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IMRE LAKATOS (1922–1974): PHILOSOPHER OF MATHEMATICS AND PHILOSOPHER OF SCIENCE

Through the sudden death of Imre Lakatos on 2 February 1974 the intellectual world has lost not only an important and influential philosopher but also an exceptional human being.

His life reflects in one way or another many of the major events in recent European history. He was born in Hungary in 1922. He was a member of the anti-Nazi resistance, fortunately evading arrest, unlike his mother and grandmother both of whom were killed in Auschwitz. (During the Nazi occupation of Hungary he changed his name from the patently Jewish Imre Lipschitz to the safer Imre Molnár. After the war he was, however, reunited with a set of his shirts monogrammed 'I.L.'. Faced with this major problem (shirts like most other things were in short supply) and now a devoted communist, he again changed his name to the more working class Imre Lakatos.) In 1947 he became a high-ranking official in the Hungarian Ministry of Education, but, never a man to bow to authority, his 'revisionist' tendencies soon got him into trouble. In 1950 he was arrested and spent over three years in a Stalinist jail. After the Hungarian uprising in 1956 he was informed of the likelihood of his re-arrest and he fled to Vienna. From there he went eventually to Cambridge where his academic career began in earnest.

As well as his intellectual legacy, he left behind him at the London School of Economics (where he taught from 1960 until his death) fond memories and a fund of well-remembered stories and jokes. He embellished the English language (at least as it is spoken in the Philosophy Department at the L.S.E.): he turned 'thinking aloud' into 'thinking loudly' and the body of accepted scientific theories into the 'body scientific'. He also, in one of his seminar papers, accused a prominent Wittgensteinian, who had recently produced an enormous tome, of committing an unforgivable 'book act'. The one lesson above all others that his students learned (by example) from him was that serious scholarship can be fun.

Lakatos made important contributions to philosophy. His first love,

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once he had turned to the West and to academic life, was the philosophy of mathematics. His Cambridge Ph.D. Thesis: 'Essays in the Logic of Mathematical Discovery' became the basis of his 'Proofs and Refutations'.[1] This paper takes the form of an imaginary discussion between a teacher and a group of his students which 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. This paper (which as well as having great philosophical and historical value is a superb literary piece) was circulated in offprint in enormous numbers, but although he had a long-standing contract for its publication in book form, Lakatos characteristically withheld it in the hope of improving it still further.[2]

The thesis of 'Proofs and Refutations' is that the development of mathematics does not consist (as conventional philosophy of mathematics tells us it does) in the steady accumulation of eternal and undeniable truths. Mathematics develops, according to Lakatos, in a much more dramatic and exciting way – by a process of conjecture, followed by attempts to 'prove' the conjecture (i.e. to reduce it to other conjectures) followed by criticism *via* attempts to produce counter-examples both to the conjectured theorem and to the various steps in the proof.

An important theme of this work is the claim that by criticising proofs ('proof analysis') mathematics very soon progresses beyond the naive trial-and-error stage of fortunate conjecture followed by undirected search for a counter example. Lakatos in fact argues that there is such a thing as mathematical *heuristic*, which is susceptible of rational analysis; in other words that the process of mathematical discovery is not simply a non-objectively analysable affair to be studied by trying to delve into the psyches of the great mathematicians. Both Popper (whom Lakatos joined at the L.S.E. and who influenced Lakatos considerably) and the logical positivists had accepted the distinction between questions about the *discovery* of scientific theories and questions about the *justification* of ready-articulated scientific theories. These philosophers claimed that philosophy was concerned solely with questions of the latter kind. Questions of the former kind were alleged to be purely psychological questions about individual scientists' thought-processes and so were claimed 'neither to call for logical analysis nor to be susceptible of it'.[3]

Lakatos (inspired here by Polya) argued that there exists a realm of rational mathematical heuristic between these two realms. This became an enduring theme of his work and was, as we shall see, carried over into his philosophy of the natural sciences.

Implicit in 'Proofs and Refutations' is a new approach to the philosophy of mathematics completely transcending the three 'foundational' schools of logicism, intuitionism and formalism, which despite known difficulties have so far dominated 20th century philosophy of mathematics. In a 1962 paper called 'Infinite Regress and the Foundations of Mathematics',[4] Lakatos placed Russell's logicism and Hilbert's formalism in a more general epistemological framework and extended the sceptics' arguments against foundations of knowledge into the sphere of mathematics.

Lakatos showed in this paper how one of the traditional ways of attempting to justify some branch of knowledge has been to try to find some indubitably true 'first principles', containing only 'crystal clear' terms, from which the whole of that branch of knowledge is derivable *via* the infallibly truth preserving rules of deductive logic. Any such enterprise Lakatos called 'Euclidean'. He showed how the logicist programme of Frege and Russell is a supreme example of such a Euclidean enterprise and how Hilbert's formalist programme falls essentially in the same category. He traced the development of these two programmes and showed how the response to difficulties within both has been one he calls 'Rubber Euclideanism'. This consists of stretching the notions of a 'crystal clear term' and of an 'indubitable *a priori* truth' so as to include precisely those terms and those 'truths' required to get round the difficulties. Lakatos argued that all such Euclidean programmes are doomed to failure even within mathematics – Euclideanism's last stronghold; and that mathematics is in need of foundations no more than are the physical sciences.

