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Imre Lakatos: A Critical Appraisal

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Imre Lakatos holds a well-deserved primary place in current philosophy of science. In this essay, Leslie Allan critically examines Lakatos' theory of knowledge in two key areas. The first area of consideration is Lakatos' notion that knowledge is gained through a process of competition between rival scientific research programmes. Allan identifies and discusses four problems with Lakatos' characterization of a research programme. Next, Allan considers Lakatos' proposed test of adequacy for theories of rationality using his methodology of historiographical research programmes. Allan discovers tensions within Lakatos' criteria and suggests ways of strengthening his test.

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1. Introduction

Imre Lakatos is widely regarded as one of the most significant philosophers of science of the twentieth century. Along with Karl Popper, Thomas Kuhn and Paul Feyerabend, he helped shape modern approaches to the theory of knowledge. Lakatos championed the importance of the study of the history of science and its implications for epistemology until his sudden death in 1974.

In this critical review, I will examine two of Lakatos' fundamental theses. His first thesis sought to answer the question, 'What is science and what are its methods?' Lakatos' answer to this question was that science is a body of knowledge arrived at by a methodology of competing research programmes.[1] In the first part of this review (§2), I will outline Lakatos' view of the core components of a scientific research programme and how it operates to advance our knowledge of the world. Although I accept his characterization of the nature and methods of science as being substantially correct, I go on to identify four shortcomings requiring a solution.

Firstly, I question the reason proponents of a research programme protect its metaphysical core from refutation. Is it by methodological fiat, as Lakatos suggests, or is there a less dogmatic reason? I also question Lakatos' separation of variants of a single theory during its historical development into different theories. Another quandary I address about theories is whether we should include the background assumptions required to make predictions as part of the theory itself, as Lakatos suggests. And I leave unsettled a key difficulty in separating out the metaphysical core and the positive heuristic of scientific research programmes.

Lakatos' second thesis proposed a solution to the problem of developing and testing theories of rationality. On Lakatos' view, a methodology should be judged by how well it reconstructs the history of science as a rational enterprise. The most useful meta-methodology, he proposed, was his own methodology of scientific research programmes elevated as a meta-criterion.[2] In the second part of this essay (§3), I critically examine Lakatos' historiographical criteria for evaluating theories of rationality based on how well they reconstruct the history of science. I reveal the internal tensions lurking in Lakatos' four criteria and replace them with two revised criteria. To finish off, in order to strengthen his test for methodological adequacy, I supplement his historiographical conditions with a new coherence test.

[1] See Lakatos [1978a: ch. 1], also reprinted in Lakatos and Musgrave [1970: 91–196]. For an earlier formulation, see Lakatos [1978b: part 2, ch. 8, §6].

[2] See Lakatos [1978a: ch. 2].

2. The Methodology of Scientific Research Programmes

I take Lakatos' description of science as a system of competing research programmes to be substantially correct. In other words, I do have misgivings and think that it needs to be revised in some of its details. Firstly, how did Lakatos see the nature and function of the research programme?

Lakatos had considered that the unit of appraisal in science was not the isolated theory, but a research programme. To know whether a theory constitutes a part of science, according to Lakatos, it is necessary to know its history. If it had been arrived at by content reducing *ad hoc* modifications, in the face of anomalies, of earlier theories, it is not 'scientific'. It is a series of theories—a research programme—then, that is deemed 'scientific' or 'pseudo-scientific'.

A research programme, Lakatos explained, is composed of a 'negative heuristic' and a 'positive heuristic'. The 'negative heuristic' specifies the 'hard core' of the programme; its metaphysical foundations or conceptual framework. This 'hard core' is deemed irrefutable by the methodological fiat of the programme's proponents. Every worthwhile research programme develops in an ocean of anomalies. It is the function of the 'negative heuristic' of the programme to prevent such anomalies from refuting the 'hard core' by directing the scientists' attention to the revision of the 'protective belt' of auxiliary hypotheses and initial conditions. Just how the 'protective belt' is to be modified is specified by a partially articulated plan; the 'positive heuristic'.

