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THE LAKATOSIAN REVOLUTION

1. LAKATOS AS A TEACHER

Lakatos' classical '*Proofs and Refutations*' reports the ongoings in a classroom in Utopia. Lakatos himself tried out the Utopian experiment in a real class early in the day - it was in Popper's seminar, and while he was writing his doctoral dissertation which includes an early draft of his masterpiece. Not surprisingly, then, he was acidly critical of some aspects of the accepted modes of mathematical teaching (to be discussed below). Nonetheless, I know of no discussion of his educational philosophy – printed, manuscript, or orally presented.

Perhaps this reticence, thus far, relates to the fact that Lakatos took active part in the student revolt affair, and the definitely wrong part. There was, I am of the opinion, no right part, at least no obviously right part, to the students' revolts of the sixties anywhere in evidence (except, I think, for the initial demands of the French students, the rejection of which sparked off their revolt); but there were more or less degrees of wrong. The students have demanded at times the wrong things; and they usually did so in the wrong way. But this is not to condemn them off-hand, much less to take a reactionary stand in the name of the preservation of all that deserves preserving, refuse to yield on any point, and recommend non-negotiation. Lakatos did take such a stand.

Perhaps the reticence, thus far, is due to the fact that Lakatos evolved into a highly successful university lecturer of the old style and was highly censorious of those colleagues of his who, he felt, did not live up to the obligations of a university lecturer as he understood them, failed to prepare for each lecture elaborate lecture-notes, cover much informative material in each lecture, etc. etc. I am too averse to the old-fashioned view to do it justice, but I think the reader may be familiar enough with it, even if he is not familiar with its Central European rich and thick and learned and emotionally charged and witty and sweeping variants. I assure the reader that all these epithets fit Professor Lakatos' later performances as a university lecturer in his lecture courses in the London

School of Economics; that he would have taken this ascription of them to him as a high compliment; that I for one do not see anything to praise in the tradition or mold he was so proud to belong to; and that his Utopian lectures, in his classic 'Proofs and Refutations' as well as in his earliest performances in the London School of Economics as a guest speaker in Professor Popper's seminar on scientific method, which I so admire, had as little to do with that tradition as is conceivable at all.

We once used to wonder about the fact that makers and starters of revolutions so often stayed behind. T. S. Eliot has changed this with his deep insight in his *Journey of the Magi.*

2. SCHOOLS AND SCHOLARSHIP

Inasmuch as schools supposedly convey in a condensed manner all that is worth preserving in our heritage, school may just as well be an excellent representation of the heritage. Even when what schools pass on to the vast population that passes through them is not the very best, nevertheless it often is representative. I found from experience that some puzzlements about a foreign culture may be solved by even a superficial perusal of the curriculum, the set texts, and similar school articles.

I suppose it is very customary still, in the curricula and syllabi of many university departments around the globe, to claim that students are offered there not only materials but also the means of acquisition of more materials. These means are soft-wear and hard-wear, with instruments and libraries as hard-wear and techniques of using them as soft-wear. The soft-wear is often carried in the student's – or researcher's – head or notebooks, and is known by the embracing name of methods and/or methodology. Schools, to repeat, often claim to be teaching methods and/or methodology. They do not.

An exception, noted by Lakatos any number of times, is this. Whereas most teachers claim that they teach methods, some of the most high-powered mathematics teachers openly deny that they teach methods. They think that the acquisition of large doses of mathematical knowledge in itself develops in the student a high level of mathematical sophistication which enables him to resolve old puzzles his teacher has chosen to leave unsolved and to develop the necessary skills to continue in his predecessor's lives of investigation into new areas of knowledge.

What Lakatos briefly suggested is that this is a mere excuse; that, in other words, the puzzles a teacher leaves unsolved he cannot solve – at the early stage in which they arise or even at any other state – and that the methods he does not teach he does not know. A fish, Lakatos was fond of saying, may well be able to swim with not the slightest knowledge of hydrodynamics. Lakatos never explained.

