

## REVIEWS

### LAKATOS IN DISGUISE

John Kadvany, *Imre Lakatos and the Guises of Reason*. Durham: Duke University Press. Pp. xx + 378. US\$23.95 PB, US\$69.95 HB.

By John Worrall 

The Reverend Sidney Smith, a prolific book reviewer for the *Edinburgh Review* in the early nineteenth century, used to say ‘I never read a book before reviewing it, it prejudices a man’. If reading without understanding also counts, then what follows ought to be largely unprejudiced since I found large parts of the book under review impenetrable. This is no doubt because of my own blind-spot for all things Hegelian, Lukàcsian, Habermasian and so on-ian. But blind-spot or not, it seems to be beyond remedy—despite my best endeavours, I just could not take in large parts of what was being claimed. 

In outline Kadvany’s theses are straightforward. First, there are two Lakatoses, the clear, analytic, decidedly Anglo-American follower of Popper, on the one hand, and, on the other, the Continental “Hegelian who covertly introduced innovative ideas about history, reason, and criticism into Anglo-American philosophy” (p. xi). Secondly, understanding both *Proofs and Refutations* and the “Methodology of Scientific Research Programmes” (MSRP) as thoroughly infused with and inspired by Hegelian ideas sheds enormous light on them. And finally, the philosophy of the Hegelian Lakatos (Lakatos<sub>2</sub>) also provides an illuminating way of viewing the “macabre” history of post world war two Hungary, “warning of the power and dangers manifested by the several guises of reason” (p. xvi).

The problems come in understanding the details—in grasping what exactly it is that we are meant to learn from looking on Lakatos’s philosophy through the eyes of the alleged Lakatos<sub>2</sub> (indeed in grasping what exactly Lakatos<sub>2</sub>’s views are); and what exact light is meant to be shed on recent Hungarian history by viewing it against the background of Lakatos<sub>2</sub>’s philosophy. (Kadvany himself, unlike his antihero with his Popper<sub>i</sub>, does not in fact resort to subscribing philosophers.)



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To indicate the order of difficulty here, let me quote a couple of representative passages. Of course this risks the charge of taking a cheap shot by quoting out of context; my only reply is that, for me at least, the context did not help.

With almost mathematical elegance, when the “base” theory itself includes historical categories, their historicization is the natural consequence of the Pyrrhonian peritrope, whether reinvented as Lakatos’s quasi-empirical self-application, or presented as Lukàcs’s changing function for historical materialism. (p. 229)

The truth of the dogmatic starting point is only conditionally assumed in order to carry to completion the construction of the isostheneia, and the formal residue of Gödel’s immanent critique of the Hilbert program. A typical dogmatic origin is found in the conditional antecedents of the incompleteness theorems. Mathematical undecidability, like isostheneia, is based on a particular “concession” of dogmatism, and thereby only conditionally refutes an absolute criterion of truth, which is not to show that there is absolutely no criterion of mathematical truth. There is no opposition of principles, but a single immanent critique from within one system and an absence of any genuine positive doctrine. (p. 120)

Of course it would be astounding if Lakatos’s Hungarian education and florid political past had left no mark at all on his mature thought. But what Kadavy calls the ‘English’ view of Lakatos’s intellectual history seems to me essentially correct—namely that the Hegelianism became ever more marginalised and those theses that are well-argued and clear in both Lakatos’s philosophy of mathematics and his philosophy of science are independent of anything distinctively Hegelian. Of course parallels can always be drawn between any two systems of thought—especially when one of them is as diffuse and malleable as Hegel’s—but this does not mean that any of those parallels are significant.

In *Proofs and Refutations*, Lakatos described in fascinating detail the process (or a process) by which mathematical knowledge is *developed* in a way that involves conjectured proofs, refutations of those proofs, and improved proofs that overcome those refutations. In my view nothing follows about the ultimate epistemic status of the theorems once this proofs and refutations process comes to an end (and despite the odd suggestion that it never comes to an end, there is no good argument to this effect in Lakatos). There are all sorts of fascinating incidental remarks—for example, about the ‘problem of translation’, the relationship between informal mathematical notions and the formal notions that allegedly ‘capture’ them. But so far as well-argued systematic theses go, the above claim about mathematical discovery is all. I would be happy to be convinced that there is more, especially more of a genuinely epistemological kind, but I cannot see it and certainly was not convinced of any extra import by Kadavy’s, for me impenetrable, treatment. The discussion in Chapter 4 of the Gödel theorem and its alleged relationship to Lakatos’s

philosophy is especially obscure. You can add to the passage quoted above from p. 120 the baffling, though certainly deep-sounding remark that the “essence of the proof of the second theorem is that the proof formalizes the informal mathematical proof of the first incompleteness theorem; and then that the conditional second theorem is formally provable, too” (p. 122). (In fact, the proof of Gödel’s second theorem simply proceeds by showing that, where ‘Consis’ is the ‘natural’ translation within arithmetic itself of the statement that arithmetic is consistent—‘from outside’ we can see that this sentence says that there is at least one number that is the Gödel number of a well formed formula but not the Gödel number of a theorem—then the conditional sentence ‘if Consis then G’ (where G is the Gödel sentence) is provable in any system satisfying the conditions of the theorem. Hence, if ‘Consis’ were provable in any such system S, then so, by one application of *modus ponens*, would G—contrary to the result of the first theorem.) There are what seem to be mistakes here. For example, Kadavy reports “Gödel proved that he could show conditional undecidability if arithmetic is consistent, and if arithmetic is inconsistent, then everything is provable, including Gödel’s theorems” (p. 120). But of course only every sentence expressible in the language of the system of arithmetic at issue would be provable if that system were inconsistent—this does not include Gödel’s theorems themselves, as opposed to the Gödel sentence G. It be this as it may, certainly no one who had not already understood complex and subtle issues involved in, for example, Feferman’s discussion of the true foundational impact of Gödel’s second theorem (and the ‘naturalness’ of ‘Consis’ as ‘the’ formal representative within S of the assertion of the consistency of S) would gain any enlightenment from this account of it.

