown, following formulation of a conjecture. Should not a heuristic theory account for the development of such a proof? Indeed, Pólya--but not Lakatos--has significant things to say here (cf., Section 7 below.)[8]

(vii) What is the form of conjectures? All the examples of conjectures given by Lakatos take the form

(1) for all objects satisfying given hypotheses A, a conclusion B holds,

which is symbolized logically by

(2) $$\forall x[A(x) \rightarrow B(x)]\ .$$[9]

But there are a number of other forms of statements of mathematical (and historical) interest. For example, there are singular statements, such as $e^{i\pi} = -1$, or $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \ldots = \frac{\pi}{4}$, or that 641 divides $2^{2^5} + 1$. Of course such statements have logical structure when the concepts involved are analyzed, and there is the theoretical possibility of "infinite regress" in such an analysis. But we are interested here in a description of the naive form of a statement, i.e., as it presents itself to the working mathematician. There are also existential statements

(3) $$\exists x\ A(x)$$

to consider, for example that the 17-sided regular polygon is constructible by ruler and compass, or that there exists a decision method for the elementary theory of real numbers, or that there exists an equation of degree 5 with rational coefficients which is not solvable by radicals. In refinement of (2), one is very often concerned with statements of the form:

(4) $$\forall x[A(x) \rightarrow \exists y\ B(x,y)]\ ,$$

for example, that every complex polynomial of degree > 0 has at least one complex root, or that every Jordan curve in space has a minimal spanning surface. The statement that there exist infinitely many prime numbers and the conjecture that there exist infinitely many twin primes are actually both of this form (for every integer n there exists a larger prime, resp., twin prime) though usually here we think of a representation

(5) $$\forall n\ \exists m\ B(n,m)$$

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where the variables 'n', 'm' range over the non-negative integers. As an example of increasing complexity in the same direction we have the statement of Waring's conjecture:

$$\forall k\ \exists n\ \forall m\ \exists q_1 \ldots \exists q_n\ [m = q_1^k + \ldots + q_n^k] .$$

Finally, there are interesting statements of the form

(6) $$\forall x\ A(x) \rightarrow \forall x\ B(x) ,$$

for example, the statement Con(ZF) → Con(ZF + AC + GCH) which says that if the system ZF of Zermelo-Fraenkel set theory is consistent, i.e., if there is no proof of a contradiction within it, then there is no contradiction to be obtained when we adjoin the axiom of choice and the generalized continuum hypothesis. Now one can expect that methods of attack on a conjecture will be sensitive to the (naïve or logical) form of the conjecture. We should be suspicious of a supposed logic of mathematical discovery which only concerns itself with statements of the form (2).

(viii) Can ordinary logical analysis account for the same examples as the method of proofs and refutations? I believe it can, somewhat as follows. The primitive conjectures considered by Lakatos as we have seen take the form

(1) $$\forall x\ [A(x) \rightarrow B(x)] .$$

The structure of the informal proof is supposed to decompose (1) into a series of subconjectures or lemmas, i.e.,

(2) $$\forall x\ [A(x) \rightarrow A_i(x)] \qquad (i = 1,\ldots,n)$$

where

(3) $$\forall x\ [A_1(x) \wedge \ldots \wedge A_n(x) \rightarrow B(x)]$$

is supposed to hold. There can be various kinds of troubles, including the following two. First, in (2) we may not be clear enough about the concepts involved in $A(x)$ to be sure that the lemmas indeed follow. This is the first issue which is raised concerning Euler's conjecture ([7], p. 8). Secondly, we may be pretty clear about the concepts involved and (2) holds but (3) may not be logically valid. This is the case where we look for a "hidden lemma", i.e., a property $A_{n+1}(x)$ such that

(3)' $$\forall x\ [A_1(x) \wedge \ldots \wedge A_n(x) \wedge A_{n+1}(x) \rightarrow B(x)]$$

is valid. But now the lemmas have to be re-examined, because we do not necessarily have

(2)' $$\forall x\ [A(x) \rightarrow A_{n+1}(x)] .$$

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Indeed, in the situation contemplated by the method there is a global counter-example c , i.e., one such that A(c) holds but not B(c) . Then (2), (2)', and (3)' are of course logically impossible. However, in this case we seek an "improvement" of the conjecture

(1)* $$\forall x\ [A^*(x) \rightarrow B(x)]$$

for which

(2)* $$\forall x\ [A^*(x) \rightarrow A_i(x)] \qquad (i = 1,\ldots,n+1)$$

now holds, as well as (3)'. This may be done by "incorporating" the hidden lemma into the hypothesis (most simply by taking $A^*(x) = A(x) \wedge A_{n+1}(x)$).

