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## THE PROPOSITIONAL CONTENT OF THE POPPER–LAKATOS RIFT

Imre Lakatos telephoned me on the morning of February 2, 1974. He was in a fury. He had received page proofs of his contribution to the Schilpp volume on the philosophy of Karl Popper. He had been promised that his notes would appear as foot-notes; he now found that they were collected together as end-notes. He urged me to join him in getting the volume held up while this was put right. The volume was already several years behind schedule and I declined. It was our last conversation. He died later that day, going down, one might say, with guns blazing. The volume appeared a few months later (with an editorial apology about the placing of his foot-notes).<sup>1</sup>

The title of his paper, ‘Popper on Demarcation and Induction,’ harked back to the two problems which the young Popper had taken as fundamental and which he claimed to have solved at a stroke. The previous orthodoxy, as Popper saw it, was that science is differentiated from metaphysics, pseudo-science and other kinds of non-science, by its use of an inductive method. But induction, as Hume’s deadly analysis had showed, is without justification – the glory of science but the scandal of philosophy, as C.D. Broad (1952, p. 143) had put it in 1926. Popper sought to solve the demarcation problem by saying that science consists, not of verifiable hypotheses, but of falsifiable conjectures which it tests as rigorously as it can. However well they survive testing they are never verified or quasi-verified or probabilified or confirmed in any inductive sense. They can never be more than well-tested and unfalsified *conjectures*. A theory such as Freudian psycho-analysis, although it may seem to gain a great mass of confirmations, is only pseudo-science because (or so Popper claimed) it is untestable. This solution of the demarcation problem solves the problem of induction by eliminating it; science needs only deductive inferences, from premises to a prediction and from a falsified prediction to the falsity of the conjunction of premises which entail it. “There is no need even to mention ‘induction’,” Popper declared (1959, p. 315, italicized in the original); it plays no role in science.

Besides demarcation and induction, Lakatos’ paper in the Schilpp volume took up a third issue; he brought in, and then discussed and amended with much appeal to the (largely unspoken) value-judgements of the scientific elite, a meta-criterion by which Popper’s and others’ demarcation criteria may be assessed and amended. My heart rather sinks when a discussion turns

to meta-criteria; I feel like protesting, "First provide me with the meta-meta-criterion by which I may judge your meta-criterion." In the present discussion I am going to stick to Lakatos' challenges to Popper over the two main issues. This will provide quite enough material.

When Popper read Lakatos' Schilpp volume paper, late in 1969, he responded angrily. The dispute escalated into a bitter quarrel whose reverberations continue to this day. Spectators have tended to take sides, some friends and pupils of Lakatos concluding that an old tyrant had been humbled, while others regarded his paper as a shameless work of deconstruction. Among the latter is Joseph Agassi. He writes: "Lakatos died suddenly in 1974 ... His posthumous contribution to *The Philosophy of Karl Popper* of 1974 says of Popper that he has made no contribution to philosophy worth mentioning" (1993, p. 9). Does it? Lakatos' paper opens with these sentences:

Popper's ideas represent the most important development in the philosophy of the twentieth century; an achievement in the tradition – and on the level – of Hume, Kant, or Whewell. Personally, my debt to him is immeasurable: more than anyone else, he changed my life. I was nearly forty when I got into the magnetic field of his intellect. His philosophy helped me to make a final break with the Hegelian outlook I had held for nearly twenty years (1978, p. 139).

You may say that those are only opening compliments. Well, in the body of the paper he mentioned, quite truly, that some great scientists whose judgement had been warped by previous philosophies had been helped by Popper's philosophy (1978, p. 154). More relevant to an issue that will come up later is Lakatos' evaluation of Popper's theory of verisimilitude. He said that it was "an achievement marvellous both in its simplicity and in its problem-solving power. It became possible, for the first time, to define *progress* even for a sequence of false theories" (1978, p. 156). Agassi's memory let him down here.<sup>2</sup>

