Beyond footnotes: Lakatos's meta-philosophy and the history of science

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Abstract

This paper revisits Lakatos's meta-philosophy concerning the use of historical facts for the purpose of philosophical theorizing about science. Despite Lakatos's bad reputation on that question – which mostly springs from his suggestion that the actual history could be detailed in the footnotes of texts of rational reconstructions of science – Lakatos in fact had quite reasonable things to say about the meta-philosophy of science. In particular, Lakatos's writings contain the idea that any philosophical methodology of science should aim at the maximization of rationally explainable facts, albeit without pretence to ever be able to explain *all* historical facts as rational. I will discuss this idea in the light of the contemporary meta-philosophical literature. Finally, the paper argues with Kuhn that there are many unappreciated analogies between Kuhn's and Lakatos's accounts of science and that Lakatos's account is, contrary to what Lakatos himself claimed, not more rational than Kuhn's, even by Lakatos's own meta-philosophical criteria.

1 Introduction

Lakatos famously advised to detail the actual history of science that "misbehaved" in the light of the rational reconstruction of science in the footnotes of philosophical texts (Lakatos 1971, 107). Unsurprisingly, this did not bode well with historians (Kuhn 1971, Holton 1974; 1978, Kuhn 1980, Arabatzis 2017). It did not help much that Lakatos later called this an "'unsuccessful joke" and claimed never to have suggested in all seriousness that one should treat history of science like that (Lakatos 1978, 192, see Nanay 2010). The damage had already been done (see e.g., McMullin 1970, Kuhn 1971, Holton 1974, Koertge 1976, Laudan 1977, Holton 1978, Kuhn 1980, Godfrey-Smith 2009, Arabatzis 2017). This is unfortunate in many ways. Not only has the dismissive view of the history of science implied by Lakatos's "joke" given philosophers a bad reputation among historians, but it also has overshadowed some of the more reasonable things Lakatos had to say about historically grounded philosophy of science.

In this paper I argue – contrary to the widespread cliché – that Lakatos actually did believe that good philosophy of science ought to accommodate as many historical facts as possible. My argument is based mostly on a close analysis of Lakatos's "History of Science and its Rational

Reconstruction", which first appeared in the 1971 PSA proceedings (Lakatos 1971), and later also in Lakatos (1978). This paper has often been misconstrued as giving advice to historians of how to do history of science (Holton 1974; 1978, Arabatzis 2017, Kuukkanen 2017).[1] But even though Lakatos indeed seemed to address "historians" in his paper, he never really engaged with the work of historians nor was he really interested in their concerns (see also Dimitrakos 2020).[2] Instead, he squarely focused on philosophy and how history could be used to support philosophical claims. As he put it himself, his arguments were "primarily addressed to the philosopher of science and aimed at showing how he can – and should – learn from the history of science" (122). Lakatos's project is thus better described as *meta-philosophical*. [3]

Lakatos, despite his bad reputation with the historians, actually can be seen as one of the main defenders of an integrated history and philosophy of science approach, as can be gleaned from his reformulation of Kant's famous dictum, with which he opened his metaphilosophical essay: "philosophy of science without history of science is empty; history of science without philosophy of science is blind" (Lakatos 1971, 91).[4] Lakatos also made clear that he opposed an "aprioristic philosophy" that is indifferent to empirical facts, and he also did not believe that there were any "immutable" scientific standards that could somehow be discovered by philosophers without analysis of the history of science (121). To argue that Lakatos had good grounds for making these claims – despite his notorious footnote quote – will be the goal of this paper.

The structure of this paper is as follows. Section 2 briefly introduces the basic ingredients of Lakatos's meta-philosophical account. Section 3 tracks more closely Lakatos's seemingly dismissive remarks about the history of science and whether there is anything to his claim that the famous footnote remarks was not meant seriously. Section 4 discusses several aspects of Lakatos's proposal as to how to test philosophical theories with historical evidence. There is one idea by Lakatos which I call the "maximization of rational facts" and which I will focus on in section 5. Section 6 discusses this idea in the context of the contemporary meta-philosophical literature. Section 7 focuses on the rationality of Lakatos's account by comparing it to Kuhn's account, which, as Kuhn pointed out, apparently very much influenced Lakatos. Section 8 concludes this paper.

