REVIEW ARTICLE

LAKATOS'S PHILOSOPHY OF MATHEMATICS*

1. In November of 1956, at the age of 31, Imre Lakatos left Hungary, going first to Vienna and then to Kings College, Cambridge, as a Ph.D. student. For his thesis he chose the topic of mathematical discovery and, at the suggestion of the Hungarian mathematician George Polyá, he based his research on an historical case study; the development of a conjecture, due to Euler, about polyhedra. This part of the thesis is in the form of a dialogue, while other chapters, concerning the ancient method of analysis-synthesis and developments in 19th Century analysis are given in conventional, expository form. The thesis was completed in 1959,[1] and later the first two chapters (concerning Euler's conjecture) were expanded into a series of articles.[2] After Lakatos's death in 1974 these articles, together with another two chapters from the thesis, were published in book form.

In this paper I shall examine some of the main theses of *Proofs and Refutations*,[3] and I shall try to put the views expressed there into the context of Lakatos's other writings, both on the philosophy of mathematics and the philosophy of science.

2. Lakatos was not interested in the sorts of problems in the philosophy of mathematics which concern many contemporary philosophers, such as questions about mathematical ontology (e.g. What are numbers?), or the relation between mathematics and set theory (Can mathematics be founded on set theory?). Rather his concern was with the growth of mathematical knowledge, and in particular with the way in which mathematics grows from informal conjectures and heuristic proofs into formalized or semi-formalized theories. He rejects the idea of foundations for mathematical knowledge and holds that it is

Synthese 42 (1979) 335–351. 0039–7857/79/0422–0335 $01.70.

just as fallible as our knowledge of the external world. In mathematics we do not know, we can only guess, but some of our guesses can be better, more fruitful, than others.[4]

What, then, is the role of mathematical proof, for surely it is proofs which distinguish mathematical knowledge from other kinds of knowledge? It is usual to think of mathematical proofs as devices for establishing mathematical results. The central thesis of Lakatos's philosophy of mathematics is that proofs can play an important heuristic role in mathematics *quite distinct* from their role in establishing the logical connections between the statements of a theory. According to Lakatos, we can use proofs to improve our mathematical guesses.

The problem with which Lakatos deals can be put in the following way. Suppose we have a mathematical conjecture according to which all objects of a certain kind (e.g. natural numbers, continuous functions, polyhedra) have a certain property. How can we decide whether to accept the conjecture or not? If we think that the conjecture is true we can try to prove it, that is, derive it from accepted premisses. If we suspect that it is false we can try to derive a counterexample to it. The important point here is that if we are working within an informal, growing area of mathematics there will be difficulties about doing either of these two things. First, we do not possess a well specified set of axioms from which to invent the conjecture; instead we have to look for premisses *ad hoc*. Secondly, we do not have a well defined source of counterexamples; it will sometimes be possible to say that a *prima facie* counterexample is not really a counterexample, because it is not the right kind of object, the class of objects under scrutiny lacking any precise characterization. (In the 19th Century it was an important question whether certain very badly behaved 'functions' were really functions at all). How then should we proceed? Lakatos describes a number of sucessively more sophisticated methods by which we can respond to the conflict between proof, conjecture and counterexample.

A word first about how 'proof' is being used in this context. Lakatos describes a proof within informal mathematics as a '*thought experiment which suggests a decomposition of the original conjecture* *into subconjectures or lemmas*, thus *embedding it* in a possibly quite distant body of knowledge'.[5] The idea is this: we present certain conditions which, we suppose, hold of the object under consideration. We guess that these conditions jointly guarantee the presence of the condition stated in the original conjecture. Thus instead of one conjecture, we now have several; the original conjecture, the more or less conjectural premisses, and the meta-conjecture that the premisses imply the conclusion. None of these conjectures may survive criticism, but Lakatos is willing to call what we now have an 'informal proof'. As he says elsewhere, such an informal proof should not be confused with what is often called an 'informal proof' nowadays, which is simply an incompletely stated *formal* proof:

To call this sort of proof an informal proof is a misnomer and a misleading one. It may perhaps be a quasi-formal proof, or a 'formal proof with gaps', but to suggest that an informal proof is just an incomplete formal proof seems to me to be to make the same mistake as early educationalists did when, assuming that a child was merely a miniature grown up; they neglected the direct study of child behaviour in favour of theorizing based on simple analogy with adult behaviour.[6]

Given Lakatos's characterization of an informal proof it is clearly possible to 'prove' a conjecture, and yet for there to be a counterexample to it. It may be that one or more of the premisses is false, or that the premisses do not, in fact, imply the conjecture. In the present scheme of things, counterexamples can be of three kinds. (1) Counterexamples to the conjecture which are not also counterexamples to any of the premisses ('global but not local counterexamples', pp. 42ff); (2) counterexamples to one of the premisses which are not counterexamples to the conjecture ('local but not global counterexamples', pp. 10ff); (3) counterexamples to both the conjecture and to the premisses ('local and global counterexamples', p. 29). Let us leave (3) aside for the moment and concentrate on the other two.

If the counterexample is local but not global it refutes one of the premisses, say P_i, which must be replaced by a new premiss P_i', which is not subject to the same counterexample, and which (apparently) does as much to establish the conjecture as P_i did. If the counterexample is global but not local, it refutes the conjecture without refuting any of the explicitly stated premisses. In that case the premisses do not establish the conclusion; there must be some hidden lemma which needs to be added to the premisses already stated, and to which the global counterexample is also a local counterexample. When articulated, the new lemma has to be added as a condition on the full statement of the theorem. (The conjecture is turned into a theorem by having the antecedent conditions, the premisses of the proof, built into it. Thus if $\forall x(Px \rightarrow Qx)$ is the conjecture that, for all polyhedra, $V - E + F = 2$, and $\forall x(P'x)$ is the statement formed from the conjunction of the premisses bound by a quantifier, then the 'proof generated theorem' can be expressed as $\forall x(P'x \rightarrow Qx)$.)

Lakatos gives many examples of this pattern of growth, most of them relating to the conjecture about polyhedra. Here is an example of his from a more familiar branch of mathematics. Cauchy had announced (and 'proved') the statement that any convergent sequence of continuous functions has a continuous limit function. But there are well known counterexamples to this, such as the series, noted by Abel,

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

According to Lakatos, this and similar problems in analysis were 'solved' by Abel, not by looking at the proof to discover some hidden lemma which would be refuted by the counterexamples, but by the recommendation that we drastically reduce the intended domain of applicability of theorems in analysis; they were to be stated only for functions which can be expanded as power series. This avoids the exceptions, but also leaves outside the intended domain many functions for which the theorem holds. The retreat to a safe domain ('exception barring') is too drastic. We need some more subtle way of interpreting the counterexamples so as to distinguish exactly between functions for which the theorem holds and those for which it does not. It was Seidel who, in this particular instance, made the methodological breakthrough by examining Cauchy's proof and discovering the condition of uniform convergence which needs to be added to a statement of the theorem.[7]

This, then, is the relation between proofs and counterexamples. Counterexamples help us to improve the proof by pointing to hidden lemmas, and proofs help us to interpret the counterexamples. We may know that an object a is a counterexample to a conjecture. By discovering a suitable lemma to which a is also a counterexample we show that a belongs to a class of objects which lack a property essential to guarantee the truth of the theorem. Lakatos calls this the 'dialectical unity of proofs and refutations'.[8]

3. We come now to counterexamples of the third kind; those which are counterexamples to the conjecture and to one or more of the premisses (global and local counterexamples). Now if we incorporate the lemmas as conditions on the conjecture (and thus make them part of the conjecture itself) such counterexamples are not refutations in the ordinary sense at all; they do not show that the premisses fail to entail the conclusion, and they falsify the antecedent of the revised conjecture, thus satisfying it. Suppose that our theorem states that for all 'normal' polyhedra, $V - E + F = 2$. A polehedron with a tunnel (thus contradicting the definition of 'normal') and for which $V - E + F = 0$, trivially *instantiates* the conjecture. But is this cause for satisfaction? Is our aim to find the (possibly very narrow) domain of validity of the original conjecture that $V - E + F = 2$, or is it rather to find a *general* relation between V, E and F for any polyhedron whatsoever; a relationship to which our original conjecture was but a poor first approximation? If the second is the case, then counterexamples of the third kind can be very valuable indications of the direction in which we ought to extend our results. Counterexamples like this may guide us to a very sophisticated and general formula giving the Euler characteristic of polyhedra of great complexity.[9] Lakatos calls counterexamples which lead to this kind of growth 'heuristic counterexamples'.[10]