Popper's Reply contains a passage which is out of line with his critical rationalism. He said of a thesis of Lakatos: "Were the thesis true, then my philosophy of science would not only be completely mistaken, but would turn out to be completely uninteresting" (1974, p. 1005). The inference the reader was presumably intended to draw was that since his philosophy of science is not completely uninteresting Lakatos' thesis is not true. But why should finding his philosophy of science to be greatly mistaken render it uninteresting? According to his critical rationalism, the exposure by a pupil of a master's great mistake is the lifeblood of intellectual progress. He praised Thales in this connection. By contrast with dogmatic schools which seek to preserve a doctrine pure and unchanged, Thales created a new type of school based on a new relation between master and pupil (1963, pp. 149–150). Thales had explained the stability of our earth by saying that it is held up by water on which it floats (when it is rough, earthquakes occur). His pupil Anaximander explained it by saying that the earth "is held up by nothing, but remains stationary owing to the fact that it is equally distant from all other

things.” This implies that Thales’ explanation is greatly mistaken; does that have any tendency to render it uninteresting? Not in Popper’s eyes: he found it a ‘beautiful theory.’ Or consider Popper on Kant. Popper made a short but devastating analysis of confusions in Kant’s idea of synthetic apriori truth (1963, pp. 47–8). Since that idea was central to Kant’s whole philosophy, Popper was convicting him of a great mistake; but Popper would not have dreamt of saying that this renders Kant’s philosophy uninteresting. In the Preface to *The Open Society* Popper said that great men may make great mistakes; there was no suggestion in that book that ideas of Plato and Marx that are mistaken are thereby uninteresting.

One can regard the Popper-Lakatos dispute as a World 2 contest and ask how the contestants fared: was there an outright winner, and if not, did either man win on points? But I want to view it as a World 3 contest, and see how the underlying propositions fared. More specifically, I want to find out what revisions, if any, to the propositional content of Popper’s philosophy of science are called for by Lakatos’ challenge. However, that is easier said than done. The propositions involved here are sometimes distorted or disguised or even hidden by the words of our two protagonists, and to get at the World 3 content we will need to work through a good deal of World 2 material.

The lack of a straightforward correspondence between written sentences and propositional content takes different forms with the two men. I begin with Lakatos. In his case the problem is often that he was writing in a coded way or using a kind of Doublespeak. In his Schilpp paper and elsewhere he talked a good deal about different falsificationists; for instance, ‘the’ naive or dogmatic falsificationist and ‘the’ methodological falsificationist. For a time he seemed to be pairing the former with a mythical ‘Popper’ invented by Ayer, Medawar and others, and the latter with the real Popper; thus he often brought in quotations from Popper in the course of presenting ‘the’ methodological falsificationist’s position (1978, p. 24). Yet at one place he claimed to have exposed the main weakness of *Popper’s* naive falsificationism (1978, p. 150), and at another place, where he referred to “our savage falsificationist” (1978, p. 26), the foot-note trail leads back to Popper. He also operated for a time with numbered Poppers, ‘Popper<sub>0</sub>’ (invented by Ayer, Medawar et al.), ‘Popper<sub>1</sub>’ (the naive methodological falsificationist), and ‘Popper<sub>2</sub>’ (the sophisticated methodological falsificationist), and then agonized over which is the real Popper. It would have been much easier for the reader if he had discarded this proliferating apparatus in favour of unambiguous phrases like ‘According to Ayer, Popper holds ...,’ ‘Popper holds ...,’ ‘In opposition to Popper I hold ...’.

Just what did Lakatos say about Popper and the problem of induction? After mentioning that Popper in his early philosophy had suggested a purely *negative* solution, he added that Popper’s “later philosophy (based on the idea of truth-content and verisimilitude) involved a shift of the problem and also a *positive* solution of the shifted problem; but, to my mind, he has not yet realized the *full* implications of his own achievement” (1978, p. 140). It

sounds as though Popper, with his theory of verisimilitude, had in his hands a positive solution though one he had not yet fully articulated. Lakatos also spoke of three prongs in Popper's anti-inductivist campaign, declaring himself in full agreement with Popper concerning the first two. Concerning the third he reported:

I had long discussions with Popper in 1966–7 about these issues; I profited immensely from them. But I was left with the impression that on what I called the 'third prong of his anti-inductivist campaign' we may never see eye to eye. *The reason is not that our disagreement is too big; but that it is so very small* (p. 164, his italics).

What was this minuscule disagreement? It was nothing less than whether Popper, the 'scourge of induction' as Lakatos called him (1978, p. 161), should introduce a synthetic inductive postulate to link corroboration-appraisals to verisimilitude-appraisals. I suggest that Lakatos was using a kind of Doublespeak, flattering on the surface but less flattering underneath. I would decode the above sentences of Lakatos as follows:

Popper's negative solution didn't work. He later discovered the idea of verisimilitude. Around 1966–7 I suggested to him that he could obtain a positive solution by linking corroboration and verisimilitude with the help of a synthetic postulate. He opposed this suggestion.