A research programme was regarded by Lakatos as 'progressive' if the successive modifications of its protective belt satisfy the following two conditions. Firstly, each successive modification must be 'theoretically progressive', or have 'excess empirical content' in the sense that the new theory, which consists of laws of nature, auxiliary hypotheses and initial conditions, must predict some hitherto unexpected, novel fact. Secondly, the modifications must be 'empirically progressive' in the sense that the predicted novel facts must be at least occasionally corroborated. Conversely, a programme that is not 'progressive' is deemed 'degenerating'. Lakatos considered that for a research programme to be 'scientific', it must be at least theoretically progressive. For one research programme to supersede a rival, he added, it must be progressive while its rival is degenerating. Furthermore, it must satisfactorily explain the previous predictive successes of its rival.

The main problems that I have with Lakatos' thesis, as I have outlined it here, are these. Firstly, it is historically inaccurate to claim that every proponent of a scientific research programme had deemed its hard core to be irrefutable by a methodological decision. Musgrave [1976: 457–67] has adequately argued this objection using the Newtonian mechanics programme as an example.

We must find a new explanation of the historical continuity of a hard core through successive phases of the programme of which it is a part. A promising explanation may be this. The metaphysical framework principles constituting the hard core would have reduced explanatory value if modified, in the face of anomalies, in the ways suggested by its proponents. Take Musgrave's example of the suggested modifications to the mathematical form of Newton's Universal Law of Gravitation. To have modified the law would have made

it inexplicable, for its inverse square law form was understandable in geometric terms, just as the laws regarding light and radiant heat intensity were for the earlier Newtonians. The inverse square law was also later found applicable to magnetic and electrical attractions and repulsions; the later Newtonians similarly understanding the intensity of these phenomena in terms of the surface area of spheres with varying distances from a central point source. So, to have modified the inverse square law in the case of gravitational attraction would have rendered a partly understandable phenomenon wholly inexplicable.

My second misgiving with Lakatos' first thesis is that he regarded a research programme as a series of theories. But is it strictly correct to describe, say, Newton's development of his increasingly complex planetary models as formulations of new theories? It seems more in accord with accepted usage to say that Newton was articulating new versions of the same theory. I shall henceforth follow McMullin's [1976: 416, 420] suggestion and refer to a research programme as developing the one theory.

McMullin has made other important constructive criticisms of Lakatos' thesis; a second one deserving of mention here. Contrary to Lakatos, a 'theory' is not normally understood as including all of the background knowledge required to make specific predictions.[3] As McMullin [1976: 416, 421] notes, such 'auxiliary hypotheses' as theories of measurement are more properly regarded as 'theoretical assumptions over and above those that form an explicit part of the theory itself'. I shall adhere to McMullin's terminology here as well.

The fourth problem that I have is the difficulty in reconstructing the hard core and positive heuristic of actual research programmes. Lakatos [1978a: 119] himself admitted that this can only be done with hindsight, but even here we need reasonably clearly formulated rules to guard against arbitrary selection of materials for inclusion. Feyerabend [1979: 131–6] forcefully presses this point. I have no suggestions to offer on this problem. More work needs to be done in clarifying and strengthening Lakatos' first thesis. In spite of these shortcomings, I regard Lakatos' methodology as being far superior to earlier proposals. I now turn to his second thesis concerning theories of rationality.

[3]For an example of where Lakatos states this view explicitly, see his [1978b: part 2, ch. 8, §6, 175].

3. History of Science and Its Rational Reconstructions

3.1 Internal Coherence of Theories of Rationality

Lakatos had not only argued that his methodology of scientific research programmes (MSRP) is the best available characterization of scientific method, but that it is also the most adequate theory of rationality. According to his theory of rationality, it is rational to epistemically value progressive research programmes while epistemically devaluing degenerating programmes. Naturally, he considered it irrational to do the converse. In his [1978a: 119], Lakatos attempted to defend this thesis in a novel manner. I shall argue that Lakatos' defence is not wholly adequate and then go on to propose what I think is a more successful vindication of Lakatos' criteria.