Is there any limit to mathematical growth through the discovery of counterexamples of the third kind? After a point, such counterexamples will no longer remind us of the original intuitive interpretation of our terms, but will begin to 'stretch' their meaning beyond our original intentions. Thus at first we discover counterexamples to our conjecture such as the 'picture frame'; we have little difficulty in accepting this as a genuine polyhedron, for the Euler characteristic of which we ought to have an explanation. But what about the cylinder (p. 22), or two tetrahedra joined along a single edge (p. 15)? Is this one polyhedron? And what about single sided and multi-dimensional polyhedra (p. 98, note 1)? The discussion here is inconclusive. Reference is made to the distinction between descriptive and logical terms: in the case of the former (such terms as 'function', polyhedron') we can stretch their meanings as much as we like. In the case of the latter ('all', 'some', 'not', 'and') their meanings are always fixed.[11] This distinction is no doubt important from the point of view of characterizing logical truth, but for present purposes this is not very helpful; we have already noted that counterexamples of the third kind do not show that the theorem is false or the proof invalid. They show instead the *poverty* of the conjecture; that it does not tell us enough about the class of objects under discussion. Our present problem is when should we *stop* expanding the class ('stretching our concepts')? If we treat the term 'polyhedron' as a descriptive term, and stretch its meaning quite arbitrarily, we will find ourselves landed with the problem of, for instance, classifying continuous functions as polyhedra, for which notions of face, edge and vertex have to be defined, and their Euler characteristics investigated. (The silliness of the example should not obscure the importance of the point.) What is lacking here is a distinction between mathematically interesting concept stretching and concept stretching which is merely vexatious.[12] It will, I think, be impossible to lay down *general* criteria to decide this issue in all cases. Probably the best that can be said is that mathematically interesting concept stretching is that which leads to significant growth; the formulation of interesting new conjectures. But this is hardly a satisfactory answer.

4. Lakatos claims that the method of proofs and refutations – the method which consists of the most sophisticated heuristic rules discussed in the book, and which I shall outline in a moment – did not emerge as a part of mathematical practice until the 1840's. If this is true, how did mathematics proceed before this time? How did great mathematicians like Archimedes, Euler and Gauss produce their results, and how does their mathematics differ from later work?

Proofs and Refutations does not contain answers to these questions, but Lakatos does connect the method of proofs and refutations with a much older pattern of mathematical heuristic, that of analysis-synthesis. Elsewhere Lakatos discusses this in some detail.[13] This method, which he takes from Pappus's account, consists of starting with a conjecture, trying to deduce from it a conclusion which is known to be true (analysis), and then trying to reverse the inference from the known premisses to the conjecture (synthesis). As Lakatos points out, this method may help us to prove a true conjecture, but it will not help us to *improve* a false one; if we arrive at a false consequence of the conjecture we know that the conjecture is false, but we have learnt nothing about how to modify it. In this respect the method of analysis-synthesis is much inferior to that of proofs and refutations; what is the historical connection between the two methods? Lakatos explains Descartes' attempt to provide rules for correct reasoning as an attempt to extend the method of analysis-synthesis to non-mathematical knowledge, and to create an inferential circuit between theoretical and observational statements in science, but he does not suggest that there is any connection here with later developments in the methodology of mathematics.[14] He does make it clear however that the method of proofs and refutations does not make analysis-synthesis redundant. Rather the latter is a heuristic pattern which can form the starting point for the application of the former. Proofs and refutations can begin only once we have discovered some premisses to form the basis of our proof. Analysis-synthesis may help us to discover these premisses. We start with a conjecture P, and we deduce from it consequences $P_1, \dots, P_n$, until we arrive at some conclusion which we believe to be true, $\bar{P}$. We then reverse the procedure and try to deduce P from $\bar{P}$. But this may be possible only with the help of auxiliary premisses $Q_1, \dots, Q_k$. By searching for plausible assumptions which guarantee (or seem to guarantee) the inference from $\bar{P}$ to P we may discover some of the non-trivial premisses of our first attempt at an informal proof (the Q_i's). Thus analysis-synthesis may provide the vital first step in the improvement of a conjecture *via* the method of proofs and refutations.[15]

