**Imre Lakatos[[1]](#endnote-1)**

1. **Introduction**

In the 1988 American comedy film – which blended cartoons and live acting – the voluptuously-drawn wife of Roger Rabbit, Jessica, was given the memorable line: ‘I'm not bad; I'm just drawn that way.’ One might be tempted to say something similar about a notable teacher of mine at the London School of Economics, Imre Lakatos, although with possibly less justification. Lakatos was a most intelligent man, with a huge range of knowledge. He was entertaining and sophisticated, and had a devilish sense of humour. But a problem for Imre was that there was also a diabolical look about him, physically.[[2]](#endnote-2)

I was taught by Lakatos, in the sense of attending his lectures on the philosophy of science, and being a graduate student in the small department in which he was working. He also participated in some notable lectures given by Paul Feyerabend. Lakatos might be described as having heckled these, creatively, while standing between his friend Feyerabend and a chalk-board on the wall at the front of the lecture theatre. On this, Lakatos wrote a critical commentary while Feyerabend lectured.

Lakatos was himself a striking figure. He was a Hungarian, who spoke imaginatively-constructed and heavily accented English. He cultivated a humorous persona of behaving in an amusing and outrageous manner – which it seems to me he sometimes used as a cover for actually behaving in an outrageous manner.[[3]](#endnote-3)

Lakatos had come to England as a refugee from Hungary, during the Hungarian Revolution. He managed to obtain a grant from the Rockefeller Foundation to study for a second Ph.D. at Cambridge. There he wrote a distinctive thesis under the supervision of Richard Braithwaite. What he produced, ‘Essays in the Logic of Mathematical Discovery’, is an imaginative mixture of material on the history and the philosophy of mathematics, from part of which his **Proofs and Refutations** was developed.

In his Ph.D. thesis, Lakatos made acknowledgement to Hegel’s Logic; to the Hungarian writer on mathematical heuristics, George Polya (whose **How to Solve It** Lakatos had translated from English into Hungarian[[4]](#endnote-4)), and also to Karl Popper. It is a matter of contention to what extent Hegelian ideas play a continuing role in Lakatos’s work.[[5]](#endnote-5) There are further complications, in the sense that, in Hungary, Lakatos had been a Marxist, and a Communist Party apparatchik, and he had also been influenced by the important Marxist philosopher George Lukács.[[6]](#endnote-6) In Britain he was a stateless person, and attempts to acquire British citizenship were resisted by the authorities. There has been speculation about whether this related to his earlier activities in connection with the Communist Party, or to his having provided information to the Hungarian authorities after he had been released from political imprisonment prior to the revolution.[[7]](#endnote-7) I will say more about Lakatos’s activities in Hungary, and controversies about it, below. The Hungarian-related material, of necessity, must be based on my reading of secondary sources. My personal knowledge of Lakatos was of having him as a teacher, and of interactions with him when I was Karl Popper’s assistant. I have also undertaken work on the Imre Lakatos papers in the London School of Economics (L.S.E.) archive.

1. **Lakatos at the L.S.E.**

It is my understanding that Lakatos was introduced to the L.S.E. by Joseph Agassi. Agassi was a smart but idiosyncratic Israeli. He had studied physics in Israel, become interested in the inter-relations between physics and metaphysics as discussed for example by Popper in his ‘The Nature of Philosophical Problems and their Roots in Science’,[[8]](#endnote-8) and worked with Popper as his graduate student and assistant.[[9]](#endnote-9) Agassi wrote extensively on aspects of the history and philosophy of science. But he had particularly wide-ranging interests, and striking things to say about many different topics. He was ready to discuss Popper’s ideas, and his own take on them, in many different settings. He seems to have been a particularly influential teacher who was a real inspiration to students who worked with him. I personally found him frustrating, because in my view he did not spell out sufficiently clearly what his views were, or what, in other people’s work, he was discussing. At any rate, Agassi seems to have introduced Lakatos to the group at L.S.E. centred round Popper, and Lakatos would appear to have done substantial work on his Ph.D. thesis while in contact with people at the L.S.E.