Lakatos' argument was this. All of the methodologies of science so far proposed have been offered as normative criteria for scientific rationality. Each methodology serves to provide a different rational reconstruction of the history of science. Furthermore, they differ on where to place the dividing line separating what is to be explained 'internally' as the idealized application of scientific method and what is to be explained 'externally', in socio-psychological terms, as the difference between the rationally reconstructed 'internal' history and actual history.

Conventionalist methodologies are notoriously difficult to criticize on logical and epistemological grounds. However, Lakatos proposed, they may be criticized for how inadequately they provide a rational reconstruction of the history of science. The criterion by which a rational reconstruction, and hence a methodology, may be judged is that methodology itself. So, falsificationism, which posits the conventional acceptance of 'basic statements', employed as such a normative historiographical meta-criterion, when applied to itself, 'falsifies' itself. This is so because the falsificationists' rationally reconstructed history of science is contradicted by an accepted 'basic value judgement' of the scientific elite. Most other methodologies, when used as a meta-criterion and applied to themselves, also fail their own standards.

Attempts at applying the meta-criterion of falsificationism, Lakatos continued, in order to 'falsify' the MSRP result in a hollow victory, for any methodology whatsoever can be 'falsified' because no community of scientists is completely rational, and so no rational reconstruction of science can ever perfectly mirror actual history. A more adequate normative historiographical meta-criterion is a meta-methodology of scientific research programmes or, what Lakatos termed, a 'methodology of historiographical research programmes' (MHRP). Different methodologies are now seen as the hard cores of normative historiographical research programmes.

The advantage of the MHRP is that it allows the proponents of a historiographical research programme to ignore anomalies as long as the programme is progressing. Secondly, the proponents of the programme need only take notice of criticism if it is constructive; that is, if the criticism will further our knowledge of method. On this meta-criterion, the MSRP is progressive since it reconstructs more of the history of science

as rational, has led to the reversal of some historiographical appraisals and has successfully predicted novel historical facts.

Lakatos [1978a: 132] was correct in 'maintaining that a theory of rationality has to try to organize basic value judgements in universal, coherent frameworks'. However, he failed to raise, let alone answer, the question of why Enlightenment and post-Enlightenment physics should be regarded as the paradigm of rationality. Lakatos had given us no argument as to why it is not more reasonable to accept, say, the basic value judgements of fundamentalist theologians as exemplars of rationality and test our rational reconstructions of the history of fundamentalist theology against these judgements. 'Rational reconstructions', therefore, cannot be methodologically instructive unless we have some defensible reason for our choice of historical subject matter.[4]

So, a methodologist faced with a failure of his methodology (*qua* rational reconstruction of science) to stand up to historical tests has the option of retaining his methodology while discarding the thesis that science is a rational enterprise. For Lakatos simply to charge such people with 'disrespect' [1978a: 127] and 'temerity' [1978a: 129], and to leave it at that, is just not good enough.

I do think, though, that Lakatos' thesis is of some value if it is supplemented with an argument for the *prima facie* rationality of the scientific enterprise. Such an argument would refer to the way in which the spectacular predictive and technological success of the sciences was foreshadowed by deliberate theoretical developments, making it even more unlikely that this success was due to 'fantastic networks of coincidences'.[5] Any alternative theory of rationality must adequately explain this predictive and technological power as being the byproduct of irrational and non-rational forces. Seen in this light, the testing of methodologies against the history of science has *prima facie* plausibility.

[4]Feyerabend has forcefully argued this point in his [1975: 201–14] and again in his [1979: 109–20].

[5]In presenting one form of this argument, Shimony [1976: 474–8] refers to the difficulty in accounting for the success of science as the result of 'fantastic networks of coincidences'. Worrall [1976: 164] also briefly alludes to the *prima facie* plausibility of the rationality of science.

3.2 Tensions in Lakatos' Meta-criterion

Lakatos' novel contribution to the theory of how to test theories of rationality against the history of science is very interesting. I would like to make a few brief remarks. His critique of meta-falsificationism is devastating because, as Lakatos points out, meta-falsificationism does not allow for the fact that science as a human enterprise is not completely rational [1978a: 131, 134] and, secondly, is incapable of evaluating progress in theories of rationality [1978a: 132]. Lakatos' proposed alternative meta-criterion [1978a: 132–4], the MHRP, recognizes progress in theories of rationality if the new theory:

1. reconstructs more, but never all, of the basic value judgements of scientists as rational,
2. leads to an empirically progressive revision of some previously held basic value judgements,
3. predicts novel historical facts, and
4. anticipates further basic value judgements.