Now, let us be open-minded about this. Popper’s philosophy, which influenced Lakatos at the time he was writing his drafts, starts with the assertion that there is no scientific method. And, Lakatos is right in claiming that the view that there exists scientific method in the sense in which Popper combats it – the Baconian sense of a sausage-making machine with a sure output as long as input keeps flowing – thanks to Popper "at least among philosophers of science Baconian method is now only taken seriously by the most provincial and illiterate" (Schilpp’s Popper Volumes, p. 259). Inductivism, Lakatos thought, was definitely out. I am not even quite happy about that since I read Konrad Lorenz’s and Nikolaas Tinbergen’s Nobel Lectures that are fairly Baconian and considering that philosophers of science are still not ready to dismiss as insignificant views of such big fish. Anyway, at least in the present essay we can take it for granted that no Baconian method exists. It is clear that Popper – and Lakatos too – assumes that in other senses methods do exist, however vague this statement is. For example, it is left open whether the more general method of testing in science or more specific methods, more peculiar to given ages and fields, like the mathematical methods in theoretical physics, or the empirical methods common in ever so many contemporary laboratories, etc. Can these be taught? Are laboratory manuals of much use, and, even if they are, should one read them rather than consult them on occasion?

I speak of the teaching of mathematical methods because, I think Lakatos was right: mathematicians did not teach them because they were ignorant of them. I shall claim that the only excuse teachers have for teaching is ignorance – their own; their students’ is taken for granted, of course. If so, then teachers will do better to study rather than teach, since the more they will know the less they will wish to teach: which is all to the good. This is particularly true concerning methods: we all know that foreign aid in the form of consumers’ goods is only good for emergencies and as a stand-by, that the important goods foreign aid can transmit are means of producing consumers’ goods – both hard-wear and soft. What is true of underdeveloped countries is true of underdeveloped individuals.

3. THE PROBLEM OF THE MAGIC OF MATHEMATICS AND LAKATOS' SOLUTION.

Consider the observation of a magician pulling a rabbit out of a top-hat. He can do so spontaneously, or after he is requested to do so by members of his audience. It is in a sense less surprising – namely, expected – in a sense more surprising – that he could do so upon a request. The second sense of surprise, not the first, concerns a mathematics student. For, in the first sense surprise is momentary and can be cushioned by preparation of an anticipation; in the second sense surprise can be retained until alleviated by explanation, and so is a kind of puzzlement. (This is why some magicians like it this way, some that way. Those who like it this way keep talking to avoid interruption, those who like it that way plant people in their audience who make the proper request.) Now observing a magician we are puzzled and are meant to be puzzled; not to understand. But when a mathematics teacher pulls rabbits out of a top-hat we are meant to comprehend, yet we are puzzled. How can we both comprehend and remain puzzled?

There is no doubt that mathematics teachers do pull rabbits of exciting theorems from top-hats of all sorts of axioms and definitions. Whether they do so apologetically – sorry to be unable to relieve the puzzlement as any magician does to his apprentices – or with a vengeance – defying his students as any magician defies his audience, especially if the audience is rather presumptuous – is not a matter I shall go into. I do accept Lakatos' observation: they cannot resolve the puzzlement: at least not as yet. Perhaps later on they can explain the puzzlement to a budding colleague – after he has acquired much more insight to the working of the mathematical method. All right, says Lakatos, let us spell out to our-selves what is this working of mathematical method and how the puzzlement is to be resolved by it. It turns out to be no mean matter: very few studies exist on mathematical methods, whether written by working mathematicians or not, and their whole product is quite meager.

Before Lakatos came to make his mark, a few ideas were extant on the matter. The simplest idea, one which is still not rare in mathematics departments, one which he felt compelled to fight time and time again though now it is somewhat less popular and, so, towards the end of his life he felt he could ignore, is the sausage-machine idea. Essentially, the view goes, you choose a set of axioms, try to avoid inconsistency, and feed them into any old deduction-machine which will start churning out theorems, mostly worthless and uninteresting, but some useful, some interesting, some both. If so, then mathematicians are simply finding needles in haystacks and have no idea which haystack – axioms – to choose or how to increase the frequency of finding needles – Interesting or useful theorems – in the hay-stack. If so, the puzzle is irresolvable, and all a budding mathematician can hope for is better luck and a better intuition for short-cuts. But that's all. The promise which the mathematics professor makes to his students, of a better understanding in maturity, then, is quite pointless: he does not have it himself.

