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LAKATOS' CRITICISMS OF POPPER

I. INTRODUCTION

Imre Lakatos' thought is very fascinating, but it is also fascinating to study the development of his thought. This development is not at all linear and uniform. On the contrary it is full of great upheavals, and dramatic changes of direction. The works of philosophers who often change their opinion, usually give the impression of a lack of any coherent position. Strange to say, the works of Lakatos, despite his frequent changes of opinion, do not give this impression. The reason for this paradoxical situation is perhaps that at the centre of Imre Lakatos' philosophy there is a dialectical concept of continual change. His own changes of opinion are, consequently, compatible with his philosophical vision of the world.

In this paper, I want to discuss only one of Lakatos' great changes. In the years 1963–4 when he published *Proofs and Refutations*, he was a follower of Popper and a defender of Popperian philosophy. He wrote in the introduction:¹

The purpose of these essays is to approach some problems of the *methodology of mathematics*. I use the word "methodology" in a sense akin to Pólya's and Bernays' "heuristic" and Popper's "logic of discovery" or "situational logic."

It is obvious, moreover, that the full title of the work: *Proofs and Refutations. The Logic of Mathematical Discovery* refers to two of Popper's most famous books: *Conjectures and Refutations*, and *The Logic of Scientific Discovery*. Lakatos has the same attitude towards Popper in his 1968 article: *Changes in the Problem of Inductive Logic*. In this article with a title so characteristic of Lakatos, he defends Popper's theory of corroboration against Carnap's theory of confirmation. In 1973, only 5 years later, the situation was very different. In that year Lakatos gave his last lectures on method at the London School of Economics. These lectures were published for the first time in Italian translation in 1995 in *Sull'Orlo della Scienza*, edited by Matteo Motterlini. In them, Lakatos attacks Popper in a ruthless fashion. Lakatos still sees some merit in Popper's political philosophy, but says that there is nothing of value in Popper's philosophy of science. To quote Lakatos himself:²

Allegedly, Popper's three major contributions to philosophy were: (1) his falsifiability criterion – I think this is a step back from Duhem; (2) his solution to the problem of induction – where I think he is a step back from Hume ...; and (3) his literary masterpiece “*The Open Society by one of its enemies*” ... what is it called? *The Open Society and its Enemies*. ... *The Open Society* is frankly a literary masterpiece: not being a political philosopher I cannot comment on its contents, but I certainly think it is a marvelous book. So, in conclusion, two-thirds of Popper's philosophical fame is based mis-judgement.

To this Lakatos adds a little later:³

I think that the fact Popper's philosophy survived for so long is a sociological mystery. Popper's immortality is secured by this idiotic result.

How did such an enormous change in Lakatos' attitude to Popper take place? In fact I was working on my PhD under Lakatos' supervision when this change occurred. So I hope that it will be useful if I give, in the next section, some personal reminiscences of Lakatos at that time.

2. SOME REMINISCENCES OF LAKATOS

My undergraduate degree was from Cambridge, where I studied mathematics for two years, and then philosophy for another two years. In my last academic year as an undergraduate (1965–6), I formed the plan of doing a PhD on the philosophy of mathematics. Consequently I read all the recent articles on this subject in the hope of discovering some interesting research line to follow. After this survey of the literature, I had no doubt that the most interesting recent article on the philosophy of mathematics was *Proofs and Refutations*, published two years previously in the British Journal for the Philosophy of Science. I decided to do for my PhD a historical/philosophical study of some branch of mathematics on the model of *Proofs and Refutations*. So I wrote to the author Imre Lakatos in the summer of 1966 to ask if he was willing to take me on as a PhD student. Lakatos replied suggesting that I should come to meet him at the London School of Economics.

My first meeting with Lakatos was certainly an occasion to remember. After a little philosophical discussion, I uttered the name of Wittgenstein. Lakatos replied: “Wittgenstein was the biggest philosophical fraud of the twentieth century.” This statement really came as a surprise to me, since in those days the cult of Wittgenstein was still very strong at Cambridge, where I had been studying philosophy. Indeed my first year of philosophy at Cambridge consisted, for three quarters of the time, of reading Wittgenstein with care and attention. So I replied: “Dr Lakatos, what you say is truly surprising for me because I have just finished writing an essay in which I maintain that there are close links between your concept of mathematical proof and Wittgenstein's.” The next time I met Lakatos, he said to me: “Regarding Wittgenstein, I looked through my copy of his *Remarks on the Foundations of Mathematics*, and I was surprised to find that I had written

enthusiastic notes in the margins. But these notes were written in Hungarian which means that I must have been written them ten years ago, just after I had arrived in England." In fact Lakatos and I got on extremely well during the first few years of our acquaintance, although, unfortunately, we began to quarrel for various reasons later on. After I had known him for a few months, Lakatos said to me on one occasion: "Donald there is a problem about you. You are very difficult to understand because of your English accent." In fact in those days the philosophy department at the London School of Economics had a lively international atmosphere, and was full of Austrians, Americans, Hungarians, Italians, etc. So an English accent was really quite rare.

I began working on my PhD with Lakatos in October 1966. The first thing Lakatos told me was that I should begin by reading the entire works of Popper, because they were essential. Lakatos was truly enthusiastic about the philosophy of Popper in those days. Initially, however, I was reluctant to follow these instructions, because up to that time Popper had written little on the philosophy of mathematics. However, being a well-behaved student, I began to read Popper carefully, and, within a short while, there was no need for Lakatos to give me any further encouragement, because I was finding Popper's writings ever more interesting. At that time Lakatos was working on the article already mentioned: *Changes in the Problem of Inductive Logic*. As I said, this article is a defence of Popper's theory of corroboration against Carnap's theory of confirmation. For this reason, I also read Carnap's *Logical Foundations of Probability*, and so became more and more interested in the foundations of probability. As a result of all this I ended up by writing my PhD not on the history and philosophy of mathematics, but on the foundations of probability. A problem which particularly interested me in that area was one already discussed by Popper concerning the falsifiability of probability statements. In fact my original plan was only carried out in 1992, not in the form of a PhD, but in that of a collection of essays by various authors. I was the editor of this collection on *Revolutions in Mathematics*, and in the preface I made an acknowledgement to Imre Lakatos as the inspirer of this type of research.