Conditions 3. and 4. above serve a part of the function of evaluating ordinary scientific historico-sociological research programmes, such as Marxism and Bellah's typology of religions, in accordance with the MSRP.[6] (They are only a part because Lakatos did not include the condition that the predictions must be in accord with the positive heuristic of the programme. Fortunately, such a heuristic may be easily reconstructed.) This is to be expected if progress in theories of rationality is dependent on our knowledge of science as a historico-sociological enterprise. So, by knowing more about what scientists do and how scientists evaluate, we shall know more about rationality.

Conditions 1. and 2. introduce the normative elements into the meta-criterion. But here a tension develops. Consider this possibility. A new methodology is proposed and developed that reconstructs fewer of the basic value judgements of scientists as rational compared with the MSRP and is remarkably *empirically* progressive, predicting (postdicting) many astounding novel historical facts.

This new methodology is empirically progressive in two ways. Firstly, by writing the history of science as the application of this method it uncovers novel historical facts. It is progressive in its 'internal' historiography. Lakatos' [1978a: 133] successful predictions of 'a complicated war of attrition' without crucial experiments and 'hordes of known anomalies in research programmes progressing on possibly inconsistent foundations' are examples of this 'internal' empirical progressiveness of his MSRP.

Secondly, this new methodology postdicts novel historical facts in its neutralization of its 'anomalies'; that is, those 'basic value judgments' of the scientific community that conflict with the methodology. It explains these conflicts, in an empirically progressive way, as the result of deviant sociological or psychological causes, such as religious or political

[6]Condition 4. may be considered a subclass of condition 3.

persecution of rival scientists. The methodology is empirically progressive in its 'external' historiography.

Now, it seems that this new methodology would represent progress in our theories of rationality. However, surprisingly, for Lakatos it would not be progressive because it did not reconstruct more of the basic value judgements of scientists as rational. But is it not *more* important to develop a deeper understanding of scientific methodology than to be inadvertently drawn into a rationalization of the prejudices of some scientists by insisting on the *numerical* increase of 'rationally' reconstructed 'basic value judgements'?

Lakatos had recognized that progress in our theories of rationality 'must even lead to the revision of previously held basic value judgements'. 'This', according to Lakatos, 'is analogous to the exceptional "depth" of a theory which clashes with some basic statements available at the time and, at the end, emerges from the clash victoriously.' [1978a: 132 and n. 1 on same page] Lakatos' insight here is a statement of condition 2. above and reflects the line of thought I have. In contrast, however, Lakatos' condition 1., specifying a numerical increase in agreement, severely restricts the amount of 'depth' a theory of rationality is allowed to possess. Lakatos had never appreciated this tension between his conditions and easily conflated them into one harmonious standard: 'progress in the theory of rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational' [1978a: 133].

3.3 Improving Lakatos' Meta-criterion

In the light of my criticisms advanced in the previous section, I suggest the following revision and further explication of Lakatos' criteria for a progressive theory of rationality. I propose that the theory of rationality must be:

i) 'internally' empirically progressive by either postdicting or novelly deriving[7] corroborated 'basic value judgements' of scientists from known historical circumstances, or by successfully postdicting other types of novel historical facts from known 'basic value judgements' of scientists, in the process of writing the history of science as the application of method, and

ii) 'externally' empirically progressive by successfully postdicting novel historical facts in its explanation of the seeming inconsistency between the expected 'basic value judgements' of scientists with the application of method and the actual 'basic value judgements'.

I am unsure about what relative weight to give to these two conditions when judging rival theories of rationality. This will not normally be a problem, for usually one methodology will be far superior 'internally' and 'externally' compared with its rivals, although there will be difficulties in assessment with the appearance of a new powerful theory. This is a detail that will need to be worked out at some later time.