5. The method of proofs and refutations, as it finally emerges, consists really of two distinct methodological directives. The first is designed to lead to the elimination of counterexamples to a conjecture, and to the formulation of a valid proof of it. It may be summarized as follows.

> (1) *Start with a conjecture, and try to discover plausible premisses which seem to imply it (this may be done with the help of analysis-synthesis). Construct counterexamples to the premisses which are not also counterexamples to the conjecture and replace the refuted premisses by unrefuted ones. Construct also counterexamples to the conjecture which are not counterexamples to any of the explicit premisses, search for hidden lemmas which are refuted by the counterexamples, and add them as conditions to the theorem. In this way counterexamples to the theorem will be neutralized, and the proof made more rigorous by having its assumptions made more explicit.*

The second directive is designed to *strengthen* the conjecture by reformulating it in more general terms. This is done by looking for objects which are, from an intuitive point of view, within the intended domain of inquiry, but about which the conjecture does not give us enough information.

> (2) *Construct counterexamples which falsify both the original conjecture and one or more of the premisses. Analyse the premisses of the proof in order to see what properties these counterexamples lack and which are possessed by objects to which the proof does apply. Try to reformulate the conjecture in such a way as to take account of objects which lack these properties. (This may be done, for example, by introducing a new parameter which has a zero value for the original cases.) Then apply method* (1) *to find a proof of the new, more general conjecture.*

What is the status of these methodological rules? Clearly they are not a 'method of discovery' in the traditional sense. They cannot be applied mechanically, and they do not lead infallibly to interesting and true mathematical results.[16] In this sense Lakatos was not opposed to the now familiar distinction between 'context of discovery' and 'context of justification'. Elsewhere he said 'I take this Kantian demarcation between "logic of appraisal" and "psychology of discovery" for granted. Attempts to blur it have only yielded empty rhetoric'.[17] What Lakatos was opposed to, however, was the further view that the process of discovery is wholly ungoverned by rules of any kind, and not susceptible to any rational analysis. In his view discovery, particularly in mathematics, is not just a matter of making guesses; we make our guesses against a background of vague heuristic rules which can, to some extent, be articulated.[18]

The idea of an heuristic is one which came to play an important role in his later philosophy of science. *Proofs and Refutations* sets out heuristic rules which can guide the mathematician; desiderata of a very general kind which are intended to apply to research in a very wide variety of fields. Following these rules does not put a mathematician in one research tradition rather than another. Later, Lakatos developed the idea of heuristic to cover rules of a narrower kind; those which define research within a particular theoretical framework. The heuristic of a research programme guides the construction of particular theories which accord with some metaphysical view.[19]

Another idea of Lakatos's which is central to his philosophy of science began in *Proofs and Refutations*; the idea of rational reconstruction of history. In *Proofs and Refutations* the dialogue mirrors to some extent the historical development of the Euler conjecture. As Lakatos puts it;

> The dialogue form should reflect the dialectic of the story; it is meant to contain a sort of *rationally reconstructed or 'distilled' history. The real history will chime in in the footnotes, most of which are to be taken, therefore, as an organic part of the essay.*[20]

And at one point one of the characters makes an historical claim which, according to the accompanying footnote, is not wholly correct. Lakatos remarks;

> 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.)[21]