Another theory, which I have heard from Popper in his lectures but I think is not unknown, looks the very opposite of the above idea, but in a surprising manner turns out to be a variant of it. We start not with axioms but with problems, starts the variant, where problems are theorems to be proved. The rest is a matter of intuition and luck: you choose your axioms or you have them given, and you wade through innumerably possible deductive routes, looking for the one which leads you from your axioms to your theorem. If you succeed, you have to tell your students that you can deduce the theorem (or its negation, or its independence), but not why this route was successful, whereas other routes lead not to the same destination. (Nor can you tell whether an open case is decidable, or which way.) This method is a variant, since it only adds lucky shortcuts. There is a lot to this variant: the student is told what he is expected, to understand – namely, each single step in the deduction – and what remains a puzzle – namely, how a specific deductive route was chosen; he can see how the few attempts which have failed have failed, and that the successful one succeeds. The strong evidence in support of this explanation, of the success of the short-cut as lucky, is also mentioned by Popper (in Schilpp's Popper volumes, p. 1077): once a proof is discovered, "almost invariably it can be simplified." That is to say, even when the needle is found, it is part needle part hay to be removed; yet the very removal of the hay shows the existence of a proof idea, an idea to be hit upon by trial and error. It is this proof idea, incidentally, that allegedly the mathematical student learns to appreciate when he acquires an ever-increasing number of proofs. If so, says George Poly, we can teach him from the start both proof ideas and the method of trial and error by which he may learn to hit upon a successful proof idea himself – first a known one, when he is a true apprentice, rather than a passive student, and then a new one, when he is a novice.

Suppose all this is true. Can we, then, explain the method of mathematics, so as to make a success more understandable than drawing a rabbit out of a hat? Not quite. We may, perhaps, explain one proof-idea but each proof has a different one. And so, Polya's idea of letting students discover proofs for themselves is too hard to execute, though his intentions are perfect.

Such was the state of the understanding of methods of mathematics when Lakatos came on to the stage, with a bow and an expression of gratitude to Polya, while adopting Popper's philosophy to his end; he achieved his end and threw things into a state of havoc.

The relation between Polya and Lakatos is the same as between Whewell and Popper (even though none of the other three knew of Whewell; likewise Lakatos learned about Popper only after his work was begun). All four believe in trial and error, in deductive explanation, in starting with problems, with the search for explanations. Yet, whereas Whewell and Polya believe in verification, and see the corpus of (scientific or mathematical) knowledge as the set of successful trials, of verified explanations, Popper and Lakatos are fallibilists and view refuted theories as part and parcel of our heritage.♣

Lakatos' masterpiece deals with mathematics, not physics. So his refutations are not physical; they are potential counterexamples; at times, but rarely and gratuitously, they are even actual. The task is to look for them; proofs facilitate refutations as they specify conditions which we may violate in our search for counter-examples. This makes us able to stretch and shrink concepts, as well as to criticize theorems as at times too narrow – not covering all cases of the theorem – and at other times as too wide – covering counter-examples (whereas actual counter-examples in physics only prove a theorem too wide; its being too narrow only leads to a quest for a further or a better explanation). In a manner not quite clear to readers of Lakatos, a theorem's transformation and transfiguration may end up in formalization. In a sense formal systems are end-products. But only in a sense; formalization does not end the process of proofs and refutations since we can always ask, is the formal system the same as the preformal one, or does it have an unintended model?

So much for the view of Lakatos. He claims that he is the first to give a theory about the role of proof in mathematical research. Before him people saw a proof as closing an issue; he saw it as a part of the process of the growth of mathematics. Anyone who reads his masterpiece must notice that the very fact that the proof procedure is a long process of fumbling, of trying again and again, makes the growth of a proof idea reasonable, especially intriguing and human – as opposed to being an act of magic – in that it is far from perfect and we poke holes in it and see in its strong points and in its weak points matters of great insight and an increased interest. It is the claim that proof-ideas are essentially correct, even if they need a correction or a simplification, that leads to the attempt to offer a simplified condensed version, and thus, further, according to Lakatos, to an appearance of magic. Only if we appreciate errors enough to incorporate some obvious errors into the dynamic growth of a proof-idea do we offer our audiences those details of the making of the act which makes a magician utterly unable to be surprised when he sees a colleague use tricks he himself knows so well and draws rabbits out of top-hats galore.