My revised criteria have completely eliminated the need for Lakatos' troublesome condition 1. His condition 2. is now encapsulated under both my conditions i) and ii). It will be served by i) if the new 'basic value judgement' is shown to be irrational by empirically progressive developments in 'internal' historiography and by ii) if it is shown to be irrational by empirically progressive developments in 'external' historiography. Condition 3. is obviously superseded, once again, by my conditions i) and ii). Condition 4. is superseded by that part of my condition i) that refers to the novel derivation or postdiction of corroborated 'basic value judgements'.[8]

In this revised form, I think that the MHRP is of value in judging rival theories of rationality, although I doubt its persuasive force. This is because the arguments in its favour are a meta-level application of the MSRP and so will not be persuasive to those not already partial to this particular methodology. The first argument, that the MHRP recognizes that a rational reconstruction of the history of science can never be complete because of human fallibility, although derived from the MSRP's notion of progress in an ocean of anomalies, has independent weight. However, the second argument, that the MHRP can judge progress by the extent to which a methodology successfully postdicts novel facts, will only have

[7] By 'postdicting' or 'novelly deriving' a fact I mean deriving a statement from the theory that was known to be true to the constructors or revisers of the theory during construction or revision, but was not used to construct or revise the theory.

[8] Worrall [1976: 164–68] has produced a valuable clarification and revision of Lakatos' meta-criterion. In the last paragraph of his discussion [1976: 168], he briefly mentioned that on his account a better methodology may rationally reconstruct fewer 'basic value judgements' of scientists. However, he did not develop this idea as I have done here.

weight for those methodologists that already recognize the epistemic worth of corroborated novel facts. This is a weakness of Lakatos' meta-level defence of the MSRP.

It is at this point that I want to correct a misinterpretation of Lakatos' second thesis. It is not too difficult to be misled by Lakatos' dialectical presentation into thinking that he argued that one test of a methodology is to see how adequately it rationally reconstructs the history of science using itself as a meta-criterion of adequacy, and that on this test of coherence the MSRP passes while inductivism, conventionalism and falsificationism fail.[9] However, a more careful reading will show that no such coherence test was advocated by Lakatos, either in his [1978a: ch. 2] or in his earlier version [1978a: ch. 3].

His point was not to test the adequacy of each rational reconstruction with its own meta-methodology, but to propose a new meta-criterion for judging the adequacy of rational reconstructions of history. To do this, he adopted a dialectical approach by first showing the inadequacies of meta-falsificationism by applying it in practice, and then by proposing a better historiographical method of criticism; namely the MHRP. Lakatos wrote:

> I shall try to develop this historiograpnical method of criticism in a dialectical way. I shall start with a special case: I first 'refute' falsificationism by 'applying' falsificationism (on a normative historiographical meta-level) to itself. Then I shall apply falsificationism also to inductivism and conventionalism, and, indeed, argue that all methodologies are bound to end up 'falsified' with the help of this Pyrrhonian machine *de guerre*. Finally, I shall 'apply' not falsificationism but the methodology of scientific research programmes (again on a normative-historiographical meta-level) to inductivism, conventionalism, falsificationism and to itself, and show that —on this criterion— methodologies can be constructively criticized and compared. This normative-historiographical version of the methodology of scientific research programmes supplies a general theory of how to compare rival logics of discovery in which (in a sense carefully to be specified) *history may be seen as a 'test' of its rational reconstructions*.
>
> [Lakatos 1978a: 122f]

This is a clear, concise and precise summary of Lakatos' intention. We have here not a single word of 'coherence tests'. For Lakatos, it is the MHRP that is to supply the general theory of how to test comparatively methodologies against history. That Lakatos applies meta-falsificationism and the MHRP to other than their object-level counterparts also belies the notion that he had any coherence test in mind.

Lakatos' dialectical approach is further explained in his earlier version. Referring to his meta-falsificationist 'falsification' of falsificationism, he wrote, 'I only chose this Socratic-Popperian dialectical way of developing my position because I think this is the best way of developing a complex argument . . . ' [Lakatos 1978a: 148] No mention here of Popper's fatal incoherence. In fact, Lakatos explicitly stated that his meta-falsificationist 'falsification' of falsificationism was *no* reason to reject falsificationism: 'Popper's theory of scientific rationality need not be rejected simply because it is "falsified" by some actual

[9]See, for example, John Fox's review of Lakatos [1981: 100].