The main ideas in Lakatos’s thesis – and in the material from it published as **Proofs and Refutations** – are not easy to sum up. But, briefly, a major concern of Lakatos’s was to offer a striking reconstruction of an episode in the history of mathematics – Euler’s work on polyhedra – in the form of a witty dialogue. Lakatos’s approach stressed informal heuristic issues, but also made of proofs much more a matter of conjecture and refutation than they were, historically, understood to be. (It was interesting that, while one might see what was involved as an extension of Popperian fallibilism to the philosophy of mathematics, it was not clear how happy Popper himself was about such a view if taken as a general approach to the philosophy of mathematics.) It has been argued that the kind of approach that Lakatos took was in fact close to a tradition of work which had been taking place amongst historians of mathematics in Hungary.[[10]](#endnote-10) Lakatos also offered a wider defence of the view that he was taking, in a piece entitled ‘A Renaissance of Empiricism in the Philosophy of Mathematics’.[[11]](#endnote-11) But how far it could be taken, or defended against critics, is a matter for contention. While Lakatos was initially appointed to a lectureship in the philosophy of mathematics at the L.S.E., his interests gradually shifted to the philosophy of science.

An important role in this was played by a conference on the philosophy of science which was held at Bedford College in London, in 1965, of which Lakatos was the organising secretary. The conference is probably best known because one volume of its proceedings, **Criticism and the Growth of Knowledge**,[[12]](#endnote-12) was published by Cambridge University Press, and attracted considerable attention.[[13]](#endnote-13) It centred around issues relating to the philosophy of science posed by the contrasting views of Karl Popper and Thomas Kuhn. Lakatos did not, himself, contribute papers to the conference, but he published two papers in the proceedings, which were written after the conference had taken place.

In one of these,[[14]](#endnote-14) Lakatos offered a hard-hitting critical appraisal of Rudolf Carnap’s ideas about inductive logic. In this, Lakatos made use of a Popperian theme which he was to make very much his own: the tracing of the development of problems and theories over time, and the criticism of different versions of a view as representing a retreat, over time, in what it was claiming, intellectually, in the face of difficulties.[[15]](#endnote-15)

It was in the other paper that Lakatos started to set out his own ideas about the philosophy of science. The starting-point of this work was Lakatos thinking that some of Popper’s ideas about what he referred to as ‘metaphysical research programmes’, and some work by John Watkins (another member of the L.S.E. philosophy department) had referred to as ‘confirmable and influential metaphysics’, could be used as the basis for a ‘Popperian’ response to Kuhn.[[16]](#endnote-16) Over time, however, Lakatos’s views were modified,[[17]](#endnote-17) and it would also seem as if they drew on ideas that he had had about the philosophy of science while revising his ‘Proofs and Refutations’ from his Ph.D. dissertation, for publication in the **British Journal for the Philosophy of Science**.[[18]](#endnote-18)

I was lucky enough to be attending Lakatos’s lectures when he was developing these ideas. His lectures were excellent for a group of able students, in the sense that, rather than giving us the kind of introduction to the subject of the sort that might be presented in a textbook, Lakatos shared with us his current research. His presentation was full of anecdotes, gossip, speculative ideas, interesting asides about the history of thought – as well, it should be said, as rather heavy-handed attempts to flirt with certain of the female graduate students.[[19]](#endnote-19) All this generated a real sense of intellectual excitement, in which he brought one into the middle of work that he was engaged in at the time.[[20]](#endnote-20)

Lakatos, in these lectures, was engaged in the development of his own views, by way of a complex dissection of Popper’s views into three different versions, the third of which Lakatos himself identified with and developed in a distinctive way. It is not clear, however, that Popper’s own views really differed from the ‘third’ view that Lakatos identified[[21]](#endnote-21) – although there were, I think, sufficient substantive differences between Popper’s views and the ideas that Lakatos himself eventually championed, for these to clearly constitute a different position. To make what was going on clear, it might be useful to start with Popper’s ideas, and then to move to Lakatos.

