Lakatos, Imre (1922–74)

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Abstract: Imre Lakatos made important contributions to the philosophy of mathematics and of science. His ‘Proofs and Refutations’ (1963–4) develops a novel account of mathematical discovery. It shows that counterexamples (‘refutations’) play an important role in mathematics as well as in science and argues that both proofs and theorems are gradually improved by searching for counterexamples and by systematic ‘proof analysis’. His ‘methodology of scientific research programmes’ (which he presented as a ‘synthesis’ of the accounts of science given by Popper and by Kuhn) is based on the idea that science is best analysed, not in terms of single theories, but in terms of broader units called research programmes. Such programmes issue in particular theories, but in a way again governed by clear-cut heuristic principles. Lakatos claimed that his account supplies the sharp criteria of ‘progress’ and ‘degeneration’ missing from Kuhn’s account, and hence captures the ‘rationality’ of scientific development. Lakatos also articulated a ‘meta-methodology’ for appraising rival methodologies of science in terms of the ‘rational reconstructions’ of history they provide.

1. Life

Imre Lakatos was born Imre Lipschitz in Hungary in 1922. His early life was turbulent even by the remarkable standards of the time. He was a member of the resistance during the Second World War, fortunately evading arrest (unlike his mother and grandmother, both of whom were murdered in Auschwitz). After the war he pursued a political career and by 1947 had become a powerful figure within the Hungarian Ministry of Education; in 1950, however, he was arrested and spent over three years in a Stalinist jail. Informed of the likelihood of rearrest, he fled in 1956 to Vienna, and eventually to Cambridge, where he studied for a (second) doctorate under the supervision of R.B. Braithwaite.

In the course of these studies, he became a regular attender of Karl Popper’s seminar at the London School of Economics, and Popper’s thought and approach had a major influence on him. Lakatos was appointed to a Lectureship at the LSE in 1960, and spent the rest of his life there, being awarded a personal chair in 1970. For unrevealed reasons, the British Home Office rebuffed two impressively supported attempts to obtain British citizenship and Lakatos remained officially ‘stateless’. He died suddenly, from a heart attack, in February 1974. He had a vivid personality, strong political views and a sharp wit: he inspired intense loyalty and opposition in roughly equal measures.

2. 'Proofs and Refutations': contributions to philosophy of mathematics

Lakatos published some philosophical articles (mostly book reviews) before coming to the West, and some commentators discern the influence of his Marxist and Hegelian education throughout his career, but his significant contributions to philosophy were all made after arriving in the West. His doctoral studies at Cambridge eventually formed the basis for his 'Proofs and Refutations' (originally published as a series of four journal articles in 1963–4, and in book form with further material only posthumously in 1976). Perhaps his major work, it consists of an imaginary discussion between a teacher and a group of his (frighteningly bright) students. The first part reconstructs the history of the attempts to prove the Descartes–Euler conjecture about polyhedra (that the number of vertices minus the number of edges plus the number of faces is equal to two for any polyhedron). The real history is told in the many footnotes. A second part of the discussion reconstructs the discovery of uniform convergence as the result of the 'refutation' of one of Cauchy's results. Aside from its philosophical and historical interest, the dialogue is a literary *tour de force*.

Lakatos argued that the standard picture of the development of mathematics is seriously faulty. On that picture, either an assertion is conjectured to hold and after a time a proof of it is produced, or mathematicians simply set out to 'prove' from some agreed axiomatic basis, recording as a 'theorem' any result which they hit upon that happens to interest them. Lakatos suggested that in fact theorems are invariably conjectured ahead of proof, and the proof process is a protracted affair in which initial attempts are criticized and gradually improved, along with the 'theorem' itself. At first this is trial-and-error, involving searches for counterexamples both to the original 'theorem' and to the 'hidden assumptions' that are articulated in the course of initial attempted proofs.

This trial-and-error phase of conjecture followed by undirected search for proof and/or counterexample is eventually superseded, however, by a more systematic phase – that of 'proof analysis'. Lakatos – inspired by his countryman Polya – argued that the process of mathematical discovery is not a 'merely' psychological affair to be studied by trying to delve into the minds of the great mathematicians, but can be shown instead to be governed by articulable heuristic principles. Hans Reichenbach, Popper and the Logical Positivists all saw an unbridgeable divide between – in Reichenbach's terminology – the 'context of justification' and the 'context of discovery'; and all asserted that philosophy or logic of science is concerned only with the former 'context'. Questions of discovery were alleged neither to call for logical analysis nor to be susceptible of it. Lakatos argued that, in the case of mathematics at least, this view is importantly mistaken, and that there is a realm of logically analysable, mathematical heuristic outside the two traditional 'contexts'. This claim forms an important link with Lakatos' later contributions to the philosophy of science (see Discovery, logic of §2).