2 Rational reconstruction, methodology, and history

Lakatos thought that philosophy of science should be in the business of providing *rational reconstructions* of historical facts about science. The idea of rational reconstruction goes back to at least Carnap, for whom it was closely tied to concept explication (Carnap 1950). Lakatos did not define what he meant by it, but he described the purpose of rational reconstruction as the "rational explanation of the growth of objective knowledge" (91). In particular, Lakatos was interested in

[1] Arabatzis criticizes Lakatos for not aiming to explain scientists' beliefs and judgments, which Arabatzis takes to be central to any historiographical project (Arabatzis 2017, 72).

[2] Tellingly, the only historical work that Lakatos mentions (repeatedly), is by the *philosopher* Joseph Agassi.

[3] The meta-philosophy of history and philosophy of science has gained new momentum in recent years. See e.g., Schickore (2011), Kinzel (2015), Bolinska and Martin (2020), and Schindler and Scholl (2022). See also section 6 of the current paper.

[4] The dictum goes back to Hanson (1962) and can also be found in Feigl (1970).

making rational sense of historical facts concerning the appraisal of theories, or series of theories, which he also called "research programmes" (Lakatos 1978). In other words, Lakatos saw the role of philosophy in explaining why it was rational for scientists to accept or reject research programmes.

The central notion in Lakatos's project of rational reconstruction is "methodology", which he also described as "theories of scientific rationality", "demarcation criteria", "definitions of science", or "logics of discovery" (Lakatos 1971, 92). This is quite a mixed bag, but ultimately Lakatos meant to refer to philosophical theories about the scientific method. As examples of methodologies, Lakatos mentioned inductivism, conventionalism, falsificationism, and his own methodology of scientific research programmes. Each of these methodologies could be put to use in the analysis of the history of science and produce different "internal histories", i.e., histories that would make rational sense from the perspective of the very methodology that was used. External history was the history that could not be rationally reconstructed with the methodology at hand. Lakatos was happy to leave external history to "empirical psychology and sociology of discovery" (91).[5] For illustration, consider two of the methodologies discussed by Lakatos, namely inductivism and conventionalism.

In inductivism, or rather in Lakatos's caricature of it, scientific propositions are accepted only if "provenly true", which, according to Lakatos, is the case when they describe "hard facts" or when they are "infallible inductive generalizations" from the facts (92-93). Lakatos conceded that there are some episodes in the history of science which were consistent with inductivism, such as Kepler's conclusion that the shape of planetary orbits is elliptic and not circular, which he inferred from Brahe's exceptionally precise observations (92). One question that inductivism cannot answer, according to Lakatos, is why scientists "select" certain facts and not others (93). That is, inductivism has not much to say about theoretical motivations that drive data collection and analysis. Such issues inductivism would then have to leave to external history.

Compare this to conventionalism. Conventionalism Lakatos defined as "pigeonholing" facts into "some coherent whole", whereby the resulting system is thought "true only by convention" (94-95).[6] Progress, for the conventionalist in Lakatos's portrayal, consists in a higher degree of simplicity of the classification of facts (96). Although Lakatos overall seemed more critical of conventionalism than inductivism, he was convinced that the Copernican revolution in astronomy could be described in conventionalist terms (96). As a central problem of conventionalism, Lakatos identified the question of why scientists chose to use certain theoretical classifications over others "at a stage when their relative merits were yet unclear" (96). Such questions, the conventionalist would thus have to leave to external history to sort out.

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[5] Obviously, Lakatos did not use internal and external history in their traditional sense. Traditionally, internal history of science is historiography concerned with the internal dynamics and developments of science, whereas external history of science is historiography concerned with science in relation to society. See e.g. Kuhn (1971).

[6] Lakatos mentions Duhem as a proponent.

Thus, each methodology would result in a different external history, i.e., different historical facts that it could not explain. As we shall see later, in section 4, Lakatos believed that we could assess different methodologies of science by the internal histories that they gave rise to. But first, let us turn to Lakatos's notorious footnote quote in the next section.