4. CAN LAKATOS' METHOD BE APPLIED?

If I were a mathematics teacher, and asked whether I should emulate the Lakatos method of Utopian teaching, I should hesitate. I would not hesitate if I were asked to teach mathematics to non-mathematicians, or if I were to teach whatever I do teach (since in fact I exhort my students to break the equestrian and asinine habit of hard-work and passivity in preference for intelligent work – and the two only seldom overlap} As a mathematics teacher teaching, say, topology, I should have to cover the material which Lakatos discusses in his 'Proofs and Refutations' in the space of a fraction of one lecture. I would have no time at all for his details. Oh, I would gladly advise my students to read Lakatos on a weekend or during a vacation or as relaxing bedtime reading. Still better, if my mathematics students can take credits in either the history of mathematics or in the philosophy of mathematics, then I would gladly propose Lakatos as their major text. But the cruel fact is that Lakatos' very lengthy, discussion ends more or less where the modern textbook of topology begins, to wit with Poincaré's algebraic version of the Descartes-Euler theorem. What shall we do about advanced topology? Can I present it a la Lakatos? Do I have the time to do it within a prescribed semester course? Do I know how?

Let us not be finicky. Let us assume that in a small seminar of advanced mathematics, where we are going over raw new material, we can make better use of the deliberation and fumbling of recent students of the field in order to see how they work and not be puzzled to see them pull rabbits out of hats. Let us also agree that if we do not have to move quickly and cover a lot of ground, we can, indeed, use the Lakatosian method at leisure. Let us also observe, as a matter of fact, that Lakatos did succeed to make non-mathematicians partake in his Utopian mathematical discussions and reproduce historical cases – with the aid of the teacher, of course.

This last point signifies much. It shows that students' interruptions need not be any impediment; that, erroneous as they are, they help dispel the impression that mathematicians draw rabbits out of top-hats.

Let me stress all this. The general view of the matter is that students' participation is of low quality and so at best a necessary evil. And the minority view – the view known as the discovery-method of teaching – is that students' participation is or should be of high quality, the highest indeed, if only certain conditions are met But, no doubt, the discovery method is an unrealizable dream: students cannot possibly be systematically so good as to emulate the best minds and the best results in a given field, no matter what that field is. In any case, I insist, whether students are of high quality or low quality matters little, since we want to help them raise their quality such as it is – indeed we should worry more about the low quality ones. The fallibilist view that the students' interruptions, however low quality, can be used in class – this view is thrilling because it makes the question of quality superfluous. It assumes that students do fail, and do need help to learn what to do about their failure, how to improve performance, where to find some solution which they have overlooked, and how to try to assess these and perhaps even transcend them. There is one hard question: can we assume that we can pursue such a line of activity for a whole university course? I do not think so.

Let me also concede that in certain junctions even small doses of the Lakatos technique may be added to traditional courses with some excellent results. Let me take an unpublished example of Lakatos. The foundation of the calculus taught in a rough and ready manner to un-comprehending students is very hard. It is much better to tell them something about the early calculus, say Newton's; Berkeley's criticisms; attempts by diverse writers to answer him, culminating with Weierstrass. This can be even read into Bell's most conventional and wrong-headed history. But, says Lakatos, there is much more to it. Let us look closely at Cauchy. You take a Cauchy series, and you prove that it converges. You have a criticism: that it converges assumes a convergence point to converge into: is there one for every Cauchy series? Comes Weierstrass and says, yes if we identify the series with the point. I need not explain how revealing this example is, especially of the historical proximity of Cauchy and Weierstrass.

Can we, however, do this for a whole course of mathematics; present a body of mathematical knowledge as series of proofs and refutations? Is Lakatos right? In other words, is Lakatos' view of mathematics comprehensive?