One of the intriguing (but often frustrating) aspects of 'Proofs and Refutations' is that its dialogue form, although used to brilliant effect, sometimes makes it difficult to discern what thesis is actually being propounded: not even 'Teacher' is always right. This has resulted in some obscurity about the underlying view of the nature of mathematical knowledge. Lakatos himself believed that his message was fundamentally Popperian – that he was extending Popper's fallibilism into the area of mathematics. Another view – indicated in the book's editorial footnotes – is that Lakatos simply described (in fascinating detail) the fallible *process* by which essentially infallible, logically true, knowledge is created in mathematics. These editorial footnotes have themselves been attacked as a 'betrayal' of the real 'anti-formalist' message of the book (Davis and Hersch 1981).

3. The 'methodology of scientific research programmes': contributions to philosophy of science

After 'Proofs and Refutations', Lakatos turned his attention to the philosophy of science. Although his 1968 paper 'Changes in the Problem of Inductive Logic' defends a broadly Popperian line against Carnapian 'inductive logic', Lakatos soon came to see major defects in the Popperian approach. 'Popper on Demarcation and Induction' (1974) contains a detailed criticism of Popper's claim to have solved Hume's problem. Popperian corroboration appraisals are simply summaries of how the available theories have stood up to testing *so far*. Those appraisals, therefore, can have no consequences for the comparative future reliabilities of the various theories, nor for the reasonableness of relying on one theory rather than a rival in future applications, unless some assumption is made that – in Popperian terms – links corroboration appraisals to claims about overall 'verisimilitude'. Such an assumption amounts to a reformulation in Popperian terms of a uniformity-of-nature assumption, linking past test results to an overall judgment of the theory's truth-likeness and hence its reliability in past *and* future tests. That (merely posited) principle has, therefore, seemed to most commentators much stronger than a mere 'whiff' of induction, as Lakatos represented it.

Although argued in a new way, Lakatos' point here is similar to arguments against Popper already produced by Reichenbach, Ayer, Salmon and others. Lakatos' views on 'demarcation' were altogether more innovative. Thomas Kuhn had pointed out (1962 and elsewhere) that many aspects of the development of science seem at direct odds with Popper's falsificationist account. For example, the typical response of a theoretician to an experimental 'refutation' of his favoured theory is not, as Popper seemed to suggest, to reject that theory and look for an alternative, but instead to treat the experimental result as an 'anomaly' which could and should be accommodated within the theory. Lakatos saw his 'methodology of scientific research programmes' (further developed and defended in his 1970 paper) as a synthesis of the views of Popper and of Kuhn. He agreed with Kuhn that the correct unit of analysis in science is much broader than that of a single theory. A Lakatosian research programme is characterized by a 'negative heuristic' principle specifying its 'hard core' (the set of basic propositions that will be implied by every theory that issues from it), and by a 'positive heuristic' (a set of directives, possibly deriving from some broad metaphysical principle, governing the construction of specific theories within the programme, and governing their modification in the light of experimental difficulties that may arise). Specific theories produced by a programme *are* experimentally refutable (or would be if fully articulated), but the standard response to an actual refutation will be to look for a further specific theory within the same programme. Since this successor theory will also entail the same central ('hard core') claims, this process will seem like 'holding onto' a theory by 'modifying' it in the light of experimental difficulties, rather than rejecting it.

This sounds like Kuhn's idea of 'articulating the paradigm' and treating experimental difficulties as 'anomalies'. According to Lakatos, the main problem with Kuhn's account is that it seems not to be able to explain scientific change as a rational process. What distinguishes a proper, scientific further articulation of a paradigm or programme from a defensive nonscientific one? If it was good scientific practice for Newtonians to defend their basic 'paradigm' or 'hard core' against their failure to explain Uranus's orbit by postulating a hitherto unknown planet, was it not equally good scientific practice for phlogistonists to defend their theory – that combustion always involves release of phlogiston – against the fact that burning mercury in air produces a heavier 'ash' by postulating negative weight for phlogiston (or by postulating that burning mercury involves both the release and the absorption of material)? A second, related problem is that the distinction between real science and pseudoscience seems to be endangered by Kuhn's account. Kuhn seems committed to the view that there are no articulable 'logical' rules of good science or of correct response to evidence. Priestley was not 'wrong or unscientific' to hold on to the phlogiston theory, he was simply outvoted.

Lakatos argued that there is in fact a clear-cut distinction here – one based on the old idea of independent evidence, but given a new slant. The difference between, for example, the Newtonian shift and that involved in defending phlogistonism is the difference between a 'progressive' and a 'degenerating problem shift'. The postulation of a further planet to explain the anomalous motion of Uranus within Newtonian theory not only solved the problem of Uranus's motion but also led to further independently testable predictions – the new planet could after all be observed. On the other hand, the shift in phlogistonist assumptions simply *at best* resolved the known anomaly, while making no further testable predictions.