3 Of footnotes and distortions

Lakatos's notorious footnote quote pertains to the relation of internal and external history: "One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history in the text, and indicate in the footnotes how actual history "misbehaved" in the light of its rational reconstruction" (107). This passage has drawn criticism for obvious reasons: it seems to prioritize rational reconstruction over the actual historical facts. But what good is a rational reconstruction of history when it distorts the historical facts? Rather shockingly, Lakatos explicitly embraced the distortion of facts: "Internal history is not just a selection of methodologically interpreted facts: it may be, on occasions, their radically improved version" (106).

The reaction to the disparaging view of history of science that seemed to be implied by quotes like these were understandably stark. Kuhn for instance commented: "What Lakatos conceives as history is not history at all but philosophy fabricating examples. … Why is it … that Lakatos feels the need to protect himself from real history? Why does he provide a parody in its place?" (Kuhn 1971, 143). Similarly, Laudan wrote: "I object to the invention of historical figures and the fabrication of historical beliefs to score philosophical points or to teach philosophical lessons" (Laudan 1977, 170).

As already mentioned in the introduction, Lakatos later retracted his footnote statement and called it "an unsuccessful joke" (Lakatos 1978, 192). He even denied that he ever claimed that "this is the way in which history actually ought to be written and, indeed, I never wrote history in this way" (Lakatos 1978, 192).[7] Can this be right at all? Note that in the notorious footnote passage, Lakatos indeed spoke of only *one way* of indicating discrepancies between internal and external history; he did not say that history of science *ought to* be written like that. Is it also true that Lakatos never wrote history that way?

There are two main examples where Lakatos has been accused of distorting history. They concern Lakatos's discussion of the Bohr model and the Prout hypothesis. The former distortion concerns the fact that Lakatos himself describes the Bohr research programme as encompassing the introduction of electron spin. But Bohr never pondered the idea. This has been criticized by both Kuhn (1970) and Holton (1974), the latter of whom describes this particular part of Lakatos's account as "ahistorical parody that makes one's hair stand on end". With regards to the Prout hypothesis, namely the idea that the atomic weight of all chemical elements are multiples of the atomic weight of hydrogen, the distortion consists in the fact that Lakatos depicted Prout as being

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[7] See Hacking (1979) and Nanay (2010). Lakatos exempted his *Proofs and Refutations* from this claim (Lakatos 1978, 192).

aware of the real atomic weight of chlorine being 35.5 and that he, in spite of this, nevertheless held onto his belief that it ought to be 36. But that is not true. Prout was never aware what the real atomic weight of chlorine was; he just believed it was 36 (Koertge 1976, Hacking 1979). Lakatos did indeed mention this piece of "real history" in a footnote Lakatos (1978, 53). So at least on this one occasion, Lakatos did write history in way he later said he had not.

Neither of these two distortions strikes me as particularly severe. Surely, Lakatos would actually have helped his point if he had stated the facts regarding the history of the Prout hypothesis not only in a footnote (Hacking 1979); yet the distortion seems minor. The other distortion concerns Lakatos's decision to *name* a research programme after Bohr. As long as there is no pretence that Bohr himself subscribed to every part of it, I think this "distortion" is actually also innocuous. And Lakatos made very clear that he did not think that philosophy was in the business of explaining the beliefs of individual scientists anyway (Lakatos 1971, 106).

Of course, regardless of what he actually practiced himself, as a general recipe for philosophers doing history of science, Lakatos's recommendations regarding the distortion of history are unacceptable.[8] Unfortunately, they came to overshadow other parts of his view, which seem much more reasonable, and to which we shall now turn. Before doing so, however, let it be clear that the project of rationally reconstructing history is entirely distinct from distorting historical facts for philosophical means: the first does not imply the latter. One may very well try to identify the logical structure underlying theory appraisal in science *while at the same time remaining truthful to the actual facts*. In what follows, this will be taken for granted.