"basic judgements" of leading scientists.'[Lakatos 1978a: 132][10] This is because it is an inadequate meta-criterion and there are good arguments for a better meta-criterion, namely the MHRP, Lakatos explained. My key aim here has been to dispel this notion that Lakatos was presenting a general method of testing methodologies for coherence.

Although Lakatos did not use such a coherence test on theories of rationality,[11] it seems that such a test as advocated by Fox [1981: 100] and mistakenly attributed to Lakatos is warranted. The problem of coherence is an acute one for monistic methodologies such as the MSRP.[12] If a theory of rationality posits that a theory is rationally justifiable iff it satisfies the demands of a particular set of methodological criteria, then what is the epistemic status of this theory of rationality? In order to answer this question, methodological monists have no choice but to apply their own methodological criteria to the methodological criteria themselves. The alternative non-monist methodologies are as seriously burdened with the unavoidable charge of arbitrary selection of methodological criteria.

So, I propose two separate tests for monistic methodologies *qua* rational reconstructions; a test of adequacy of the rational reconstruction as judged by the MHRP (as given above) and a more general test of coherence. As explained by Lakatos, we cannot judge the adequacy of a falsificationist rational reconstruction, once falsificationism is put into a monistic form, by a meta-falsificationist standard, just as we cannot justifiably test any rational reconstruction by this meta-criterion. (We must use the MHRP for this purpose.) But we can use meta-falsificationism to test falsificationism for coherence. On this test, falsificationism, as with some or most other methodologies in monistic form, fails. The MSRP, however, as Lakatos had argued, is the most progressive historiographical research programme and hence passes the test of the adequacy of rational reconstructions and the coherence test.

My early impression of Lakatos' [1978a: ch. 2] essay on the rational reconstruction of the history of science was that it was of little value. However, on closer scrutiny, I have come to regard it as being of some considerable merit. In this discussion, I have outlined my reasons for this change in my appraisal. In some important respects, though, it is still deficient. The testing of methodologies against the history of science is dependent on a *prima facie* case for the rationality of science and such dependence serves to weaken the force of any such test. (The same can be said for the above coherence test.)

Secondly, the arguments for Lakatos' meta-criterion and my revised version partly presuppose the cogency of the MSRP as a theory of rationality. In this respect, it is of limited effect in independently supporting the MSRP. It is for these reasons that I develop independent arguments for the MSRP in my Allan [2016b]. I do think that once Lakatos' criteria for rational theory appraisal are shown to fall out of an analysis of the requirements of an objectivist epistemology, his MSRP is on solid ground.

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[10] See also Lakatos [1978a: 151]. Here, Lakatos' text is in agreement with Fox's complaint [Fox 1981: 100] that Lakatos has not falsified falsificationism. Fox's misunderstanding on this point and on Lakatos' main thesis may be due to his falsificationist sensitivities.

[11] It must be pointed out that Lakatos here did not criticize Popper's falsificationism for incoherence, but because he 'never offered a theory of rational criticism of consistent conventions'. See Lakatos [1978a: 123].

[12] For a brief discussion of the problem of coherence for epistemologies, see Fox [1981: 96].

4. Conclusion

In this critical review of Lakatos' work, I summarised his theory of rationality based on his MSRP and suggested four areas in need of modification and improvement. Moving on to a consideration of Lakatos' MHRP, I argued that his defence of his theory of rationality, by appealing to its success as a historiographical research programme rationally reconstructing the history of science, was inadequate. This was because Lakatos had provided us with no reason for selecting the history of science as the exemplar of rationality, and so his MHRP appears arbitrary. Once it is supplemented with a *prima facie* argument for the rationality of science, his MHRP does serve as a partial vindication of his MSRP. Although, in the form that Lakatos had developed this meta-methodology, it suffers from an internal tension. I had sought to eliminate this tension and further improve his MHRP by suggesting important revisions. I had also argued that although Lakatos did not propose a coherence test for monistic methodologies, such a test is an essential additional requirement to his MHRP.

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