Popper, in his **Logic of Scientific Discovery**, had argued that what characterises scientific ideas is their openness to falsification. At the time when he wrote the original version of this (in 1934), Popper knew personally members of the ‘Vienna Circle’. They typically shared his interests in science, logic and rationality, but were critical of metaphysics. Popper was conscious of holding metaphysical views, and also of the role that some metaphysics had played in the development of science. But he could not, at that point, offer an account of how one metaphysical theory might be rationally preferred to another. During the course of the 1940s and 1950s, Popper was to develop ideas about the way in which different metaphysical theories might serve as what he called ‘metaphysical research programmes’ for the development of science. He also discussed how such ideas could themselves be critically appraised.

It was by way of a reconstruction of this material, which Popper had set out more systematically in his as yet unpublished **Postscript** (of which Lakatos had a copy of the proofs[[22]](#endnote-22)), that Lakatos developed his own ideas.[[23]](#endnote-23) It is worth noting that one issue was Lakatos’s development of a ‘Popperian’ alternative to Kuhn, and then a gradual rapprochement with him. One issue which Thomas Kuhn had raised related to the role of what he called ‘paradigms’ in the development of science. Kuhn used the term ‘paradigm’ in many different ways.[[24]](#endnote-24) But what was particularly striking if one compared his views with those of Popper, is that while Popper stressed the role of criticism, and of people changing ideas in response to it, Kuhn stressed the role of relatively uncritical commitment in scientific education, and of the resistance of scientific ideas to change.[[25]](#endnote-25)

Lakatos was initially attracted to the idea of using Popper’s and Watkins’ work about metaphysics and its influence on the development of science as a way in which one might respond to Kuhn. But he radically re-interpreted such ideas, making use of his three-way split of Popper. Popper’s concern had been with metaphysical ideas which offered a systematic account of what the world might be like: think, here, of atomism, field theories, and Popper’s own ideas about the role of indeterministic propensities. Lakatos instead emphasised the idea that key parts of a prima-facie empirical scientific theory might be held immune to criticism, while the responsibility for empirical problems was diverted elsewhere.

This was, indeed, something that had been noted and discussed by Popper. For when a theory is tested, if it is refuted, something needs to be modified. But it is a matter for conjecture as to what needs to be changed. As Pierre Duhem had argued in **The Aim and Structure of Physical Theory**, many different assumptions may be made in a test – whether explicitly, as background assumptions, or, say, because their correctness was implicitly assumed when constructing pieces of scientific equipment of which use was made in the test. In principle, one might conjecture that any of these was responsible for the problem – and thus that it was one or more of them, rather than the theory explicitly under test, which needed to be modified.[[26]](#endnote-26) This was all to be found in Popper’s work.[[27]](#endnote-27) He discussed these issues in his **Logic of Scientific Discovery**, when discussing ‘conventionalism’: the view that our knowledge was to be understood as the product of such decisions, typically taken with an eye to what would form the simplest way in which our experience of the world could be represented.

Popper’s view – in which he was followed by Lakatos – was that modifications of one’s ideas were fine, but should be guided by the aim of making what we are claiming to be as contentful, or bold, as possible. Popper also argued – and in this he was also followed by Lakatos – that there needed to be occasional confirmations of these speculative modifications of our ideas.[[28]](#endnote-28) There were, however, two important differences between Popper and Lakatos in all this.

The first was that Popper was what might be called an aspirational realist. He stressed that our knowledge was always fallible, but urged that we should aspire towards truth, both in science and metaphysics. This meant that, for Popper, criticism of metaphysical ideas, and of the purely theoretical content of other ideas, should be taken seriously. By contrast, for Lakatos, all that was needed was growth in the content of what was being claimed empirically, together with occasional confirmations.[[29]](#endnote-29) It is striking that Lakatos’s Ph.D. student, Spiro Latsis, made use of this feature of Lakatos’s approach in a defence of non-realist aspects of the methodological writings of the economist Milton Friedman.[[30]](#endnote-30)

