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LAKATOS, REASON AND HISTORY¹

I. THE RATIONALIST PROGRAM

Lakatos' philosophy of science is puzzling and can be interpreted in widely varying ways. According to the most widespread interpretation, his aim was to reconcile a basically Popperian outlook with the historical facts discovered by Kuhn and Feyerabend.² The purpose of this paper is to use his work to critically assess the project of which, according to this interpretation, he was an outstanding representative. This project conceives the task of philosophy of science *as showing the value of science by developing a normative theory of science, a methodology*. Its chief representatives were the logical positivists and Popper. Currently, Larry Laudan is the leading representative of this approach. The turn of the century conventionalists Poincaré and Duhem do not belong here: they were interested in the epistemological status of scientific theories, rather than in laying down universal norms for scientific practice.³ It is for the same reason that later-day scientific realists like Richard Boyd or William Newton-Smith do not fit in here either. Their project is very much like that of the conventionalists, even though they reach practically the opposite conclusions. Kuhn and the other historically-minded authors also stand apart, for their approach is emphatically descriptive, not normative: they wish to study what happens in science rather than what should happen.

This project is often called "rationalism" both by its advocates and its critics. Lakatos uses a different term, "*demarcationism*." He describes it as one of the three possible answers to what he considers the central problem of philosophy of science: that of being the normative appraisal of theories. He summarizes it in this way:

In the demarcationist tradition, philosophy of science is a scientific standards watchdog. Demarcationists reconstruct *universal* criteria which explain the appraisals which great scientists have made of *particular* theories or research programmes. But medieval 'science,' contemporary elementary particle physics, and environmentalist theories of intelligence might turn out not to meet these criteria. In such cases philosophy of science attempts to overrule the apologetic efforts of degenerating programmes.

Demarcationists differ over precisely what the universal criteria of scientific progress are, but they share several important characteristics. First, they all believe in the third world of Frege's and Popper's three worlds. The 'first world' is the physical world; the 'second world' is the world of consciousness, of mental states and, in particular, beliefs; the 'third world' is the Platonic world of objective spirit, the world of ideas. Demarcationists appraise the *products* of knowledge: propositions, theories, problems, research programmes, all of which live and grow in the 'third world' (whereas the producers of knowledge live in the first and second worlds). In line with this, demarcationists also share a critical respect for the articulated. They readily agree that articulated knowledge is only the tip of the iceberg: but it is exactly this small tip of the human enterprise in which rationality resides. Finally, demarcationists share a democratic respect for the layman. They lay down *statute law* of rational appraisal which can direct a lay jury in passing judgment. Of course, no statute law is either infallible or unequivocally interpretable. Both a particular ruling and the law itself can be contested. But a statute book – written by the 'demarcationist' philosopher of science – is there to guide the outsider's judgment. (1976, 226–227)

The other two answers are scepticism and elitism. *Scepticism*, as Lakatos understands it, denies that there is a distinction between good and bad theories. Every theory is as good as any other. Given his description (1976, 225; 1978c, 107–108), it would be better call this view relativism, but he likes to project contemporary positions into the past, and the label "scepticism" serves this purpose better. He does not seem to be bothered very much by this approach. He always describes it rather briefly and never takes the trouble to argue against it. This is somewhat surprising, since the only representative of this approach he mentions is Feyerabend, with whom he had been arguing for over ten years. Later on, I shall suggest that one reason for his not attacking it may be that he has no resources to deploy. This, in turn, may explain the label "scepticism": many people think that, when push comes to shove, one just cannot argue against scepticism.

His real enemy then, the enemy he does take on, is *elitism*. This is the view that even though there is a distinction between good and bad, the criteria cannot be articulated in terms of explicit rules. Some people, the scientific elite, just see what is good due to their exceptional talents or their long experience. So quality control is possible, but it is not amenable to public justification. If we want to find out the value of a theory, we cannot apply rules but have to turn to the scientific elite. Of course, we have to know who to turn to. So the elitist lays down sociological and psychological criteria for the identification of the elite.