4 Testing philosophical norms with history … or how not to

Lakatos considered the history of science as a "test" of philosophical theories about science (Lakatos 1971, 108). Lakatos proposed two ways of going about this. The first one involved "falsificationism as a meta-criterion" and essentially consisted in seeking to falsify philosophical methodologies (in the sense introduced earlier) on the basis of historical facts, or rather on the basis of what he called "accepted 'basic value judgments' of the scientific elite" (Lakatos 1971, 110). Lakatos does not motivate the introduction of this concept other than by stating that he had been inspired by Popper's contrast between the scientific general theory of relativity and the pseudo-scientific psychoanalysis in the context of his discussion of the demarcation problem (ibid.). But I think his reasons for introducing the concept are fairly obvious: methodologically it is prima facie problematic to test norms against facts. Some would even consider this a straight-out fallacy (Giere 1973). Basic value judgments, on the other hand, are normative judgments, and thus allow Lakatos to get around the norm-fact divide. Furthermore, the basic value judgments of the *scientific elite*, if they could be had, should be fairly independent from the methodologies pondered by the philosophers. One would then have a testing procedure that would avoid charges of circularity (see also below). As an illustration of his proposal, Lakatos considers Popper's falsificationism inadequate as a scientific methodology, because contrary to what falsificationism would recommend, Newtonians did not reject classical mechanics, even when it could not account for the

[8] Larvor (1998) has argued that Lakatos's idea of rational reconstruction can be traced back to Hegelian ideas.

advance of Mercury's perihelion (Lakatos 1971, 111-112). Lakatos concludes that Popper's falsificationism would "show up even the most brilliant scientists are irrational dogmatists" (112).

The proposal, despite its initial plausibility, must raise eyebrows. First of all, it is not so clear what exactly Lakatos means by "basic value judgments". Understood at face value, they seem rather elusive: scientists rarely make statements in print of the sort "this theory, but not that theory, is a good scientific theory". Instead, it seems, Lakatos views such judgments to be implied simply by the kinds of choices that scientists make in practice. For example, if the scientific elite decides to adopt theory A instead of theory B, then the implicit value judgment would be that A is a better theory than theory B. Furthermore, one could infer from the very fact that scientists chose theory A that *one ought to* choose theory A. But that seems to amount to pulling normative rabbits out of descriptive hats.

Lakatos did allow for the possibility that not all theory choices made by scientists are the right choices. He therefore proposed a "pluralistic system of authority" in which the judgments by philosophers may on occasion overrule the judgments of the scientific elite, particularly when research programmes degenerate (and scientists keep working on them) or when a "scientific school degenerates into pseudoscience" (Lakatos 1971, 121). When the philosopher is supposed to step in, however, Lakatos left open (see also section 7).[9]

Suppose, then, that we ignore the norm-fact divide and we do try to test philosophical theories on the basis of historical facts *directly*.[10] What kind of history would we then test our philosophical theories on? It cannot be external history because a particular methodology's external history is *by definition* inconsistent with that methodology. It is also not clear that we could test a methodology with the external history of *another* methodology; according to Lakatos *any* external history is supposed to be irrelevant to philosophical theories. It seems also obvious that we cannot test a methodology on the basis of the internal history that it led to, because that would be circular or, in the case of the use of internal history produced by another methodology, question-begging (McMullin 1970, Kuhn 1971). At least that would be so if the historical facts were distorted by the methodologies, which certainly should be concern if one were to follow some of the things Lakatos has said (see the previous section).

There is in fact a third category of history of science in Lakatos's writings, namely "actual history", which can be understood as the set of all historical facts from which internal and external history are constructed. Lakatos's most promising proposal is that we can compare and test competing methodologies by the number of historical facts they succeed in grouping under internal history. We will discuss this idea in more detail in the next section.

Before moving on, though, let us note that Lakatos proposed another way of testing methodologies on the basis of historical facts, namely by way of a "methodology of *historiographical* research programmes", which obviously was supposed to mirror his methodology of scientific

[9] See also related critiques by Feyerabend (1976) and Laudan (1986b).
[10] This is the approach recommended by Donovan et al. (1988).

research programmes (Lakatos 1971, 116). Lakatos's reasons for switching from his first proposal to his second, are threefold, with the first of them being almost hilarious: (i) his own methodology of research programmes would also have to be falsified by the historical facts – obviously, that is not the best reason for the switch, (ii) the scientific community may want to reconsider their value judgment in case of a clash with methodology, (iii) if we give up falsificationism at the level of method, we should also give it up at the meta-level (Lakatos 1971, 116).