(ix) Are there no crystal-clear concepts? Certainly there have been continual historical shifts in what has been regarded as clearly understood. Throughout, though, the structure of positive integers 1,2,3,... has enjoyed a privileged status. To my mind, this is a crystal-clear mathematical concept. At any rate, if anything is a candidate for being such, this is it. Moreover, there has never been voiced any real concern or confusion on this score in the entire history of number theory (which stretches back to Euclid).[10] At no time has the criticism of proofs involved criticism of basic concepts about numbers. A heuristic logic should give some account of progress here. The fact that this subject is ignored by Lakatos is a sign that it threatens his theses, in particular that there are no crystal-clear concepts. (Note that this is a separate issue from whether there is such a thing as conceptual finality. For example, the concept of natural number is often defined these days in terms of the notion of set, thereby reducing a completely clear concept to one that is quite unclear. Of course, in the light of such moves one can always claim there is infinite regress.)

To complete this critique, we ask finally:

(x) What is distinctive about mathematics? Lakatos makes no effort to tell us what there is about the conceptual content of mathematics or about its verification structure which sets it off from other areas of knowledge. Obviously he has informal criteria, since he chooses to discuss only examples from mathematics. But he offers no theoretical criteria. It seems to me that there is nothing he says about the general idea of "proof" which could not apply equally well to "more or less convincing argument"; there is nothing about the "logic of mathematical discovery" which could not be read equally well as a "logic of rational discovery", i.e., of the process of reaching convictions rationally. If I am right, then such a logic could hope to account only for a few gross features of the actual growth of mathematics. In any case, all of my preceding comments (i) - (ix) reveal that Lakatos' "logic" hardly begins to be equal to the tasks called for in his grand program.

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7. Comparison with Pólya's Work

Pólya has written extensively on heuristic and plausible reasoning in mathematics ([9] - [11]). In the context of the present discussion, I would characterize this work of my esteemed colleague briefly as follows (cf., also footnote 5).

(i) Pólya does not voice philosophical doubts about the certainty of mathematics; he does not raise foundational issues. The concepts and problems with which he deals are supposed to be clearly understood. Moreover, we are supposed to understand what constitutes a demonstration; it is accepted that logic gives a theoretical explanation of that.

(ii) In [9] and [11] he concentrates on tactics and methods for finding solutions to problems and, to a lesser extent, on finding proofs of theorems. Pólya's motivation here is more toward helping people make their way effectively through mathematics than to establish a theory of heuristic. But in the process he develops well-structured sets of strategic rules.

(iii) In [10] Pólya concentrates on the processes which lead one to formulate general conjectures and to see what counts as support for them. In this connection he formulates a logic of plausible reasoning (or degrees of credibility); this includes a number of simple rules, of which the following is typical:

$$\frac{\begin{array}{c}A \text{ implies } B\\ B \text{ true}\end{array}}{A \text{ more credible}\ .}$$

(iv) Pólya makes use of a wealth of mathematically and/or historically interesting examples to illustrate his points and rules.[11]

Anybody who has read Pólya's works or heard him lecture knows that he is peerless within the framework for which he has set his heuristic. In contrast to Lakatos, he plumbs the relatively safe mid-stream of mathematics. But this is where most of the day-to-day experience of the subject is going on. Students and teachers could ask for nothing more. What professional mathematicians might want, though, is a continuation along the same lines which concentrated on the ins and outs of finding difficult proofs. That work is waiting to be carried on.

There is one aspect of mathematical progress which neither Pólya nor Lakatos have really attempted to deal with, namely that by convenient conceptual development. How does one go about finding the technical but general concepts that help organize masses of material and make difficult proofs understandable? (Cf., 6 (ii) above.) Lakatos' idea of proof-generated concepts seems to me a first step in this direction.

8. The Logical Analysis of Mathematics

There is little time here to mount a defense of the logical or metamathematical approach, so I shall simply try to indicate the nature of the position briefly (at least as I see it).

(i) Logic attempts to provide us with a theoretical analysis of the underlying nature of mathematics as physics provides us with a theoretic analysis of the underlying nature of the physical world. Evidently, in both cases, only a part of the experience is accounted for and, in particular, various superficial and/or accidental features cannot be treated at all.

(ii) In the case of logic, this theoretical analysis is supposed to explain what constitutes the underlying content of mathematics and what is its organizational and verificational structure.