The use of the plural ('reconstructions') here is of some significance, for it is clear that, whatever else Lakatos meant by this rather peculiar remark, he did not suppose that there is *one* 'rational' history to which actual events may or may not correspond – I take it that Hegel thought something like this – but rather that there are different *versions* of what ought to have happened in history if the process of intellectual development had been entirely governed by rational considerations, and that these versions depend upon different theories of rationality.[22] Rational reconstructions of history are, for him, a matter of convention, not of some higher, spiritual reality. However statements like the above, in which we are encouraged to ignore deviations from the course of actual events, have been taken by some to indicate that Lakatos held that the philosopher is somehow at liberty to invent his own version of history, and compare his own fabrication of history, rather than history itself, with what he would expect to be rational behaviour.[23] I do not think that this was Lakatos's real view, but this is perhaps not the place to argue such a point. For present purposes we need only note that 'deviations' between the text and the historical footnotes have little more than pedagogic importance. Lakatos had two different aims in that book; first, to expound a set of heuristic rules of mathematical discovery; second, to show that the procedures to which he referred, approving of some and not of others, had, as a matter of historical fact, often been adopted by mathematicians. The first aim can best be achieved by proceeding from the simplest cases to those which are complex. The fact that history does not, in any particular instance, mirror this pedagogically convenient order is something wholly unsurprising. What the text presents is not, properly speaking, a 'rational reconstruction of history'; its relation to history is, in this sense, quite incidental to its pedagogic merit. It is, I think, one of the failures of Lakatos's exposition that he gave insufficient weight to the aim of defending his heuristic rules on the grounds of their intrinsic merit, and too much to their harmony with historical events.

This failure is partly explained by the fact that Lakatos had yet another aim in the book, one which does link pedagogy with history. He wanted to show how mathematical ideas can be explained in an illuminating way by showing how they developed historically, rather than by the conventional take-it-or-leave-it approach of most text books. Theorems and definitions with complicated conditions are difficult to understand and seem arbitrary unless one knows that the conditions arose as a response to counterexamples, and developed out of a much simpler but unfortunately false 'naive' conjecture. Lakatos gave some examples of what he called 'proof generated definitions' in the final chapter of *Proofs and Refutations*; functions of bounded variation, the Riemann-Stieltjes integral, and measurable sets.[24] He gives a brief historical sketch attempting to show that bounded variation emerged out of Dirichlet's attempts to discover the conditions under which a function is Fourier expandable. There then follows a passage which can help us to understand the real importance of 'rational reconstructions of history' in this work. Having remarked that his exposition of the historical example just mentioned follows a pattern of Popperian conjectures and refutations, he goes on:

> But even in a rational heuristic of the Popperian brand one has to differentiate between problems which one sets out to solve and problems which one in fact solves; one has to differentiate between 'accidental' errors on the one hand which just disappear, and criticism of which does not play any role in the further development, and 'essential' errors, which in a sense will be preserved also after refutation and on the criticism of which further development will be based. *In the heuristic presentation the accidental errors can be omitted without loss, to deal with them is the business of history only.*[25]

I am not sure that I understand everything in this passage, but its main message seems to be that, although a pedagogically sound presentation of mathematical results ('the heuristic presentation') will show how a concept has developed historically in response to problems, it will not simply be a recapitulation of all the events in the historical development of that concept; for some of those events will be less relevant than others to an understanding of that concept as it stands today. So, from a certain point of view, or with respect to a certain aim, some historical events will appear as 'essential' and others as 'accidental'. But this implies no *metaphysical* division of historical events into those which conform to 'objective' developments and those which do not. The idea of rational reconstructions, as it is applied in *Proofs and Refutations* can, I think, be divested of its idealistic overtones without loss of content. Let us examine a passage where Lakatos describes what he takes to be the Hegelian conception of heuristic.

Mathematical activity is human activity. Certain aspects of this activity – as of any human activity – can be studied by psychology, others by history. Heuristic is not primarily interested in these aspects. But mathematical activity produces mathematics. Mathematics, this product of human activity, 'alienates itself' from the human activity which has been producing it. It becomes a living, growing organism, that *acquires a certain autonomy* from the activity which has produced it; it develops its own autonomous laws of growth, its own dialectic. The genuine creative mathematician is just a personification, an incarnation of these laws which can only realise themselves in human action. Their incarnation is, however, rarely perfect. The activity of human mathematicians, as it appears in history, is only a fumbling realisation of the wonderful dialectic of mathematical ideas ... Now heuristic is concerned with the autonomous dialectic of mathematics and not with its history, though it can study its subject only through the study of history and through the rational reconstruction of history.[26]