I think not.

5. MATHEMATICAL SYSTEMS AND SUB-SYSTEMS

Lakatos himself was aware of the fact that his own researches presented not a total view of mathematics and its development but first and foremost a criticism of all prevalent views – since these were invariably verificationists of one kind or another – and second a tentative view of mathematics which, when viewed as comprehensive is found wanting, exactly in the way that Popper's view of physics is.

I do not think Lakatos ever developed a comprehensive view of mathematics and of its history. I do not think he even had an idea that satisfied him about the way comprehensive views of mathematics have interacted with the growth of mathematics. He wanted to rewrite his '*Proofs and Refutations*' in a manner that would include a comprehensive view. He never did.

Let me take an example. Lakatos enthusiastically endorsed Russell's thesis (*Foundations of Geometry*) that nineteenth century mathematics is largely the outcome of a response to Kant's comprehensive view of mathematics. I have myself commented on this point elsewhere and shall not go into it now, except to say that Lakatos wanted to incorporate such facts as significant factors in his view of mathematics as a whole, yet he never did. In some place he even declared that formalism, his pet enemy, had a role to play in the history of mathematics. But he never put this into a comprehensive framework.

But let me touch upon less comprehensive instances. I am loath to take cases of any established mathematical truths – seemingly or in truth – as I remember how Lakatos fought like a lion when presented with these. He either disproved them, or claimed that his interlocutor was offering the latest modification of a theorem, designed to overcome a recent refutation. But one example, I think, he did concede and had to concede. The example is the field of ordinary differential equations. You can, of course, say that it is not so much pure mathematics as applied mathematics. This will raise a host of important questions – more questions that Lakatos' early demise prevented him from taking up as he intended to do. So let us not go into that.

As a student I found this field particularly irksome, as I suppose – though I could not articulate it then – a book will be found irksome if each of its paragraphs makes sense but as a whole it makes none (e.g. a book by L. Wittgenstein). But putting it into its context, offering its problems and methods, makes the field of differential equations eminently lucid and sensible.

Differential equations are puzzles or riddles. We cannot find answers to the puzzles, but we can guess them, try each guess and refute or, on occasion, verify it. This makes the field eminently in accord with Whewell and with Polya. It does not accord with Lakatos, since refutations of blind guesses in this field lead nowhere, but verifications are true successes. To prove this perhaps we need proofs of existence theorems and of uniqueness or generality of solutions or of forms of solutions (uniqueness up to a constant, or up to the product or sum or such of some unction or another). Yet, whether existence and uniqueness theorems are above criticism or not, clearly, when we solve a problem set by a differential equation by guessing what the function may be, differentiating and substituting, and arrive at the set equation, then we have solved the problem even though perhaps not uniquely or generally enough. That much, but not much more, Lakatos did concede. Moreover, we are taught a few useful tricks such as guessing a transformation of the variable(s), which just might transform an unsolved equation to a solved one; and of such tricks we can say we do not know why they work but many people tried many tricks and some are indeed successful All this is as plain as your nose and quite outside Lakatos' concept of mathematics; his concept is too narrow. Moreover, the idea of transforming differential equations has very wide extensions in modem mathematics, which have axiomatic systems based on similar general ideas though within them the Lakatos method may well be very useful and enlightening.

This includes category theory. Indeed, the natural way to introduce both category theory and a specific category (with respect to a specific composition rule) is axiomatic. On the whole, since mathematicians these days all too often introduce abstract entities axiomatically prior to investigating them, the role of axiomatics has radically changed. It is well-known, of course, that in the nineteenth century axioms ceased to be self-evident and competing axiom systems for geometry and the different geometries studied. But these geometries were seen as having their own characteristics, depicted axiomatically or otherwise – especially since Klein showed embedding to be a general way of presenting geometries. When Hilbert introduced his meta-mathematics and theorems became objects of a different kind, and deduction became an operation that took its own quality rather than the way to show – in a way developed by Hilbert himself! – that the characteristics of a geometry are indeed successfully depicted by the axioms. Nowadays the axioms do not depict the characteristics of a geometry (a category, an algebra, a space with a topology, etc.) but, in abstract cases of some sorts, the axioms generate the system. We have here, in this short outline quite a few views of axiomatics and these need much further study. Lakatos merely repudiated the Hilbertian one.