(iii) The study of content has received no final answer. There are a number of conflicting positions about the nature of mathematics: Platonist, constructivist, finitist, predicativist among them. However, what logic has succeeded in doing very well is formulating these positions in precise terms by a variety of formal systems. It has then gone on to give us significant information about the potentialities and limitations of each of these positions and about their interrelationships. This part of logical achievement has been particularly stressed by Kreisel (cf., [6] among other of his publications).

(iv) The logical analysis of the structure of mathematics has been especially successful. Again, there is not a single analysis, since (for example) ordinary (Platonistic) reasoning uses classical two-valued logic while constructive reasoning uses a more restricted ("intuitionistic") logic. There are two parts to this logical analysis. First is the logical syntax of language which gives a description of the structure of mathematical propositions. This accords very well with our informal experience: transforming mathematical statements from informal to logical form and back is a direct matter which is essentially unproblematic. (This is in contrast of course with attempts to provide a logical syntax of ordinary language.) Second comes the logical structure of proofs as described in certain deductive systems. In this case the relationship with ordinary experience is more or less good: "less" for Hilbert-style systems and "more" for Gentzen-style systems of natural deduction. It is commonly felt that logic gives us a good underlying analysis of the structure of completed proofs (no gaps, no unsure assumptions or steps). Indeed, I believe that the logical analysis of the structure of mathematics comes much closer to explaining our everyday mathematical experience than physics does to explaining our everyday physical experience. (For an elaboration of these views, cf., my paper [2].)

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(v) Though formal systems are normally conceived to represent "slices" of mathematics in a "frozen" state, so to speak _in vitro_ as opposed to _in vivo_, one can use these systems to model growth and change. First efforts to do so were via _progressions of theories_;[12] but these took on an unreal character when extended into the transfinite. A formalization of predicative mathematics by a _growing system_ without use of transfinite progressions has been proposed in my paper [1]. This allows one to expand one's conceptual stock as more and more things are proved which make such extensions admissible.

9. Conclusion

Lakatos' fireworks briefly illuminate limited portions of mathematics conceived as an active growing intellectual endeavor which is subject to confusion, uncertainty, and error. In contrast logic gives us a coherent picture of mathematics but which at first sight appears ideal and static and which is irrelevant to everyday experience. However, it alone throws light on what is distinctive about mathematics, its concepts and methods. Pólya's heuristic provides one bridge from theory to practice. I believe that Lakatos' successes should inspire us to seek a more realistic theory of mathematics. But his failures and limitations should make us aware that much more from logic will have to be recognized as basic and incorporated into such a theory. It would be best to reserve the name "the logic of mathematical discovery" for that which is yet to come.

Notes

[1]This is not as well-known among mathematicians as it ought to be. Recently Hersh [4] has engaged in bringing Lakatos' ideas, which he largely favors, to the attention of the mathematical community.

[2]It is the thesis of Grattan-Guinness [3] that Cauchy somehow got his ideas from Bolzano without acknowledging them, but this has been disputed. Actually, Bolzano was a bit clearer than Cauchy about basic concepts. His revolutionary work (including anticipations of set theory) was not widely publicized and appreciated until the 1870's.

[3]The concept of uniform convergence and its need for rigorous proofs of certain basic theorems of analysis (such as concern also interchange of integration and limits) was independently found by the physicist Stokes (cf., [3]).

[4]Lakatos continued to be puzzled by Cauchy's failure to acknowledge a difficulty when confronted with the counter-examples. He later wrote a revisionist "history" of Cauchy's misadventure in "Cauchy and the Continuum," which appears as the third essay of [8]. In this Lakatos seized on A. Robinson's theory of infinitesimals [12] to

propose another interpretation: namely, Cauchy was a Leibnizean at heart and still clung to actual infinitesimals. Furthermore, his theorem is correct when read as a statement in Robinson's non-standard analysis.

The following points should be made about this:

(i) Cauchy defined variables as quantities which "one considers as having to successively assume many values different from one another." For limits he says that "when the successive values attributed to a variable approach indefinitely a fixed value so as to end by differing from it as little as one wishes, this last is called the limit of all the others." Then infinitesimals are said to be variables whose "numerical value decreases indefinitely in such a way as to converge to the limit 0." (cf., Kline [5], pp. 950-951).

(ii) In particular, Cauchy defined differentials dx (one principal form in which infinitesimals had previously made their appearance) as any finite variable quantity. Then, given a functional relationship $y = f(x)$, one defines dy to be $f'(x)\,dx$. This "saves" the equation $dy/dx = f'(x)$, in which $f'(x)$ has been defined independently in terms of limits by $\lim_{h \to 0} [f(x+h)-f(x)]/h$. Infinitesimals are thus treated here as a suggestive notational convenience.