The thinkers Lakatos identifies as elitists are those who are not relativists but do not traffic in universal rules: Kuhn, Polányi and Toulmin. They would probably object to being classified in this way, and they would have a point: their professed aim is not to solve the problem of normative appraisal but to analyse how science works in descriptive terms. However, Lakatos' regarding of them as elitists is not groundless. He could argue that, by rejecting the enterprise of rule-based normative appraisal, they imply that appraisal is to be left to the scientists, a group of people with certain privileges, i.e. an elite.

So their rejection of the problem amounts to an elitist answer. He could add that many things these thinkers say would very much fit in with the elitist position.

But why does Lakatos think that this approach is wrong? His first objection (1978c, 114) is that normative evaluation of products must precede the sociological or psychological identification of the producers. The whole point of identification is to find out whose opinions can be trusted in the future. Using purely descriptive means, it is possible to identify many groups – high energy physicists, astrologists, stamp collectors, etc. – but this alone will not answer the question of whom to trust. It seems that criteria are necessary in order to decide which opinions are trustworthy, after which one may then employ sociological criteria to identify the group whose members are likely to have trustworthy opinions. However, the criteria for selecting the trustworthy opinions are just the sort of explicit universal rules the demarcationists are looking for. So elitism either does not get us anywhere, or it is incoherent, for it builds on the results of the demarcationist program, which it rejects.⁴ Alas, this objection is not particularly convincing. An elitist (and not just an elitist, for that matter!) may reply that one does not need explicit rules to start trusting some people. It is without rules that we trust parents and teachers. And these initial authorities may direct us to other authorities, say, scientists, and then we may start applying sociological techniques to identify that group.

Lakatos' second objection (1978c, 115) is that the elitist position implies that every change, in the elite's view, constitutes progress. If we ask the elite whether their current view is better than the earlier one, they will certainly answer "yes." So every change is improvement, which is absurd. However, this objection works only if the elitist is not fallibilist. The fallibilist elitist may say this: "Look, this is what the elite believes. Your best shot is to believe the same. It may happen later that they realize that they were wrong, and that their change for their current view was a step backwards. So from the vantage point of their future opinion, the change for the current view was not progress." So the elitist may re-evaluate his authority-based judgment when the authority's view changes, but this falls short of acknowledging that every change is progress.

The third objection is that elitism does not help if the elite is divided. That is true. Under these circumstances that elitist will not know what opinion to adopt. But is this a fatal shortcoming? Why should the elitist be able to provide an assessment at any time? Why cannot he suspend his judgment until the elite sorts out the questions?

I did not raise these difficulties to defend elitism, but to illuminate Lakatos' position. The fact that he does not consider these – not particularly ingenious – counterarguments suggest that he believes they are beside the point. And the reason why they are beside the point is that he believes that normative appraisal should be *objective*, in the sense that it should not rely on anything particular, local or personal. It should be made from the standpoint of

someone with no specific features or determinations at all (with the exception of the minimal rationality all sane adults possess). If one insists on this sort of objectivity, then the reply to his first objection is irrelevant. It is just a biographical matter what authorities one first comes to trust, and to what further authorities one is directed by them. These personal factors cannot lead to objective value judgments. So the reply simply does not speak to the point. The reply to Lakatos' second objection can be dismissed on similar grounds. Since the elitist's judgments vary together with the views of the elite, such judgments cannot be objective, since objectivity requires independence of accidental matters. If we have two theories and a body of relevant evidence, and these are kept constant, one should always arrive at the same judgment concerning their comparative value, and the judgment should not depend on local conditions such as the elite's opinion. The judgment can be overruled only if the theory or the evidence changes. A change in the elite's opinion is not an objective factor. Roughly the same reasoning can be applied against the reply to Lakatos' third objection. It might indeed happen that we cannot give higher marks to one competitor than to the other, i.e. we have to suspend the judgment. But the suspension of judgment cannot be based on the local condition of the elite's being divided.