Returning to 1966, the reason why Lakatos was engaged in a defence of Popper against Carnap at that time was the following. In the previous year (1965), Lakatos had organised a conference on the philosophy of science and mathematics in London. Many famous philosophers of the time attended, including Carnap, Kuhn and Quine. Lakatos was the editor of the conference proceedings, and decided to add to these proceedings two essays, both defending Popper, but against two different opponents, namely Carnap and Kuhn. All went well while Lakatos was writing against Carnap. But the defence of Popper against Kuhn turned into a critique of Popper, and the development of a new approach to scientific method – Lakatos' methodology of scientific research programmes. In the next section I will consider Lakatos' criticisms of Popper.

3. LAKATOS' CRITICISMS OF POPPER

In this context the key work of Lakatos is *Falsification and the Methodology of Scientific Research Programmes*, published in 1970. His criticisms of Popper are based fundamentally on the Duhem thesis. It is true that in the appendix of the article just cited, Lakatos speaks of the Duhem-Quine thesis, but in fact Quine's philosophy had little impact on Lakatos, while he used to study Duhem with the very greatest attention. It is not by chance that Lakatos, in one of the passages already quoted from his last lectures on method, speaks of⁴ "his falsifiability criterion – I think is a step back from Duhem ...".

Duhem expounds his thesis as follows:⁵

In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed.

If we accept the Duhem thesis, it would seem to be impossible to falsify an isolated hypothesis, and hence that the falsifiability criterion is unsatisfactory. The Duhem thesis poses the following further problem. If experience disagrees with a group of hypotheses, how can we know which of the hypotheses should be changed? Lakatos proposes his methodology of scientific research programmes as a solution to this problem of Duhem's. According to Lakatos, a scientist always works in the context of a research programme, which has a *hard core* or *negative heuristic*. Lakatos claims that⁶

This "core" is "irrefutable" by the methodological decision of its protagonists: anomalies must lead to changes only in the "protective" belt of auxiliary, "observational" hypotheses and initial conditions.

So if experience disagrees with a group of hypotheses, the scientists do not change those hypotheses which constitute the hard core of their programme. This reduces the difficulty of choice, and, moreover, each scientific research programme has a *positive heuristic* which gives advice on the way in which the programme should be developed.

These are the fundamental ideas of the methodology of scientific research programmes. Here I will not give further details, because this methodology is now well-known. Instead I would like to present the reasons why I did not, and still do not, accept this Lakatosian account of scientific methodology.

4. SOME CRITICISMS OF THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES

When Lakatos began to develop his new methodology, I had, following his instructions, just finished reading the works of Popper. I must say that I had found Popper's philosophy very much to my taste. I do not have Lakatos'

capacity for making sudden changes in my intellectual opinions, and I was, therefore, reluctant to give up the new faith which I had just acquired. Moreover, in my researches on the problem of the falsifiability of probability statements, I had discovered that the concept of falsification gives a reasonable foundation to the theory of statistical testing developed by Fisher and used by the majority of statisticians. Falsificationism therefore succeeds in providing a simple and satisfying explanation of probability which accords well with the most widely diffused statistical practice. This situation convinced me that there was a certain validity in the concept of falsification, despite the criticisms based on the Duhem thesis, and that Lakatos was therefore wrong to abandon falsificationism completely.

When Lakatos began to develop the methodology of scientific research programmes, the philosophy department at the London School of Economics was divided into two groups: those who accepted Lakatos' new ideas, and the 'old guard,' who tried to defend Popper's philosophy and criticize the new methodology. After Lakatos' death in 1974, I continued for many years to think about these problems. I finally reached the conclusion that Lakatos was correct in thinking that Popper's philosophy needs to be modified in the light of the Duhem thesis. However, I have come to accept the methodology of scientific research programmes only in part and with modifications, and so think that the problem created by the Duhem thesis needs to be solved in a way different from that suggested by Lakatos. In the rest of this section, I will explain the reasons why I do not accept Lakatos' position, or, rather, why I accept it only in part. In the next and final section, I will give a sketch of my own position regarding falsificationism and the Duhem thesis.

At the most abstract level, the difference between my position and that of Lakatos can be explained as follows. I accept the distinction between the *discovery* of scientific hypotheses and their *justification*. Lakatos, by contrast, tried to reduce the problem of the *appraisal* of knowledge to that of the *growth* of knowledge. My criticism of Lakatos is that this reduction does not work, and that, we need some notion of the empirical confirmation of theories in addition to those of the progress and degeneration of research programmes. Of course this notion of empirical confirmation need not be the same as Carnap's – indeed I think it will be closer to Popper's concept of corroboration. This then is my general position. I will begin to expound it in more detail by giving a quotation from Lakatos where he explains his idea of the reduction of *appraisal* to *growth*.⁷

But then two new problems arose. The *first* problem was the *appraisal of conjectural knowledge*. ... The *second* problem was the *growth of conjectural knowledge*. ...

In this situation *two schools of thought emerged*. One school – *neoclassical empiricism* – started with the first problem and never arrived at the second. The other school – *critical empiricism* – started by solving the second problem and went on to show that this solution solves the most important aspects of the first too.

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