PART VIII

LAKATOS' PHILOSOPHY OF MATHEMATICS

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The Formal and the Informal

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I became acquainted with Lakatos's work in 1965 when I started studying at London School of Economics--where Lakatos taught. As his work was developed over the succeeding years until his death in 1974, one thing always puzzled me: his work seemed to contain such conflicting tendencies. He would continue developing his ideas along a progressive line, and suddenly would insert an element which appeared to me quite reactionary. By 'reactionary', I should hasten to add, I mean imbued with the spirit of Positivism--a person of different bias might reverse the labels!

When I was given this opportunity to reflect again on Lakatos's work I did not try to resolve the puzzle by looking into Lakatos's intellectual history[1]; rather I attempted to separate clearly those aspects of Lakatos's work which seemed to me driving in the right and the wrong directions. Both aspects concern the research program which has dominated both philosophy of science and philosophy of mathematics in the twentieth century. This research program is aimed at constructing a formal system in which all important disputes are rationally decidable. The progressive aspect of Lakatos's work was his critique of this program in mathematics and in science, and his attempt to go beyond it. The reactionary aspect was his falling back into the program, and attempting to produce decisive criteria for settling disputes.

In the first part of this paper I explain how Lakatos's work constitutes a critique of the program, and in the second I discuss his attempts to go beyond it. Along the way I note his major lapses back into the program.

1. The Limits of Formal Systems

In modern philosophy Leibniz is the origin of the program of constructing a formal system within which all disputes would be rationally

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decidable. There were two aspects to this program. For necessary truths, the formal system could resolve any dispute with certainty. For contingent truths, the statements of empirical science, a calculus of probabilities would be necessary to show which belief was most probably correct and therefore which belief any rational person must hold to. Together these two aspects would enable rational people to agree upon the truth or falsity of any claim.

Around the turn of this century Leibniz's program was taken up by Frege ([8], [9]) and Russell [23] and subsequently by many others who have attempted to develop it. Russell attempted to carry out the program first for mathematics, hoping subsequently to be able to extend it to providing a foundation for the empirical sciences. Others, for example Hilbert [13] followed in trying to develop a formal system within which all mathematical claims could be decided. For science, Keynes [16]--partly under the influence of Russell--tried to develop a probability theory within which empirical questions would be decidable, and many followed in his footsteps, including Reichenbach ([20], [21]) and Carnap [6].

The extent to which the various followers of Leibniz have shared his optimism about the power of formal systems has no doubt varied. The key point for me here is that they have presumed that the limits of reason were the same as the limits of whatever a formal system can decide.[2] This presumption has, I believe, been the impetus for much of the work which has been done in developing formal systems for mathematics and for induction in empirical science.

1.1 The Limits of Formalization in Mathematics

Before I explain Lakatos's critique of Leibniz's program in mathematics, let me explain how Lakatos's conception of what he was criticizing is related to Leibniz's program. Lakatos's main target in philosophy of mathematics was what he (perhaps unhistorically) called 'Euclideanism'. This is the attempt to reduce mathematics to a set of trivial axioms. The axioms would be trivial in the sense either that they would be self-evident to any rational person, or in the sense that they are truths of logic, tautologies.

Euclideanism is one way to attempt to carry out the program of *finding a formal system to decide all mathematical questions*. The key goal here is to find axioms which are certain, and methods of proof which are also infallible. Need for certainty springs from the nature of the Leibnizean program. Formally valid inference by itself cannot force a rational person to accept anything but tautologies. We may attempt to reduce mathematics to logical truths as Frege and Russell did, but here it is necessary to insure that the principles of the logic themselves are obvious to any rational person. If we accept that non-tautologous assumptions are needed in mathematics, these again must be self-evidently certain to any rational person.

If the principles of the logical system or the non-logical principles are not self-evident and certain, they will not be an objective decision procedure. One rational person may reject an assumption and another accept it. But then rationality will not by itself decide a question: there will be room for subjective feelings. Only with rational certainty can we make a decision method which is conclusive for all rational people. Thus certain axioms and methods of deduction are required.