This conception of objectivity makes it easy to understand some of the things Lakatos says about rationalism, i.e. demarcationism. The normative rules should be *universal*, since local rules, by definition, are not objective. They must be *explicit* rather than tacit, for the uninitiated cannot apply but explicit rules. Whether someone has been trained to pass judgment in a certain way or not is again a personal matter, so an objective judgment should not depend on it.⁵ Finally, the objects of objective judgment should belong to the *third world*, since second world objects, like beliefs, are not available for everyone. Newton's thoughts were available only for a few people at best, but his theory is there for everyone.

2. FINDING THE RULES

Hopefully, this analysis of Lakatos' demarcationism results in a clearer picture of the aim of the rationalist project: to show the value of science in terms of rules that are objective in the above sense. But how should the rationalist go about finding such rules? The only possibility seems to be to take some theories whose scientific credentials are impeccable, and compare them with those which do not qualify as being scientific. Popper actually tells such a story about how he arrived at his rules (1963, 33–37), and Lakatos repeats it (1971a, 123–124). Around 1919, when Popper first started to think about these issues, there were four widely discussed theories: Einstein's theory of relativity, Marx's theory of history, Freud's psychoanalysis and Adler's individual psychology. He was impressed by the first, but has come to question the scientific status of the other three. He was thinking in this way:

Einstein alone took a serious risk – Eddington's 1919 measurements could have refuted his view. The advocates of the other three theories, however, had claimed to have won without taking risks. Everywhere they looked, they found evidence. From here, he arrived easily at the criterion of falsifiability and the ban on ad hoc adjustments.

Now this way of finding objective rules may raise two sorts of concern. First, suppose Popper had adored Marx, Freud and Adler, but despised Einstein. Would not have he come up with a completely different set of rules then? Surely, the rules must be based on something more solid than personal taste. I will come back to this point, but only later in the paper. Let us now accept that his choice of good and bad examples was legitimate. The second worry is that he had helped himself to some particular examples, which became available only at a particular point in history. Suppose Popper had been a contemporary of Plato. Could he have *then* come up with the same criteria? Probably not. One's conception of what science should be like is certainly heavily dependent on what science is at the moment. Since her place in history is a specific fact about the prospective rationalist, it cannot be relied on in the formulation of the rules for science, since the rules must be objective. Fortunately, this worry can be put to rest by invoking the distinction between *discovery* and *justification*. What warrants the objectivity of the rules is that they can be justified in an objective manner. Their discovery can – and actually must – depend on particular facts, but that does not matter.

Thus, the objectivity of the methodological rules must be grounded in their justification. So how are methodological rules to be justified? The obvious strategy for this is the following. Find something suitable to serve as the ultimate goal of science, something that does not stand in need of further justification, and show that the consistent application of the rules leads towards the realization of this ultimate goal. The best (the only?) candidate for serving as the ultimate goal is truth. So you have to show that the theories your rules of appraisal favour are those which have a better chance to be true. If the logical positivists had been asked to defend their methodology, they would have said that the reason why scientists should prefer highly confirmed theories is that the better a theory is confirmed, the more likely it is to be true. However, justifications of this sort can be subjected to epistemological criticism. An example of this is Popper's attack on logical positivism. He pointed out first that the logical positivists' preference for highly confirmed theories has the disastrous effect of leading to theories poor in content. The richer a theory is, the less likely it is to be highly confirmed. In effect, the high confirmation rule favours truisms. Second, he repeated Hume's criticism of induction in order to sever the link between confirmation and truth. If induction is ruled out, there is no reason to believe that high confirmation is a mark of truth.

Popper's own rules suffered similar criticism at Lakatos' hands. He pointed out that Popper's requirement of high content and high falsifiability does not

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