Lakatos' idea was that without such a link the corroboration a theory gains when it passes a searching test remains no more than an excellent move in the 'game of science.' Such a link is needed to turn it into an advance in our understanding of the world.

In Popper's case the problem of getting at the propositional content of his sentences is different. He was not in the habit of writing sarcastically or with his tongue in his cheek (though he did describe Hegel as a "master logician" employing "powerful dialectical methods"). Where an individual sentence of his should not be taken quite literally this is usually because of exaggeration. I quoted him earlier saying that if a certain thesis of Lakatos were true his philosophy of science would be completely mistaken; but it is not logically possible for any body of propositions to be *completely* mistaken. Or take another sentence of his: "A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history" (1963, p. 35): on *every* page, including the sports pages? More serious difficulties are sometimes raised by the ways he deployed sentences. It is an interesting exercise to ask whether Popper ever explicitly addressed Lakatos on the question of induction. His Replies in the Schilpp volume are in five sections; section II is entitled 'The Problem of Demarcation' and section III 'The Problem of Induction.' Lakatos was accorded fifteen pages at the end of section II. His name does not appear, apart from two incidental mentions, in section III, whose opening sub-section is entitled 'My solution of Hume's problem of Induction.' (Soon afterwards this was expanded into Chapter I of *Objective Knowledge* which opens with the words "I think that I have solved a

major philosophical problem: the problem of induction.”) And Popper did not refer to Lakatos in any subsequent publication. So the answer appears to be that Popper did not explicitly address Lakatos on the question of induction.

But that turns out to be incorrect. We found that the gist of the propositional content of the passages from Lakatos quoted earlier was: corroboration needs to be linked to verisimilitude. Now a corroboration appraisal as understood by Popper sums up how a theory has performed so far and says nothing about future performance; by contrast, verisimilitude appraisals have no temporal restriction. So a principle linking corroboration to verisimilitude in the required way would have an inductive character. And in the answer which Popper made to Lakatos under the heading ‘The Problem of Demarcation’ there occurs the following remark: “I *did* suggest in *Conjectures and Refutations*, Chapter 10, that the degree of corroboration may be taken as an indication of verisimilitude” (1974, p. 1011, his italics). That little sentence means that the propositional content of Popper’s earlier publications already contained the inductive postulate called for by Lakatos. As I put it elsewhere (1984, p. 283), the whiff of induction which Lakatos invited Popper to introduce into his philosophy was already there.

Lakatos generated at least two pseudo-disputes with Popper as a result of what I call his Research-Programme imperialism. Let  $p$  be a proposition held by Popper and  $q$  be a proposition held by Lakatos, where  $p$  and  $q$  are, from a logical point of view, mutually compatible. A pseudo-dispute arises if Lakatos nevertheless asserts that  $p$  must be supplanted by  $q$ , although he makes no case for this and his reason for it is only that it is good propaganda for his Research-Programme methodology. Let  $p$  be the proposition that in science we appraise competing *theories* while  $q$  is the proposition that in science we appraise competing *research-programmes*. It might likewise be held that in architecture we appraise individual buildings and that in architecture we appraise streets. Neither kind of appraisal need exclude the other. But now imagine that within the architectural profession a militant Street Party starts up whose main plank is that street-appraisals must supplant building-appraisals since *streets* are the basic unit of appraisal in architecture. The main plank of Lakatos’ Research Programme Party was, in his words, “The basic unit of appraisal must not be an isolated theory or conjunction of theories but rather a *research programme*.”<sup>3</sup> I accept that scientific research programmes, about which he said interesting and important things, do constitute genuine units of appraisal. But what argument did he offer for his conclusion that theories, which he usually spoke of as ‘isolated,’ do not also constitute genuine units of appraisal? After saying, rightly, that a scientific theory is judged in relation to its predecessors, he immediately added: “Then, of course, what we appraise is a *series of theories*” (1978, p. 33). That is rather as if, after saying that a building has to be appraised in relation to neighbouring buildings, he had added: “Then, of course, what we appraise is *streets*.” And in any case a series of theories is not yet a research programme: the series terminates with a last theory, whereas a

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