The attempts to provide a certain foundation for mathematics in the early part of the century are well known: The Logicism of Frege and Russell, the Formalism of Hilbert, and the Intuitionism of Brower [5] and Heyting [12]. Lakatos focused his critique on the first two. To understand the nature of Lakatos's critique it is helpful to see its relation to Gödel's critique. Gödel[10] showed that no formal axiom system could suffice to provide means of deciding all questions concerning number theory. And he showed that the consistency of a formal system sufficient for number theory could not be proved by the kind of supposedly secure methods which Hilbert had hoped to use.

Gödel's critique was thus an internal critique of Logicism and Formalism: he took the assumptions of these programs and showed that they could not do the work demanded of them. There would according to Gödel always be an informal side to research in mathematics, a side which could be included in the formalism after discoveries were made, but which could not be gotten out of the formalism in advance.

Lakatos's critique is largely in harmony with Gödel's, but is an external critique, and while less compelling is in a sense more radical. For while Gödel showed that formal methods would always be incomplete, Lakatos argued that they have often been and will always be capable of being in error. Lakatos's point is that formal systems are designed to formalize something informal, and when we can find statements in the informal theory which we want to hold as true, and they contradict the assumptions of the formal theory, then they may in fact be refutations of that theory. For example, the existence of geometrical objects where there is more than one parallel to a line through a given point refutes Euclid's geometry.

The importance of such counter-examples--and of informal mathematics generally--was shown by Lakatos in his brilliant dialogue Proofs and Refutations [14]. He has shown that refutation has played a crucial role in the development of mathematics. Theorems thought proved have been refuted, the mathematical concepts altered and new proofs formulated to take account of the counter-examples. This process of trial and error has been part of the engine of progress in mathematics. And furthermore, it can continue to be. Just as previous advances in mathematics have led us to reject, e.g., Euclidean geometry, as generally true, and now regarded it as true only for a restricted class of geometrical objects, so present day mathematics may be fundamentally modified by future advances.[3]

Assuming Lakatos's critique to be correct, what role, if any, should 'foundational studies' play in mathematics? If Lakatos is indeed correct, we cannot expect to secure mathematics by reducing it to logic, to psychological intuitions, or to Platonistic mathematical objects. But what we can do is the reverse: we can try to use mathematics and logic to investigate the nature of logic, of our intuitions concerning number, and of the Platonic nature of mathematical objects, whatever that turns out to be. Such an investigation is fallible, but it may be extremely interesting. The results of such investigating can in turn affect mathematics: they can stimulate the growth of new mathematical theories. And thus there can be an interaction between non-"Foundational" studies of the nature of mathematics and logic and the subjects themselves.

For example, the investigations in proof theory, especially Gödel's results, have already given us considerable enlightenment into the nature of proofs and into the limitations of proofs in mathematics. And these results in the hands of Robinson [22] for example, have led to new methods in mathematics proper.

Similarly, such investigations may help enlighten us concerning our mathematical intuitions. We may use the formal systems as possible models of our thought processes, and test them empirically. However, if we are to use these systems effectively, we must be clear that we are undertaking a fallible psychological investigation, and not giving mathematics a secure foundation. That is, we should be clear that we are trying to do psychology and not trying to carry out the program of psychologism. Intuitions are empirical facts, but they 'are not mathematically authoritative'.

For whatever our mathematical intuitions turn out to be, we will not be constrained to adopt them as the system basic to mathematics. For example, they may be inconsistent, as Russell's paradox indicates. Or they may turn out to be inadequate for physical theory. As Popper has pointed out, Brouwer's reliance on our intuitions of time as the basis of mathematics is vitiated by Einstein's analysis of the relativity of simultaneity and the new concepts of time in relativity. Thus, for example, we should reject Piaget's claim that we should settle which is the better approach to set theory--Russell's or Zermelo's--by finding which one actually matches our intuitions as they develop in childhood. But we should take seriously these systems as possible models of our psychological processes of thinking about mathematics.

The same attitude should, I think, be taken toward the other purported bases of mathematics: the relations of mathematics to the natural world, the nature of the idealized objects of mathematics, the relations of logical and mathematical concepts--these issues should be taken as interesting problems to be investigated rather than as ways of securing mathematics against refutation, something which cannot be done.

1.2 Attempts to Formalize Theory Choice in Science

As in the case of mathematical claims, there have been in the twentieth century a number of attempts to create a formal system within which competing scientific claims would be decidable. Here the effort has been to produce an 'inductive logic' which would provide such a decision method for choosing between competing scientific claims.