Lakatos was always interested in the relationship between the philosophy of science (and mathematics) and its history and felt that philosophy of science in the post-war period had suffered from its paying too little attention to actual scientific practice. In his 1971 paper ‘History of Science and its Rational Reconstructions’, he proposed a general method for the evaluation of rival philosophies or methodologies of science in terms of the ‘rational reconstructions’ they provide of the history of science and especially of historical episodes of major changes in accepted theories. His basic idea was that there is a range of historical cases in which, speaking intuitively, the ‘scientifically correct’ decisions were clearly made (for example, the acceptance of Newtonian theory or of Maxwell’s theory). A methodology should endorse (and so give a general explanation of) these cases; and should, in cases where it implies that the ‘wrong’ decision was made, be able to point to independent evidence for the operation of ‘external factors’ (political or religious interference, for example). He argued that his own methodology did better on these terms than other accounts, such as inductivism, conventionalism or Popperian falsificationism.

List of works:

Lakatos, I. (1963–4) ‘Proofs and Refutations’, The British Journal for the Philosophy of Science 14: 1–25, 120–139, 221–243, 296–342; repr. with editorial footnotes in Lakatos 1976. (The original published version of Lakatos’ major work in the philosophy of mathematics.)

Lakatos, I. (1968) ‘Changes in the Problem of Inductive Logic’, in I. Lakatos (ed.) The Problem of Inductive Logic, Amsterdam: North Holland; repr. in Lakatos 1978. (Lakatos’ account of the dispute between Carnap and Popper over probabilistic, inductive logic.)

Lakatos, I. (1970) ‘Falsificationism and the Methodology of Scientific Research Programmes’, in I. Lakatos and A.E. Musgrave (eds) Criticism and the Growth of Knowledge, Cambridge: Cambridge University Press, 1970; repr. in Lakatos 1978. (The definitive account of Lakatos’ views on scientific research programmes and their development.)

Lakatos, I. (1971) ‘History of Science and its Rational Reconstructions’, in R.C. Buck and R.S. Cohen (eds) PSA 1970, Boston Studies in the Philosophy of Science 8, Dordrecht: Reidel; repr. in Lakatos 1978. (This paper contains Lakatos’ views about ‘meta-methodology’: how to evaluate rival theory-appraisal criteria.)

Lakatos, I. (1974) 'Popper on Demarcation and Induction', in P.A. Schillp (ed.) The Philosophy of Karl Popper, Library of Living Philosophers, La Salle, IL: Open Court; repr. in Lakatos 1978. (Lakatos' appeal to Popper for a 'whiff of induction' and a systematic comparison of his views on science with those of Popper.)

Lakatos, I. (1976) Proofs and Refutations: The Logic of Mathematical Discovery, ed. J. Worrall and E. Zahar, Cambridge: Cambridge University Press. (This book contains extra material on Cauchy and uniform convergence as well as the original 1963–4 papers.)

Lakatos, I. (1978) The Methodology of Scientific Research Programmes: Philosophical Papers vol. 1 , ed. J. Worrall and G. Currie, Cambridge: Cambridge University Press. (Volume 1 of Lakatos' collected papers.)

Lakatos, I. (1978) Mathematics, Science and Epistemology: Philosophical Papers vol. 2 , ed. J. Worrall and G. Currie, Cambridge: Cambridge University Press. (Volume 2 of Lakatos' collected papers, including some hitherto unpublished material.)

References and further reading:

- Davis, P.J. and Hersh, R. (1981) The Mathematical Experience, Brighton: Harvester. (Contains an endorsement of some of Lakatos' views on mathematics and criticism of the editorial footnotes in Lakatos 1976.)
- Hacking, I. (1983) Representing and Intervening: Introductory Topics in the Philosophy of Natural Science, Cambridge: Cambridge University Press. (Contains an important chapter on Lakatos' methodology of research programmes.)
- Howson, C. (1976) Method and Appraisal in the Physical Sciences, Cambridge: Cambridge University Press. (Contains several 'case studies' of important episodes of theory-change in the history of science analysed from the point of view of the methodology of scientific research programmes.)
- Kuhn, T.S. (1962) The Structure of Scientific Revolutions, Chicago, IL: University of Chicago Press, 2nd edn, 1970. (The most influential contemporary account of theory-change in science.)
- Lakatos, I. and Musgrave, A.E. (1970) Criticism and the Growth of Knowledge, Cambridge: Cambridge University Press. (Contains an outline of Kuhn's account of science, together with a range of commentaries and Kuhn's replies.)

- Newton-Smith, W.H. (1981) The Rationality of Science, London: Routledge. (Contains a sympathetic, but critical analysis of the views of Lakatos and of related authors on scientific progress.)
- Zahar, E.G. (1989) Einstein's Revolution: A Study in Heuristic, La Salle, IL: Open Court. (A critical account of the methodology of scientific research programmes, including a significant elaboration of the idea of rationally analysable heuristic, together with an extended case study of the relativistic revolution.)