This paper together with 'Proofs and Refutations' and another important paper called 'A Renaissance of Empiricism in the Recent Philosophy of Mathematics?' (only a part of which has so far been published[5]) sets out a new philosophy of mathematics. This philosophy recognizes Euclideanism as utopian, but does not on this account embrace the despairing claim that there are no objective standards of acceptability in mathematics or that which proofs and which axioms are accepted is at best a question for aesthetics. Rather this philosophy proposes that growth in mathematics is controlled by objective standards no less than is growth in the physical sciences, and that there can, therefore, be good growth, or progress, and bad growth, or degeneration, in mathematics just as there is in physics.

Lakatos still felt that his philosophy of mathematics needed further improvement and development, but before he could supply them, circumstances turned his attention to the philosophy of science. He was asked to write a paper on Popper's philosophy. Lakatos had so far regarded himself as extending Popperian fallibilism into the domain of mathematics but he now critically scrutinized Popper's philosophy itself and found within it some open problems. Lakatos also agreed to organize an international colloquium on philosophy of science which was held in London in the summer of 1965. Having organised this conference with great success, he edited its proceedings in four volumes.[6]

The major intellectual outcome of all this activity was a series of four important papers. The first of these is called 'Changes in the Problem of Inductive Logic'.[7] It critically analyses the debate between Carnap and Popper concerning the relations between scientific theories and evidence. It charts the development of the two approaches and argues that Carnap's approach solved no philosophical problems except ones of its own creation. It also argues the importance of the switch in the Popperian programme from concentrating simply on a theory's testability to requiring that a theory have *independent* or *excess* testability over its rival theories. Lakatos argued that scientific theories can *only* be corroborated by successfully predicting the outcomes of *independent tests* (i.e. tests the outcome of which is not also predicted by rival theories). He pointed out that this makes the question of whether or not a theory is corroborated by a piece of evidence depend on what rival theories are around when the corroboration appraisal is made and hence gives corroboration a historical character.

Lakatos also showed in this paper that while Popperians and others were correct in their attribution of metaphysical synthetic *a priori* assumptions to Carnap and other inductive logicians (for example ascriptions of a specific value to Carnap's λ parameter reflect metaphysical assumptions about the degree to which nature is uniform), the postulation of a weak metaphysical 'inductive principle' is necessary also within the Popperian system. A statement ascribing a degree of corroboration to a scientific theory given certain evidence is, for Popper, analytic. It simply records the extent to which the theory has *in the past* stood up to 'severe' testing. Lakatos persuasively argued that the choice of the best corroborated theories for use in technological applications hence involves an assumption that a theory's past performance is a guide to its future performance.[8]

The outline of Lakatos' major contribution to the philosophy of science appear already in this 1968 paper, with its emphasis on scientific growth rather than on falsification and its use of the notions of progressive and degenerating problem-shifts. But his methodology of scientific research programmes is developed in detail only in his 1970 paper on 'Falsification and the Methodology of Scientific Research Programmes'.[9]

Several philosophers and historians of science had pointed to certain typical kinds of development in science which do not cohere well with the falsificationist model of scientific rationality. It was pointed out (for example by Kuhn) that the typical response of a theoretician to an experimental 'refutation' was *not* to reject the theory but to retain it whilst trying to modify the auxiliary and observational assumptions involved in the 'refutation' in the hope of explaining it away as merely 'apparent'. Lakatos himself documented several cases where a theory which is alleged to have been defeated in a 'crucial' experiment was in fact developed by its protagonists so as to keep up with its rival for some time after the supposedly crucial result.

The methodology of research programmes is the result of Lakatos's creative development of two discoveries. First the discovery that falsificationism could be developed so as to deal with these apparently anomalous aspects of scientific development; and secondly the discovery of the critical role played in science by the heuristic principles whose importance for mathematics Lakatos had already stressed.

According to this methodology the basic unit of scientific discovery is not an isolated theory but rather a research programme. Such a programme, developing under the guidance of its heuristic, issues in a series of theories. Each such theory though it may contain an irrefutable ('metaphysical') part, will be refutable, but the typical response of the proponent of the programme to an experimental refutation will be to amend his theory – leaving certain assumptions (the 'hard core' of the programme[10]) unchanged, whilst replacing other ('auxiliary') assumptions.[11] (But the auxiliary assumptions are changed not only under the pressure of anomalies but also (and more importantly in the case of the best research programmes) under the guidance of the heuristic.) There will in general be rival research programmes in any field and it may be (and generally is) the case that the latest theory produced by each of these programmes is inconsistent with accepted experimental reports (i.e. 'refuted').