Lakatos viewed the attempt to create an inductive logic as part of the 'Justificationist' program, the program to conclusively justify all rational claims. The relation between the attempts at justification in science and the Leibnizean program is similar to the relation between Euclideanism in mathematics and the Leibnizean program: the demand for a decision procedure which will force agreement requires some premises which any rational person must hold to, and a method of inference which any rational person must assent to. In mathematics the attempt was to find the basic premises--axioms--on which mathematics should be based, and to make the deductive connections between axioms and theorems 'gap free'. In the choice between scientific claims, the attempt was made to reduce claims of observation and experiment to a reliable observational basis, and to create methods of inductive inference which any rational person must follow.

When we look at Lakatos's criticism of Carnap in this light, we can see its similarity with his criticism of Foundational studies in mathematics. Lakatos had a number of objections to Carnap's system, the most fundamental of which was probably his point that the formal language Carnap wished to use could not be independent of scientific theory. In fact, when present theories are altered, the concepts in them are likely to be changed. This objection is in fact devastating to Carnap's program. Let me explain.

The problem which Hume already saw is that principles of inductive inference must themselves be empirical, and thus a rational person need not be constrained to decide in favor of a belief which has been inferred from observation using the inductive principles. Carnap, it seems from Lakatos's account, had hoped to *finesse* this difficulty by constructing a formal system which did not rely on empirical assumptions. But then Lakatos pointed out that not only does the construction of his formal system make some empirical assumptions, but it also makes assumptions which are likely to be overthrown with the refutation of existing theories--a possibility (refutation) which Carnap never took as a serious possibility.

Lakatos's argument, then, is quite similar to the one he used in criticizing the foundations of mathematics. As noted earlier, he pointed out that the construction of formal systems involves the attempt to capture some pre-formal theory, and that the formal system may be, and frequently is, refuted in the course of further progress in mathematics.

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The moral concerning the Foundations of argument in science also seems to me similar to that concerning foundations of mathematics. We can investigate psychologically and sociologically how scientists--and non-scientists--come to make choices between competing theories. And we can give arguments, based on some empirical claims, that one method of evaluation is superior to another for specified purposes and situations. But such methods of evaluation cannot be expected to compel all rational decision-makers to make the same decision. The standards of evaluation can only be one ingredient in the decision, and the thought, experience, and feelings of the decision-maker another ingredient. When we do take such standards to be based on empirical theories, and as tentative and debatable, then the need for a special inductive logic vanishes, however. For when the empirical assumptions are made explicit they can be used and argued about using deductive logic.

Generally speaking, Lakatos's arguments show that both in mathematics and in scientific inference, we cannot identify the process of rational decision-making with whatever can be decided within a formal system. Both in mathematics and in scientific inference, that system must meet demands made externally to it, and as science and mathematics grow, they can be expected to fail these demands and to be in need of alterations. Formal systems, then, should be viewed as an aid to rational inquiry and decision-making, and not the whole of the subject. I have argued at length elsewhere [3] that to develop the theory of rationality further, we should view rational inquiry--with or without the aid of formal systems--as a guide to choice rather than as a full determinant of choice. However, it would be out of place to continue with the more general argument, as my subject here is Lakatos's contribution to the evaluation of the strengths and weakness of formal systems.

I should note that Lakatos himself did not draw the same moral that I have from his critique of Carnap. Perhaps because he viewed the problems in terms of Euclideanism and Justificationism, and did not focus on the issue of decision-making, he fell back to a search for standards which could compel agreement amongst rational people. His feeling of a need for such standards, I believe, as the reason for his (unhistorical) idea of a 'hard core' to a research program, and for his call to regard Popper's 'corroboration' as a measure of inductive support. What I have explained so far I hope makes clear why I think these 'reactionary' moves are a bad idea. When we look at Lakatos's critical work as a critique of the program identifying rationality with decidability in a formal system, we can see that Lakatos's own attempts to produce criteria dictating uniform decisions are a mistake.

2. The Interaction between Formal and Informal

Having discussed the important critical side of Lakatos's contribution to the evaluation of formal systems, I would like now to consider the positive side. Perhaps the most fascinating of Lakatos's

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ideas is his notion of 'concept-stretching', and it is this notion around which cluster his attempts to understand the interaction between the formal and informal aspects of mathematics.