(iii) Nevertheless, Cauchy's position on infinitesimals seems to be equivocal, and it may be said that he continued to "practice infinitesimalism" (Grattan-Guinness [3], pp. 57 ff.).

(iv) Robinson provided the first coherent theory of actual infinitesimals in which a Leibnizean-style calculus could be interpreted. However, this reconstruction involves the use of logical concepts (such as the distinction between internal and external properties in certain formal languages) which are foreign to infinitesimal analysis as it had been practiced.

(v) Robinson himself examined interpretations of earlier statements involving infinitely small and large quantities in analysis and considered possible reinterpretations of them in his system, in particular those given by Cauchy (Robinson [12], §10.5). Among these is the statement about convergence of series of functions $f_n(x)$ which we considered in the text. In his interpretation, Robinson allows the subscript n to take on infinite values, but considers x to range only over standard real numbers. He found that additional assumptions, such as uniform convergence, are still needed to obtain a correct theorem. By contrast, Lakatos (in the essay mentioned) assumes in his interpretation that x ranges over the full extended real number system (comprising infinitesimals) as well. For this alternative form, he verifies Cauchy's theorem to be correct "as it stands."

(vi) My view is that one can hardly credit Cauchy (or his predecessors) with having a coherent use of infinitely small and large quantities which merely awaited a Robinsonian-style foundation to legitimize it. In his theory of infinitesimals, Cauchy looks forward to the current

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standard methods for their elimination, while in his practice he slips backwards. The type of "rational reconstruction of history" revealed at length by this example seems to me to provide a good illustration of the dangers of Lakatos' free-wheeling "bold, imaginative" approach.

[5]According to Hersh [4] the one criticism Pólya made of Lakatos' treatment of the Euler conjecture was that it is "too witty." But Pólya added the following in a recent conversation which I had with him. In his view, Lakatos' method of proofs and refutations simply boils down to the alternating procedure (going back to Pólya and Szegö in 1925) and described in [11], vol. 2, pp. 50-51, from which I quote the following portions:

> A problem to prove is concerned with a clearly stated assertion A of which we do not know whether it is true or false: we are in a state of doubt. The aim of the problem is to remove this doubt, to prove A or to disprove it. ... If we cannot prove the proposed assertion A we try to prove instead a weaker proposition (which we have more chances to prove). And, if we cannot disprove the proposed assertion we try to disprove a stronger proposition (which we have more chances to disprove). ... In this way, by working alternately on proofs and counter-examples, we may attain a fuller knowledge of the facts.

No doubt Lakatos would have quarrelled with this and in particular with the assumption that A is clearly stated; however, much of the Lakatosian dialectic is accounted for in Pólya's alternating procedure.

[6]I must confess to being ignorant of the critical literature on Lakatos' work except for an excellent over-all essay review of the two volumes of his Philosophical Papers by my colleague Ian Hacking, which I have read in draft form. One specific critical question concerning his mathematical philosophy is raised by Hersh [4], which is otherwise extremely favorable (the same question is posed here in (x) p. 320).

[7]Even Euler, that most inductivist of mathematicians, operating at a time of low rigor, knew when he had a proof and when he didn't: "This law, which I shall explain in a moment, is, in my opinion so much more remarkable as it is of such a nature that we can be assured of its truth without giving it a perfect demonstration. Nevertheless, I shall present such evidence for it as might be regarded as almost equivalent to a rigorous demonstration."(Euler, quoted by Pólya in [10], vol. 1, p. 91).

[8]Lakatos tells us that Pólya is concerned with the heuristic leading to conjectures and that he takes up where Pólya leaves off. This is false on two counts: Pólya does concern himself as well with the heuristics of finding proofs (cf., Section 7 of this paper) while Lakatos

says nothing about the big stretch from conjecture to first real proof-ideas.

[9]We use here and below the logical symbols '$\wedge$' , '$\rightarrow$' , '$\forall x$' , '$\exists x$' for 'and', 'implies', 'for all x ', and 'there exists x ', respectively.

[10]Formalism has bred some skeptics who refuse to be convinced that elementary number theory is consistent. There is even an occasional loner who claims to have established its inconsistency; in these cases critical examination by others has only hardened their position (i.e., there is no dialectic taking place).

[11]R. Parikh asked whether Pólya says anything about the rule in (iii) above applied to the case that $A = B \wedge C$ and C is false. To my knowledge he does not. Naturally, this sort of example doesn't arise in practice. It also suggests that there are concepts implicit in the actual situation (knowledge changing over time, relevance of statements) which may need to be made explicit in such a logic in order to give it significance.

[12]By Turing, Kreisel, and myself.

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