But if even the best scientific programmes are always in experimental difficulties and if their proponents are allowed to elaborate and amend their assumptions rather than give them up in the face of experimental difficulties, what distinguishes one programme from a better one? And what distinguishes the 'best' science from apparently pseudo-scientific programmes, like the Freudian and Marxist ones, whose proponents seem to defend them in precisely this way? Lakatos showed that the distinction is one between 'progressive' and 'degenerating' research programmes. Given any finite set of anomalies to, or refutations of, a theory it will in general be trivially easy to construct modified auxiliary assumptions which deal with these anomalies. Many such modifications will, however, be *ad hoc*; only a few will, on the contrary, have extra predictive power over the original theory, and even fewer will have their extra predictions empirically confirmed. Newton's programme, for example, produced theories which not only dealt with some of their predecessors' anomalies but also correctly predicted new facts; it was therefore progressive. The Cartesians, on the other hand, managed to incorporate the Newtonians' successes within their programme but only in a *post hoc* way and without at the same time predicting anything new (and hence this programme degenerated).

Lakatos developed his views on how historical case-studies can be used as a source of criticisms of philosophies of science in his paper on 'History of Science and its Rational Reconstructions',[12] where he attempts to give substance to his (increasingly famous) paraphrase of Kant: 'Philosophy of science without history of science is empty; history of science without philosophy of science is blind.' He had always been eager (as befits an ex-Hegelian) to bring the philosophy both of mathematics and science closer to their histories. He now became in this paper the first to propose a general method for the evaluation of rival methodologies in terms of the 'rational reconstructions' of the history of science they provide and of the historical accuracy of these reconstructions. This meta-methodological criterion synthesized, he argued, the *a prioristic* approach to methodology (which claims that there are immutable *a priori* general rules for scientific appraisal) and the antitheoretical approach to methodology (which claims that there are no general standards of appraisal and that all we have to go on is the scientific élite's instinctive decisions in individual cases).

Lakatos was a master of the methodologically motivated study of specific historical cases (a subject which, according to Feyerabend, he turned into 'an art form'). His last publication, a joint paper with his colleague Elie Zahar, was such a case-study. It argues that amongst available methodologies only the methodology of scientific research programmes can explain the Copernican revolution as consisting of the replacement of one theory by an objectively better one without distorting the historical facts.[13]

Imre Lakatos leaves behind him a mass of so far unpublished material and a set of thwarted plans to reply to some of his critics (like Kuhn, Feyerabend and Toulmin, for Lakatos's methodology had become one of the focal points of debate in philosophy of science) and eventually to apply his methodological ideas to other fields. Fortunately he also leaves behind him (and it was of this achievement that he was most proud) a thriving research programme manned, at the London School of Economics and elsewhere, by young scholars engaged in developing and criticising his stimulating ideas and applying them in new areas.

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NOTES

[1] This was published in four parts in *The British Journal for the Philosophy of Science*, in 1963–4.

[2] The piece has also occasionally been performed by groups of mathematics students in the U.S.A. It was also 'pirated' in the Soviet Union where it is apparently a best-seller. (*Proofs and Refutations* is finally to appear in bookform in 1976, published by Cambridge University Press.)

[3] Popper, *Logic of Scientific Discovery*, p. 2.

[4] Published in *Proceedings of the Aristotelian Society, Supplementary Volume* **34** (1962).

[5] This part is in Lakatos (ed.), *Problems in the Philosophy of Mathematics*, 1967.

[6] *Problems in the Philosophy of Mathematics*, 1967; *The Problem of Inductive Logic*, 1968;

Problems in the Philosophy of Science, 1968; and *Criticism and the Growth of Knowledge*, 1970. (The last two works were edited jointly with Alan Musgrave.)

[7] This paper appeared in *the Problem of Inductive Logic* volume.

[8] This argument is also further developed in Lakatos's paper 'Popper on Demarcation and Induction', in P. A. Schilpp (ed.), *The Philosophy of Karl Popper*, earlier published in German as 'Popper zum Abgrenzungs– und Induktionsproblem' in H. Lenk (ed.), *Neue Aspekte der Wissenschaftstheorie*, 1971.

[9] This was published in the *Criticism and the Growth of Knowledge* volume; a briefer and less fully argued account of the methodology had already appeared in the *Proceedings of the Aristotelian Society* **69** (1968).

[10] The 'hard core' of the Newtonian programme, for example, consisted of the three laws of motion and the law of universal gravitation.

[11] Lakatos called the set of auxiliary assumptions (which included in the Newtonian case theories of optics and in particular of atmospheric refraction) a programme's 'protective belt' since it protects the hard core from refutation.

[12] This paper forms part of Buck and Cohen (eds.), *Boston Studies in the Philosophy of Science*, vol. VIII, 1971. See also his 'Replies to Critics' in the same volume.

[13] "Why did Copernicus's programme supersede Ptolemy's?" in R. Westerman (ed.) *The Copernican Achievement*, 1976.