It would be an interesting and difficult task to decide how much of this, taken as a statement of metaphysical position, is true and how much false. But let us instead ask, does this view really characterise the concept of heuristic as Lakatos uses it in this book? The answer is, I believe, that it does not. The term 'heuristic', which Lakatos explicitly takes over from Polyá, is used by Lakatos to denote the study of informal methods of mathematical discovery, which in this book are summed up in the 'heuristic rules' of the method of proofs and refutations.[27] These are quite plainly not rules of autonomous development, but rules which are available to the mathematician and which may help him to improve his mathematical conjectures and his proofs. Whether they are applied or not is a matter of human decision. The outcome of their application depends – partly – upon the objective facts about the mathematical structure under examination, and upon the logical relations which actually subsist between the statements involved. But this dependence does not warrant the introduction of the notion of an 'autonomous dialectic of mathematics', for there is no reason for us to assume that there is anything about these structures and logical relations which is susceptible to temporal change. Indeed it would only complicate our picture of mathematical discovery if we were to do so. To refer to the second last sentence quoted, heuristic, as Lakatos actually employs that concept, refers to the 'activity of human mathematicians', and not to the 'wonderful dialectic of mathematical ideas', whatever that may mean.[28]

6. The final part of the dialogue (chapter two) is rather inconclusive. A new proof of Euler's conjecture is discussed. It is due to Poincaré, and utilizes the methods of vector algebra. It explains the Euler characteristic of ordinary polyhedra, as well as star polyhedra (and, incidentally, the cylinder!), but it leaves unexplained polyhedra which are not simply connected (pp. 119–20). In the context of the dialogue, the proof results from the attempt to avoid endless concept stretching by reducing the conjecture and its proof to primitive terms which are 'perfectly well known', and therefore not susceptible to any legitimate change of meaning (pp. 106–8).[29] In response to this, doubts are raised about the reliability of the translation from the (dubious) terms of intuitive geometry into the (supposedly clear) terms of vector algebra (pp. 120–1). Does the translation preserve the meanings of the original terms? This question, as it relates to the case of polyhedra, is not really answered in this chapter, but the issue of translation is taken up by Lakatos in a later paper where he deals with the relation between formal and informal mathematics. Is consistency the only methodological constraint on the acceptability of a formal theory? Lakatos proposes that we demand of a formal theory that it satisfy certain material adequacy requirements as well; that it should, in particular, be a satisfactory formalization of some piece of intuitive, informal mathematics; in other words, that it should be a faithful translation of informal results into the formal theory. He says

> ...if we insist that a formal theory should be the formalization of some informal theory, then a formal theory may be said to be 'refuted' if one of its theorems is negated by the corresponding theorem of the informal theory. One could call such an informal theorem a heuristic falsifier of the formal theory.[30]

As an example of this idea, Lakatos notes the introduction of ω-consistency as a constraint upon first order theories of arithmetic.[31] But the general problem is, as Lakatos noted, not susceptible to an easy solution. One cannot *automatically* reject a formal theory which clashes with informal results, because the informal theory may itself have been shown to be inadequate in some way. As he says 'Some argue that after the destruction of naive set theory by *logical* falsifiers [the discovery of the paradoxes] one cannot speak any more of set theoretic facts; one cannot speak of an *intended* interpretation of set theory any more.'[32] He notes on the other hand that it seems likely that conjectures about very large sets can be tested against results in more elementary branches of mathematics.[33]

One further point needs to be discussed concerning Lakatos's attitude towards formal mathematics; to what extent is Lakatos opposed to the idea of formalized mathematical theories? He is certainly opposed to the *identification* of mathematics with formal mathematics; 'Formalism denies the status of mathematics to most of what has been commonly understood to be mathematics, and can say nothing about its growth'.[34] But can we not achieve a peaceful coexistence between the *heuristic* study of informal, growing mathematics, and the metamathematical study of formal mathematical theories? Surely there need be no contradiction between the two activities. At one point Lakatos rejects the idea of studying formalised mathematical theories on the grounds that there are only two things which such a study can consist in. First, the solution of 'problems which a suitably programmed Turing machine could solve in a finite time (such as: is a certain alleged proof a proof or not?). No mathematician is interested in following out the dreary mechanical method prescribed by such decision procedures.' Secondly one can investigate a formal theory in order to 'discover the solutions to problems (such as: is a certain formula in an undecidable theory a theorem or not?), where one can be guided only by the method of unregimented insight and good fortune'.[35]