What Lakatos had to say about the choice of axioms, of such systems or of geometry, was largely negative. We start not with axioms but with problems, theorems, proofs, refutations, he said. We axiomatize a system only after its concepts were quite sufficiently knocked into shape – he gave no criterion - and often systems are not axiomatized or only quasi-axiomatized. Yet the fact is that often a new field springs into being almost axiomatized almost from the start, not struggling towards its axioms as the calculus or as the theory of probability did.

Let this be. How do we get axioms (choose axioms or move towards them)? By trial and error, of course. But what do we aim at? There j proofs and refutations systems do not make. We have peculiarities of systems, and Lakatos says nothing about them.

Take a trite example that also troubled me as a student In the calculus we postulate the existence of divergence of functions, and we allow our variable(s) to vary from minus to plus infinity. Yet we postulate one point at infinity. In affine geometry we postulate a line at infinity. This is puzzling. It is more puzzling if we know that measure theory postulates two points of infinity for each variable, but I did not know this then. I asked my math professor why the difference between the calculus and affine geometry. He said, the one projects a sphere on the plane, the other a semi-sphere. I asked why and never got an answer.

I asked a few mathematicians the same question since. They all know the answer but many could not articulate it. The answer is this. The calculus is concerned with well-behaved functions and tends to lump together unpleasant and bothersome exceptions; in affine geometry we are concerned with directions (i.e., complete sets of parallel lines) and each point at infinity corresponds exactly to one direction.

Speaking as conventionalists, mathematicians are bound to view their choices as arbitrary, and so not explain them; speaking as naturalists they feel an urge to prove the correctness of their choices. Lakatos quite rightly rejected both of these philosophies and wanted problem orientation or the dialectical view to take over. But he showed no way to explain overall concerns and overall or global problems of mathematical sub-fields. Work in this direction should well suit his general attitude, and it has already started -but is not yet at a stage where we can fully apply the Lakatos method.

6. THE ONGOING REVOLUTION

The feeling that mathematicians, and teachers of mathematics, are pulling rabbits out of hats is rightly disturbing – at least to an apprentice who wishes to know how the master does it and how he can emulate him.

We cannot fully explain this, as there is no systematic way for inventing mathematical ideas – there is no algorism of discovery. Yet much can be done. First, the aim of a given exercise can be explained. Second, attempts to accomplish the aim that look fairly obvious may be presented and criticized These two kinds of steps take much of the mystery away and explain to some extent given discovery, thereby it also offers partial algorisms of sorts, as I have explained elsewhere when examining the parallel situation in empirical science. In mathematics, but not in empirical science, often the partial algorisms playa significant role in that they become subject to investigation in attempts to complete them into algorisms proper.

Lakatos has shown that the formalist view is erroneous, in that what sets of problems we do have algorisms for the solution of we do not much care about, that where the action is matters are fluid and criticism is the daily routine. Nevertheless, here is a complete and unbridgeable break within the system; whereas in physics we have nothing but conjecture and test and no algorism, in mathematics algorisms are an important and an ever-increasing sedimentation.

All this, I feel, require much more study. And the more we cover in the spirit of Lakatos, the more we can teach in class in a dialectical method, as practiced in Lakatos' lovely utopian class.

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♣ In his contribution to the Schilpp Popper volume Lakatos ridicules this view, and opts for a more inductivist philosophy of science. I have expressed my view on his philosophy of science elsewhere; here we come to praise him, not to bury him. Let me just mention one fact that sharply exhibits the overall change in Lakatos' philosophy when he permitted a bit of inductivism in. His later works repeatedly – 3 times – attack the Popperian view (he quotes me on this) that empirical learning from experience is the discovery of counter-examples. This very idea, implicit in Popper, was explicitly stated in Lakatos' own classic 'Proofs and Refutations' (IV, pp. 303-4) as an anti-inductivist stand.