The second was that Lakatos drew attention to the significance of an aspect of the work of the Nineteenth Century philosopher, William Whewell.[[31]](#endnote-31) Lakatos had had a long interest in heuristics, or the process of discovery, and whether anything interesting could be said about what was involved. Popper had tended to be sceptical about this, seeing discovery as a matter of an interplay between speculative conjecture, and refutation.[[32]](#endnote-32) Whewell’s idea[[33]](#endnote-33) was that one could offer criticism of Cartesianism as a programmatic approach within science, on the following basis. The followers of Newton had been able to develop, in line with their core ideas, many different and impressive scientific advances. The Cartesians, by contrast, simply trailed along behind them, translating the Newtonians’ achievements into their own terminology. This idea of Whewell’s seems to me a really interesting critical tool, and to be a useful addition to a ‘Popperian’ approach.[[34]](#endnote-34)

But what, in the end, was the most significant difference between Popper’s approach and Lakatos’s? I have already indicated that for Popper, one must take criticism of metaphysics and of key theoretical ideas seriously. In terms of the importance of advancing bold theories, and of getting occasional confirmations of them, there is nothing between them. And, when Lakatos stressed the idea that one needed to evaluate the development of theories (and research programmes) over time, to see if they made progress, or retreated in the face of problems, he was developing a key Popperian theme.

There were, I think, three problems about Lakatos’s approach.

The first, as what I have written should serve to indicate, is that Lakatos seemed to me to misrepresent the degree to which his ideas differed from those of Popper. There were, indeed, differences. But they were typically variations on Popperian themes, and many of the things for which he is well-known are already there in Popper’s work if one knows where to look.

Second, and as I recall from hearing Lakatos lecture, Lakatos initially had the belief that, if he could argue that if a research programme was degenerative, this would provide a rationale for its no longer being funded. But – as he in the end admitted – this was incorrect.[[35]](#endnote-35) For there is no reason why a particular programme which has been pursued unsuccessfully might not be revived.[[36]](#endnote-36)

Third, and I again recall this from later lectures of Lakatos’s, and as may be found in his ‘History of Science and its Rational Reconstructions’,[[37]](#endnote-37) he moved on to something rather different. Popper’s approach was avowedly normative – in the sense of a Kantian hypothetical imperative. That is to say, Popper’s argument was: if you have a particular aim for science (about which there could be differences of opinion, and argument), then you should proceed in broadly the following manner…[[38]](#endnote-38) Lakatos ended up claiming that his ideas were, as a matter of fact, implicit in the best science – even if those who had been pursuing it actually thought that they were guided by different methodological ideas. It is not clear if Lakatos was, in this, consciously going back to a Hegelian approach. But there was more than a whiff of the idea of Hegel’s idea of the Cunning of Reason involved; of Reason working itself out through history, by way of the instrument of figures who thought that they were doing something rather different. I recall his being challenged about this by a Marxist student who was attending his lectures, and Lakatos admitting to it. Lakatos’s approach seemed both strange and question-begging (not least, because one might ask: who or what gets to identify the best science?)[[39]](#endnote-39) But it led to Lakatos developing a distinctive approach to the historiography of science, which he encouraged his graduate students to pursue. This would amount to reconstructing the history of science to exhibit that Lakatosian ideas had been implicitly at work, as the history of science unfolded

Indeed, this was accompanied by an exercise which seemed to me to smack of the pre-LSE Lakatos, whom I will discuss in the final section. It related to Thomas Kuhn. Lakatos had set up the session involving Kuhn at the Bedford College conference as a kind of confrontation between a Popperian approach to science, and Kuhn’s. **Criticism and the Growth of Knowledge**, and Lakatos’s own contribution to that volume, had been written very much in that spirit. Since that point, however, Lakatos became much closer to Kuhn, and proposed a couple of conferences in which there would be collaboration between them.[[40]](#endnote-40) The idea, as I recall it from material in the Lakatos archive, would be to put on show work by younger scholars which exemplified commonalities between their approaches. But – perhaps cynically – it looked to me as if what was going on, was Lakatos trying to take advantage of Kuhn’s reputation in the history and philosophy of science, in a setting in which it was Lakatos, rather than Kuhn, who had a lot of graduate students whose work could be put on show.