Two things ought to be said about this accusation. First, the fact that a problem is susceptible of a mechanical solution does not render that problem unimportant. Surely it is one of the most philosophically important achievements of modern logic to have shown that the question of whether a putative proof really is a proof is one to which a definitive answer can be given. As to the second part of the accusation, this is really a very strange thing for Lakatos to be saying. Surely we can investigate formal systems and the problems which they raise, such as questions of decidability, by using the very same heuristic procedures as those which he advocates for ordinary mathematical problems. There is no reason to suppose that the only way to answer informal metamathematical questions is with 'unregimented insight and good fortune'. Indeed, elsewhere Lakatos emphasised that the formalisation of a mathematical theory, far from signifying the end of its growth, may open up new areas of informal investigation at the metamathematical level.[36]

University of Otago GREGORY CURRIE

NOTES

* Review article of I. Lakatos: *Proofs and Refutations. Essays in the Logic of Mathematical Discovery.* Edited by J. Worrall and E. G. Zahar, Cambridge University Press, 1976, xii + 174pp. An earlier version of this paper was read at the University of Oslo in 1977, where valuable comments were made by Professor D. Føllesdal. I am also grateful to Professor Alan Musgrave for his criticisms.

[1] 'Essays in the Logic of Mathematical Discovery', University of Cambridge Ph.D., 1959.

[2] 'Proofs and Refutations', *British Journal for the Philosophy of Science* 14 (1963–4), 1–25, 120–39, 221–43, 296–342.

[3] All references to *Proofs and Refutations* in the text are to the book rather than to the articles.

[4] The comparisons with Popper's philosophy of science are obvious. Lakatos wrote that *Proofs and Refutations* 'should be seen against the background of Polyá's revival of mathematical heuristic, and of Popper's critical philosophy.' (See *Proofs and Refutations*, p.xii.).

[5] Ibid., p.9, italics in the original.

[6] 'What does a Mathematical Proof Prove?', in J. Worrall and G. Currie (eds.), *Mathematics, Science and Epistemology*, p. 63. Cambridge University Press, 1978.

[7] Appendix 1 of *Proofs and Refutations* (in which the material on Cauchy and Seidel is contained) should be read in conjunction with a later essay by Lakatos, called 'Cauchy and the Continuum', in *Mathematics, Science and Epistemology.* In *Proofs and Refutations* there is no real explanation of Cauchy's apparent mistake in proving the false theorem according to which all convergent sequences of continuous functions have a continous limit function. Although Lakatos describes Cauchy as a 'rigorous exception barrer' whose main contribution to mathematical rigour was to insist that the 'mathematician should not stop at the proof; he should go on and find out what he has proved by enumerating the exceptions, or rather by stating a safe domain where the proof is valid' (p. 55), Lakatos produces no evidence to show that this was Cauchy's attitude towards the exceptions to his theorem on continuous functions. Indeed,

Lakatos suggests that Cauchy did his best to ignore the exceptions (see p. 131). Where, then, is Cauchy's 'rigorous exception barring method'? The problem appears in a very different light in Lakatos's later paper. There he argues that Cauchy did not make a mistake at all when he proved his conjecture, at least not in the commonly accepted sense. Instead Lakatos claims that Cauchy's theorem is embedded in a theory of the continuum ('the Leibniz theory') which was quite different from Seidel's ('the Weierstrass theory'), and relative to which 'Cauchy's theorem was true and his proof as correct as an informal proof can be' (p. 49). Thus the Fourier series were not counterexamples to the Leibnizian theory of the continuum. Seen in this light, Seidel's result was not a correction of Cauchy's 'invalid' proof, but the radical reinterpretation of it against the background of a different research programme.

[8] *Proofs and Refutations*, p. 37. See also p. 48, where Sigma (one of the characters in the dialogue) say '... not only do refutations act as fermenting agents for the proof analysis, but proof analysis may act as a fermenting agent for refutations'.