As for Lakatos’s “Methodology of Scientific Research Programme” the ‘English’ view is that this is an important, if relatively modest, improvement of Popper’s account of science, inspired by the attempt to bring it closer to actual scientific practice, especially some of the elements of scientific practice highlighted by Kuhn. Again this is not far from the mark. Kadavy weaves a much grander interpretation from all sorts of strands, only two of which are unambiguously in Lakatos. The first of these is Lakatos’s fun-sounding, but misleading, claim in his paper on “History of Science and its Rational Reconstructions” that MSRP can be ‘self-validated’ by applying it at the meta-level. We think of the ‘basic value judgments of the scientific elite’ as analogous to empirical basic statements, then MSRP constitutes a ‘progressive shift’ compared to falsificationism and its other rivals: it explains more of those basic value judgments in a non-ad hoc way. The second basis for Kadavy’s grander

interpretation is Lakatos's intention, stated more than once, to write a book to be called *The Changing Logic of Scientific Discovery*.

But concerning the second of these, it is no mystery why Lakatos never even started this often-announced book. He thought it was a sexy title, but he didn't have any good arguments for a *changing* logic of scientific discovery, except in the sense that research programs and hence their associated heuristic principles ('look for wave theories of any group of optical phenomena', 'theories should be Lorentz-covariant', etc.) change. MSRP is clearly based on the idea that the fundamental logic of appraisal—what it takes for a program to be progressive or degenerating—is constant. Research programs change but the rules governing their change in the light of evidence lie outside the changing scientific fray, as unjudged judges.

As for the so-called self application of MSRP at the meta-level, Lakatos did develop *some analogy* between the way in which scientific theories are appraised and the way in which philosophical theories of good science are appraised. Since he held that the correct way to appraise scientific theories was via MSRP, this meant some analogy between MSRP and the appraisal of philosophical theories of science. However, it would be a mistake to get carried away. There is, for instance, no analogue for the crucial notion of independent testability at the meta-level; MSRP won't generate *new* predictions about the value judgments of the scientific élite that can then be tested. In fact, his suggestion amounts to little more than the idea—one that has been developed in the realm of ethics by Rawls and others—that we have a firmer grip on judgments about particular cases (in methodology, particular cases of good science) than we have on general judgments. The idea also has close similarities to the standard idea of 'explicating' a concept (here the concept of 'good science'), where in an explication one seeks to capture the clear-cut particular cases, exclude the clear-cut particular non-cases, and allows the analysis to decide the 'grey' cases. There is no significant sense in which MSRP 'applies to itself' and it is a recipe for confusion to hold that Lakatos "creates a theory that is its own metatheory" (p. 229). Despite the occasional remaining piece of Continental rhetoric, there is nothing either self-referential or historicised about Lakatos's philosophy of science. And nothing in Kadvaný suggests that, had there been, a coherent (let alone interesting) position would have resulted.

The final section of the book concerns post-war Hungary. Here the history—both of Hungary and of Lakatos in Hungary—seems to me clearly and interestingly told. However, with more space than I have here, I would enter several protests on behalf of my old friend. For example, having reminded the reader not to be too quick to judge the actions of

people subject to the most unimaginable pressures, Kadvany nonetheless feels able to describe Lakatos as “someone who earlier in his life was a dangerous thug with something like a criminal record, and who consistently displayed a pattern of dissemblance and cunning across the decades” (p. 314). This is an appalling, perhaps even libellous remark: I would have thought, for one thing, that someone with even ‘something like’ a criminal record should have been charged and given the opportunity to defend himself. Moreover, the suggestion that Lakatos was any sort of thug, let alone a dangerous one, is, to say the least, based on a highly contentious reading of history.

The main mystery for me here is what exact light is supposed to be shed on this history by juxtaposing it with Lakatos’s philosophy. Of course there are superficial similarities: Lakatos liked to talk about the value of ‘falsifying’ history of science and there was a great deal of falsification of history in Stalinist Hungary. Lakatos talked about the scientific élite, while the personality cult and the literary ‘élite’ both played significant roles in that history. The ÁVH (the Hungarian Secret Police) thought of themselves as rationally reconstructing by distilling “the allegedly essential aspects of conspiratorial history” (p. 293). And so on. In fact Lakatos’s talk of ‘falsifying’ the history of science was a quite unnecessary, merely rhetorical provocation. But laying that aside, the natural reaction to this list is ‘so what?’ Kadvany clearly believes that the similarities, far from being the sort of superficial similarities that one can find between *any two* quite disparate things, are of really deep significance. Again I can only confess that I couldn’t see it—perhaps other readers, more in tune with the *Zeitgeist*, will.

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