In the event, Lakatos died before the conferences (and subsequent volumes) appeared, and the volumes display Lakatos’s approach when applied to both the physical sciences, and economics.[[41]](#endnote-41) The conferences were funded by a grant from the Latsis Foundation: Lakatos’s former graduate student was, in fact, the son of a wealthy Greek shipping magnate, the business of whom Lakatos’s student took over, on the death of his own father.

1. **Lakatos in Hungary**

I had known Lakatos simply as a colourful Hungarian-born academic, based in London. But after his death, information gradually came to light about his prior career in Hungary. There have been several studies of this, but what is said is not always consistent. A useful overview is offered in Gábor Kutrovátz’s ‘Lakatos' Philosophical Work in Hungary’.[[42]](#endnote-42)

Imre Lakatos was born into a Jewish family in Debrecen, eastern Hungary, on November 9, 1922, as Imre Lipsitz. (His father, Jacob Marton Lipsitz, managed to survive the Second World War, and emigrated to Australia. Letters to and from Lakatos’s father – who comes across as a bit like a character in one of I. B. Singer’s engaging stories about ageing European Jewish refugees in America – are held by the L.S.E. archive.) Lakatos entered Debrecen University in 1940, and graduated in Physics, Mathematics, and Philosophy in 1944.[[43]](#endnote-43) He ran an illegal Marxist-inspired study group, and subsequently an illegal communist cell. His role in this has become controversial. A young woman who was a member of the group, faced problems with her accommodation (which was typically in houses owned by people hostile to Jews and/or communists). Lakatos, concerned that if she fell into the hands of the authorities she would reveal information about the group, persuaded her to commit suicide rather than risking the group being identified. There has been controversy about the influence, on Imre and the group, of Leninist ideas about leadership and of a Stalinist novel, **Chocolate**, which glorified heroic self-sacrifice for the cause.[[44]](#endnote-44)

After the war Lakatos had a role in the Ministry of Religion and Education, and – in a period of political struggle - was involved in examining university educators for their intellectual compatibility with communist ideology. He was also involved in a Communist Party campaign against the independent and distinguished Eötvös College, for which purpose he became a student there.[[45]](#endnote-45) In 1947 he submitted a doctoral dissertation.[[46]](#endnote-46) Kutrovátz argues that Lukács was an influence on his work in this period, although Long suggests that Lakatos was also critical of Lukács’ ideas.[[47]](#endnote-47) In 1948 Lakatos went, with Party backing, to Moscow to study physics, but was swiftly recalled. What lay behind this is unclear. It would appear that questions were subsequently raised about the death of the young woman; he also took the wrong side in a political disagreement within the Party.[[48]](#endnote-48) Lakatos was expelled from the Party. In 1950 he was imprisoned in a concentration camp, was released in 1953, undertaking some work at the Mathematical Institute and translating Pólya. He also ‘wrote reports to the secret police on certain intellectuals’.[[49]](#endnote-49) He participated in debates at the time of the Hungarian Revolution, arguing for academic freedom, but left Hungary when the Russian army intervened.