[9] The Euler characteristic of a polyhedron is the number given by $V - E + F$, whatever that number is. (V is the number of vertices, E the number of edges, F the number of faces.)

[10] Lakatos uses the term 'heuristic counterexample' in 'A Renaissance of Empiricism in the Recent Philosophy of Mathematics?' to denote informal theorems which contradict results derivable in the formal theory which is an attempt to make that informal theory precise.

[11] Lakatos refers to Bolzano's interesting relativization of the notion of logical truth; that a statement is logically true relative to constituents a, b and c if it is true independently of the meanings of those terms (p. 103, note 1). But Bolzano also had some notion of *absolute* logical truth (see his *Wissenschaftslehre*, section 148).

[12] The editor's remark, correctly, that for the purposes of the validity of a proof, it does not matter how we stretch the meanings of our descriptive terms. But this is not really Lakatos's concern here, though his discussion of Bolzano's notion of logical truth makes it sound as though it is. As I remark in the text above, the question of concept stretching here is related to the question of how long should we continue to be concerned with counterexamples of the *third* kind; counterexamples which do not affect the truth of the theorem or the validity of the proof. They tell us, rather, that the theorem is not informative enough. (See their note *1, p. 100.).

[13] 'The Method of Analysis-Synthesis', in *Mathematics, Science and Epistemology*, chapter 5.

[14] Ibid., pp. 75–88.

[15] See 'The Method of Analysis-Synthesis', p. 96.

[16] This was Descartes's ideal for a method of discovery. (See his *Rules for the Direction of the Mind*, p. 9, in Haldane and Ross (eds.), *The Philosophical Works of Descartes*, volume 1.)

[17] 'Why did Copernicus's Research Programme Supersede Ptolemy's?', now in Currie and Worrall (eds.), *The Methodology of Scientific Research Programmes*, chapter 4. Cambridge University Press, 1978.

[18] This point is due to Polyá (see his *How to Solve it*). Lakatos translated this work into Hungarian while he was employed as a translator at the Mathematical Institute in Budapest, 1954–6. (See J.W.N. Watkins, 'Imre Lakatos', *The Encyclopedia of the Social Sciences*, forthcoming.) For a development of the idea of rules of discovery and

a criticism of Popper's theory that the process of discovery is beyond rational analysis, see Peter Urbach, 'The Objective Promise of a Research Programme', in G. Anderson and G. Radnitzky (eds.), *Progress and Rationality in Science, forthcoming.*

[19] See 'Falsification and the Methodology of Scientific Research Programmes', pp. 49–52, in *The Methodology of Scientific Research Programmes.* Lakatos gives one example of rival research programmes in mathematics; the struggle between Archimedean and non-Archimedean theories of the continuum. See above, note 7.

[20] *Proofs and Refutations*, p. 5, italics in the original.

[21] Ibid., p. 21.

[22] See 'History and its Rational Reconstructions', now in *The Methodology of Scientific Research Programmes*, chapter 2, pp. 108ff.

[23] This is how Lakatos is interpreted by Kuhn. (See his 'Reflections on my critics', in Lakatos and Musgrave (eds) *Criticism and the Growth of Knowledge*, p. 256), and more recently by Laudan (*Progress and its Problems*, pp. 168–70).

[24] Only bounded variation is given an historical explanation here.

[25] *Proofs and Refutations*, p. 149, my italics.

[26] Ibid., p. 146.

[27] Ibid., p. 50.

[28] I am very grateful to Alex Bellamy for discussions of this problem, though I am afraid that he will not agree with my conclusions.

[29] For a clear exposition of this proof see Coxeter, *Regular Polytopes*, chapter 9.

[30] 'A Renaissance of Empiricism in the Recent Philosophy of Mathematics?', now in *Mathematics, Science and Epistemology*, p. 36.

[31] Ibid., p. 37.

[32] Ibid., p. 36.

[33] For some reservations about this idea see the editor's note* on p. 41, ibid.

[34] *Proofs and Refutations*, p. 2.

[35] Ibid., p. 4.

[36] 'What does a Mathematical Proof Prove', p. 69.