Let me explain the basic idea involved in 'concept-stretching'. We begin with a purported theorem which some mathematician has advanced, and has given a proof of. Next, it is often found, as Lakatos has documented, that there are counter-examples to the theorem. In response to the counter-example a variety of moves are possible. One of these is 'exception-barring': merely stating that the theorem holds except in a particular case. Another is 'monster-barring': stating that the counter-example is a monster that the theorem was never intended to cover anyway, and so no counter-example at all.

The most progressive response is 'concept-stretching'. In this response two things are usually involved. First, a new 'hidden' lemma is added to the proof, and this lemma excludes the counter-example. Second, the theorem is re-interpreted to hold not only for the originally intended model (minus counter-examples), but for all models which also render the premises of the proof true. The adding of independent statements to a set of statements in general reduces the set of models of those statements. When we regard the proof as an implicit definition of the concept in question, this means that the concept has in one way been narrowed. On the other hand, when we regard the set of premises as an implicit definition, we allow the concept to stretch to include any model of that set of statements, and not merely the original intended model. In this manner, mathematical concepts are warped, stretched, and altered to suit what seems to be mathematical reality. This concept-stretching is reflected in the unintuitive and often elaborate definitions of basic concepts in modern mathematics.

Lakatos's description and documentation of this process is, I believe, the first attempt to formulate a theory of how formal and informal mathematics interact during the growth of mathematics. This pioneering effort no doubt needs to be much further developed, but it does seem very much worthy of such development. I would like to explain how it could be further developed to understand the nature of mathematics, but unfortunately I don't know how. So instead I will try to give some idea of the directions in which the notion of 'concept-stretching' applies in other fields, namely in psychology and in methodology of science.

2.1 Concept-Stretching as Psychology

Lakatos's description of the development of mathematical thought, if correct, should be a correct partial description of what went on in the minds of various individuals, and so should be good psychology. As I explained earlier, there is an important difference between this and attempting to secure mathematics from refutation by basing it on mathematical intuitions, or in other words of trying to carry out the program of psychologism. Here I want to consider what light Lakatos's

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notion of concept-stretching throws on the process of learning. How Lakatos's theory fits in with other psychological theories is most easily seen by comparison with Popper's theory, of which it is a development. As Wettersten [26] and I [4] explain elsewhere, Popper's theory of learning is part of psychology proper, and as such it is a novel and interesting theory. Popper's basic idea is that one of the ways we learn is by seeing counter-examples. Though Popper's psychological theory is still in a crude form it is nevertheless novel. This we can see by comparison with the learning theories of Piaget and of Selz and his followers, the contemporary psychological school which has attempted to make computer simulations of learning.

In Selz's theory, ([24], [25]) we learn by trial and error, as in Poppe but these trials and errors are attempts to fill in gaps in our existing mental framework or extend that framework. In Popper's theory the problems are counter-examples, which don't fit into the framework, and the trials and errors are attempts to alter the framework. Piaget has similarly described the expansion and alteration of frameworks, but only insofar as the new frameworks totally incorporate the old ones, and do not abandon them.

Popper has described the counter-example, the trial and the error in logical terms. The counter-example may be represented by a singular statement and the general belief which it contradicts by a universal statement. The new belief may be represented by a new universal statement from which the counter-example to the old belief may be deduced. How exactly these logical relations are represented psychologically is a problem Popper has not addressed.

Lakatos's idea of 'concept-stretching' adds another kind of response to counter-example to Popper's story. The construction of the new theory may involve not the alteration of a formal structure, but the re-interpretation of the meaning of the terms. The meaning shifts would exclude the counter-example as a counter-example, and possibly at the same time expand the theory in other directions. What this all means in psychological terms is of course far from clear: psychologically what is a concept as opposed to a theory? Do formal structures as opposed to their interpretation have a psychological representation in anyone's heads outside of those working mathematicians? Is the recognition of a counter-example a matter of form, interpretation, or what? Though Lakatos's notion of concept-stretching, considered as a psychological theory, raises many questions and answers few, it seems to me that they are very interesting questions, and worthy of empirical investigation.