1. I would like to thank Ali Paya for helpful comments on an earlier version. [↑](#endnote-ref-1)
2. It is striking that, as documented in Lee Congdon’s ‘Possessed: Imre Lakatos’s Road to 1956’, **Contemporary European History** Nov. 1997, 6, No. 3, pp. 279-94, several people who knew him in Hungary spoke in similar terms about aspects of his conduct there. This theme is also to be found in Jancis Long, ‘The Unforgiven: Imre Lakatos’ Life in Hungary’, in Kampis et al eds., **Appraising Lakatos**, Dordrecht etc: Kluwer, 2002, pp. 263-302. [↑](#endnote-ref-2)
3. It is striking that two people whom he knew well at the L.S.E. as associates of Popper’s, Joseph Agassi and William Bartley were highly critical of how Lakatos treated them. [↑](#endnote-ref-3)
4. Polya, while Hungarian, had written this work in English (he taught at Stanford from 1940) and it was published by Princeton University Press in 1945. [↑](#endnote-ref-4)
5. See, for a brief discussion of this, Alan Musgrave’s discussion of Lakatos in the **Stanford Encyclopaedia of Philosophy**: https://plato.stanford.edu/entries/lakatos/ [↑](#endnote-ref-5)
6. See Laszlo Ropolyi, ‘Lakatos and Lukács’ in George Kampis et al (eds), **Appraising Lakatos**, Dordrecht: Kluwer, 2002, pp. 303–38. [↑](#endnote-ref-6)
7. See, for an account of explanations that have been offered for his imprisonment, Francis Long, ‘The Unforgiven: Imre Lakatos’s Life in Hungary’, in Appraising Lakatos, pp. 263-302. [↑](#endnote-ref-7)
8. See now Popper’s **Conjectures and Refutations**, London: Routledge, 1963 etc. [↑](#endnote-ref-8)
9. Agassi has offered an interesting but not always reliable guide to this in his A Philosopher’s Apprentice: In Karl Popper’s Workshop, Amsterdam and Atlanta: Rodopi, 1993. (I have written ‘not always reliable’ just because Agassi includes interpretative speculations about events which are not always correct.) [↑](#endnote-ref-9)
10. See, on this, András Máté, ‘Árpád Szabó and Imre Lakatos, Or the relation between history and philosophy of mathematics’, **Perspectives on Science**, 14(3), 2006, pp. 282-301. See also Gábor Kutrovátz, ‘Lakatos' Philosophical Work in Hungary’, **Studies in East European Thought**, 2008, 60, No. 1/2, The Sociological

    Tradition of Hungarian Philosophy (Jun., 2008), pp. 113-133 [↑](#endnote-ref-10)
11. First published in Imre Lakatos (ed.), **Problems in the Philosophy of Mathematics**, Amsterdam: North Holland, 1967, pp. 199-202. This, it should be stressed, related to ideas of the kind with which Lakatos was concerned in his thesis, rather than, say, the kind of empiricist view of the status of mathematics defended in John Stuart Mill’s **System of Logic**. [↑](#endnote-ref-11)
12. It was edited by Lakatos and another L.S.E. academic, Alan Musgrave. [↑](#endnote-ref-12)
13. The other three volumes of conference proceedings were published – in expensive volumes – by North-Holland, an academic publisher based in The Netherlands. [↑](#endnote-ref-13)
14. Imre Lakatos, ‘Changes in the Problem of Inductive Logic’, in **The Problem of Inductive Logic, Proceedings of the International Colloquium in the Philosophy of Science**, London, 1965, Vol. II, edited by Imre Lakatos, Amsterdam: North Holland Pub. Co., 1968, pp. 315-417. [↑](#endnote-ref-14)
15. For a rather different appraisal of Lakatos’s paper, see Isaac Levi’s review in **Synthese**, 20, No. 1, (Jun., 1969), pp. 143-8. [↑](#endnote-ref-15)
16. This becomes clear from early versions of Lakatos’s paper, the title of which was ‘Demarcation Criterion and Scientific Research Programmes’, held in the Lakatos archive at the L.S.E., and the Popper archive at Stanford. [↑](#endnote-ref-16)
17. In this context, one needs to look at the material to which I have referred in the previous footnote, his ‘Criticism and the Methodology of Scientific Research Programmes’, **Proceedings of the Aristotelian Society**, New Series, 69, pp. 149-86, and ‘Falsification and the Methodology of Scientific Research Programmes’ [1970], in his **The Methodology of Scientific Research Programmes** ed J. Worrall and G. Currie, Cambridge: Cambridge University Press, 1978, pp. 8-101. [↑](#endnote-ref-17)
18. Imre Lakatos, ‘Proofs and Refutations’, **British Journal for the Philosophy of Science**, 14, 1963-4, pp. 1-15, 120-39, 221-45, and 296-342. Lakatos refers to ideas he developed during this period in archive material, and to the paper itself in ‘Criticism and the Methodology’. [↑](#endnote-ref-18)
19. Some of the spirit of all this is, in fact, conveyed by the correspondence with Paul Feyerabend which was published under the title **For and Against Method**, ed. Matteo Motterlini, Chicago: University of Chicago Press, 1999. The editor, it might be noted, did not include an impassioned letter which Feyerabend wrote at the point when he discovered that Lakatos was keeping his letters, denouncing people who read such things in archives! [↑](#endnote-ref-19)
20. A good impression of Lakatos’s substantive views at the time is conveyed by a paper that he delivered to The Aristotelian Society at the time: ‘Criticism and the Methodology of Scientific Research Programmes’. [↑](#endnote-ref-20)
21. Popper found it difficult to know what to do with the kind of criticism that Lakatos had made in these terms, and on reflection it seems to me that what Lakatos was doing was a bit like the kind of work that Lakatos had done within the Communist Part, when trying to put together an ideological critique of a leading Hungarian Communist, using material taken from his own work. [↑](#endnote-ref-21)
22. To which Lakatos made acknowledgement in the material to which I have referred to in note 15. Lakatos’s possession of these – which he said that he had received from Popper’s publishers – was one of the sources of disagreement between Popper and Lakatos when I was Popper’s assistant. [↑](#endnote-ref-22)
23. The full story of what was taking place was complex, and I can offer only a brief account of it here. The issues were complicated as a consequence of Lakatos developing his own views by way of a reconstruction of different positions within Popper’s work. Anyone seeking to reconstruct exactly what was involved might usefully consult various papers on the relations between Popper’s and Lakatos’s views by John Watkins and Lakatos’s editor, John Worral. [↑](#endnote-ref-23)
24. See Margaret Masterman ‘The Nature of a Paradigm’, in **Criticism and the Growth of Knowledge**, pp. 59-89. [↑](#endnote-ref-24)
25. Other than when a paradigm breaks down under the accumulation of anomalies. [↑](#endnote-ref-25)
26. W. V. O. Quine offered a generalization of this, such that in his view it was open to us to modify anything, including logic, to save the phenomena. [↑](#endnote-ref-26)
27. Although I think that it could fairly be said that Popper seemed averse to offering systematic reconstructions of his views, to take account of various ideas that he subsequently acknowledged in passing. [↑](#endnote-ref-27)
28. On this, see Popper’s ‘Truth, Rationality and the Growth of Scientific Knowledge’, in his **Conjectures and Refutations**, London: Routledge, 1963. [↑](#endnote-ref-28)