2.2 Concept-Stretching in Science

Although Lakatos devoted much energy to analyzing research programs in science, he perhaps surprisingly did not apply his notion of concept-stretching there. Instead he developed the historically inaccurate idea of the 'hard core' of a research program. The idea of concept-stretching is applicable to the development of scientific theories, and its application raises some interesting questions.

One of the issues 'concept-stretching' throws light on is the controversy over 'incommensurability'. As Nancy Nersessian has pointed out to me, there has been a dichotomy on this issue between those who believe that some invariant and neutral observation language may be used to compare successive theories, and those who believe that the changes are sometimes so radical as to not admit of rational comparison. In fact, she has noted, the considerable continuity which exists between concepts in successive theories shows that both extreme positions are mistaken.

Let me take one example and show how Lakatos's 'concept-stretching' helps us see what is going on here. Maxwell originally intended to apply his electromagnetic equations to a mobile ether, and regarded charge as a state of strain in the ether. Lorentz showed that there were some serious difficulties in developing a theory of the mobile ether. And partly in response to these difficulties, he stretched the concepts which Maxwell had used to interpret his equations. He changed the notion of the ether to immobile framework each point of which could hold an electric or magnetic field intensity. And he introduced charge as a property of individual particles which came as 'strangers', as Einstein put it, into the field.

These conceptual changes did not change the form of Maxwell's equations, but did change the way they were used to explain the experimental facts, changes which led to different results than Maxwell's approach would have.

Next Einstein, in response to difficulties in detecting an ether velocity, again changed the concepts of field theory. He proposed to do without the ether altogether, and to regard the equations as true of any inertial framework. In each of these cases we can see a relation between the counter-examples perceived by the theorist and the way he stretched the concept. Of course, in many cases the form of the equations is changed as well as the interpretation--such as in the changes in the concept of mass in Einstein's theory.

I would like to make two points about these examples. The first is that they are no threat to viewing science as irrational unless we identify rationality with what can be decided in a formal system. Here we cannot expect any formal system to provide us with a decision method. The new concepts which are invented cannot be in advance put into a formal system; and once they appear we can debate about the best way to formalize them and how they relate to older ideas. But there is no block to rational discussion of them: we have been able to devise rich enough languages to discuss both the new and old ideas and we can carry out the deductions needed to compare them to the facts and to each other.

My second point about concept-stretching in science is that it is another unexplored and interesting case of the interaction between

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formal theories, and informal observation reports, initial ideas, and so on. A deeper understanding of this process might shed new light both on the nature of human thought, and on the best strategies of scientific and mathematical research.

Notes

[1] I have explained some of the background to Lakatos's idea on scientific research programs, and criticized them in my paper [1] and in my book [2]. For views of the background to Lakatos's work in philosophy of mathematics see the reviews by I. Hacking [11] and P. Marchi [17].

[2] In this paper I am using the word 'decidable' in a broader sense than that usually used in discussing formal systems: I mean that any rational person who follows the argument or procedure must agree with its conclusions--which is in fact a correct conclusion. I discuss the need to reject the conclusiveness of rational inquiry, and outline an alternative theory in my 'Skeptical Rationalism' [3].

[3] Feferman [7], in his contribution to this symposium, finds much of interest in Lakatos's work, but rejects his basic idea of the eternal tentativity of mathematical results. (See p. 317). He gives the example of Pythagoras' theorem as an end to guesswork. While it is true that the syntax of Pythagoras' theorem has not changed, the semantics have, and this makes the theorem just as different. Furthermore, it has changed in a way that involves rejection of its original intended interpretation. Now it is supposed to be true only in 'Euclidean' space. That growth in mathematics involves rejection of past theorems and axioms in their intended interpretation is one of Lakatos's main novel assertions in philosophy of mathematics--just as the same view in science is one of Popper's main departures. Lakatos argued the appearance of mere extension of past results was only an appearance resulting from a persistence of form, but not substance of a theorem. The change is indicated by changing definitions of basic concepts. For Feferman to rebut Lakatos he would, I think, have to rebut this argument, which he has not attempted to do in his symposium paper.

Similarly, in defending 'the logical analysis of mathematics' (pp. 322-323) Feferman does not address the issue of the possible rejection of such analysis with the further growth of mathematical logic. Lakatos, I think, would not have objected to such logical analysis as Feferman describes, provided it were not aimed at creating an immutable basis for mathematics, or mathematical reasoning.

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