29. He also argued for the importance of what he called a ‘scientific research programme’ (essentially, aspects of a scientific theory being protected from modification by means of a conventional decision to look elsewhere for resolutions to problems) having what could be called heuristic integrity. [↑](#endnote-ref-29)
30. See, on this, Spiro Latsis, ‘Situational Determinism in Economics’, **British Journal for the Philosophy of Science** 23, 1972, pp. 207-45. [↑](#endnote-ref-30)
31. Lakatos, however, was critical of Whewell’s own theoretical treatment of what was involved; cf., on this, his ‘Criticism and the Methodology of Scientific Research Programmes’. [↑](#endnote-ref-31)
32. One might see metaphysical research programmes as playing a heuristic role, in the sense of suggesting what kind of explanation to aim at. This, however, all depends on things which, at a higher level, is itself a matter of conjecture. [↑](#endnote-ref-32)
33. Lakatos referred to Whewell’s 'On the transformation of hypotheses in the history of science', **Transactions of the Cambridge Philosophical Society**, 1856. The material was subsequently included, as Appendix G, in Whewell’s **On the Philosophy of Discovery**, London: Parker, 1860. [↑](#endnote-ref-33)
34. One can relate it to what Popper called ‘metaphysical research programmes’. [↑](#endnote-ref-34)
35. See, on this, the discussion in J. W. N. Watkins, ’The Methodology of Scientific Research Programmes: a Retrospect’, in Kostas Gavroglu et al (eds) **Imre Lakatos and Theories of Scientific Change**, Dordrecht etc: Kluwer, 1989, pp. 3-13 and the paper of Musgrave’s to which he there refers. [↑](#endnote-ref-35)
36. As, indeed, Lakatos was himself to accept, when considering Agassi’s discussion of ‘Prout’s hypothesis’ in ‘Criticism and the Methodology of Scientific Research Programmes’. For a suggestion about the kind of appraisal that might legitimately be made, however, see my ‘Why the “Hopeless War”: Approaching Intelligent Design’, **Sophia**, 49, Issue 4 (2010), pp. 475ff. [↑](#endnote-ref-36)
37. See, now, Imre Lakatos, **The Methodology of Scientific Research Programmes**, ed. J. Worrall and G. Currie, Cambridge: Cambridge University Press, 1978. [↑](#endnote-ref-37)
38. This can be seen in Popper’s discussion of ‘conventionalism’ in his **Logic of Scientific Discovery**. For there, he argues that conventionalism and what is, in effect, the aspirational realism that he prefers are both possible views of what science should be aiming for, and that what methodology it makes sense to adopt, depends on which of those goals one favours. [↑](#endnote-ref-38)
39. See, on this, Larry Briskman, **A Sceptical Theory of Scientific Inquiry: Problems and Their Progress**, ed. J. Shearmur, Leiden: Brill, 2020. My own view is that people’s work should always be understood in terms of what they were trying to do, although clearly one might claim that they in fact accomplished something else. [↑](#endnote-ref-39)
40. See, on this, Lakatos to Kuhn 24th April 1973, L.S.E. archive. [↑](#endnote-ref-40)
41. See Colin Howson ed. **Method and Appraisal in the Physical Sciences**, Cambridge: Cambridge University Press, 1976, and Spiro Latsis ed. **Method and Appraisal in Economics**, Cambridge: Cambridge University Press, 1976. [↑](#endnote-ref-41)
42. **Studies in East European Thought** 60. June 2008, No. 1/2, ‘The Sociological Tradition of Hungarian Philosophy’, pp. 113-133. I have used this source just because it is later than, and reflects critically upon, some earlier work on Lakatos, such as Congdon’s ‘Possessed’, and Jancis Long’s ‘The Unforgiven’. [↑](#endnote-ref-42)
43. See https://plato.stanford.edu/entries/lakatos/#:~:text=was%20reacting%20against.-,1.3%20From%20Stalinist%20Revolutionary%20to%20Methodologist%20of%20Science,Marton%20Lipsitz%20and%20Margit%20Herczfeld [↑](#endnote-ref-43)
44. See, for this and Lakatos, Long’s ‘The Unforgiven’, and Alex Bandy, **Chocolate and Chess**, Budapest: ‎ Akadémiai Kiadó, 2010. Bandy’s book contains a great deal of information, gathered from different often hostile sources, about Lakatos’s life. For the book itself, see Aleksandr Tarasov-Rodionov, **Chocolate** [1922], Garden City, NY: Doubleday, 1932. [↑](#endnote-ref-44)
45. See Congdon’s ‘Possessed’, p. 285-6. [↑](#endnote-ref-45)
46. See Gábor Kutrovátz, ‘Imre Lakatos’s Hungarian Dissertation: A Documentation’, in **Appraising Lakatos**, pp. 353-74. [↑](#endnote-ref-46)
47. For more detail on all this, see László Ropolyi, ‘Lakatos and Lukács’. [↑](#endnote-ref-47)
48. He was involved in the development of a hostile dossier on the writings of József Révai, searching his work for, in Long’s words (p. 281), ‘incriminating quotations to be used against him if necessary’ – a skill which one might see as also put to use in some of his work on Popper! [↑](#endnote-ref-48)
49. See on this Congdon, p. 290. [↑](#endnote-ref-49)