The outline of the approach is clear enough, with Lakatos putting most emphasis on a methodology having to have predictive success: "We should, of course, insist that a good rationality theory must anticipate further basic value judgments unexpected in the light of its predecessors or that it must even lead to the revision of previously held basic value-judgements" (116-117). In case of a *progressive shift* we would then replace one methodology by another. When it comes to the details, though I think Lakatos's approach is underspecified. In particular, it remains rather vague what would count as novel success for Lakatos. For example, he considers the already mentioned value judgment that it was fine for Newtonians not to reject their theory in the face of the advance of Mercury's perihelion as "novel success" on his own methodology of research programmes, simply because scientists' behaviour can be accommodated on it, whereas it cannot be on Popper's falsificationism. It remains unclear, though, what is supposed to be "novel" or "anticipated" about this.

Although Lakatos's proposals are beset with difficulties, there is one element of his account, which I believe is much more promising. I call it the "maximization of rational facts".

5 Maximization of rational facts and resistance to falsification

A core idea of Lakatos's methodology of historiographical research programmes is that methodologies can be criticized by "criticizing the rational historical reconstructions to which they lead" (109). As already mentioned in the previous section, this statement must at first seem puzzling, because it would appear circular or question-begging. But if internal history is just guided, rather than distorted, by methodologies, and if the goal is to maximize the number of historical facts that can be accommodated as rational within internal history, then the proposal is in fact quite plausible. First consider Lakatos himself on the idea of *maximization of rational facts*:

> An 'impressive', 'sweeping', 'far-reaching' external explanation is usually the hallmark of a weak methodological substructure; and, in turn, *the hallmark of a relatively weak internal history* (in terms of which most actual history is either inexplicable or anomalous) *is that it leaves too much to be explained by external history*. When a better rationality theory is produced, internal history may *expand and reclaim ground from external history*. (Lakatos 1970, 119; added emphasis)

This proposal provides an obvious way for conducting comparative tests of methodologies: if methodology A accommodates x historical facts as rational and methodology B accommodates y historical facts as rational, then if $y > x$, we should prefer methodology B. Because Lakatos presumes any facts that cannot be accommodated by a methodology will fall under external history, this would mean in this example that methodology B would minimize the number of historical facts that have to be treated as external 'irrational' history. In sum, the goal of philosophy of science on Lakatos's view must then be to construct methodologies that maximize the historical facts that come out as rational and to minimize the historical facts that come out as irrational.[11]

As an example, that Lakatos himself mentioned repeatedly, consider once more (and for a final time!) the advance of Mercury's perihelion, which was discovered in 1859. Newtonians could not account for it, but they did not reject their theory in the face of this anomaly. On the contrary, they kept their theory for another 56 years before Einstein provided them with a better alternative. As already mentioned, Popper's falsificationism has trouble dealing with such cases of "resistance to falsification", as one might call them. In contrast, Lakatos's account seems almost designed to deal with resistance to falsifiers: for Lakatos, it is rational to pursue a research programme so long as it is progressive and produces novel success. Empirical anomalies are being given much less weight in the development of the research programme than what Lakatos called "positive heuristic", that is, mostly theory-driven concerns (Lakatos 1978, 151). Lakatos also rejected crucial experiments and took the Duhem thesis to heart. Lakatos concludes:

> what for the falsificationist looks like the (regrettably frequent) phenomenon of irrational adherence to a 'refuted' or to an inconsistent theory and which he therefore relegates to *external* history, may well be explained in terms of my methodology *internally* as a rational defence of a promising research programme." (Lakatos 1971, 102; original emphasis)

Clearly, this third of Lakatos's proposals as to how to test philosophical theories is giving history a prominent role. It also avoids the charge of circularity or question-begging, because one can criticize a methodology on the basis of *the number* of facts it can explain rationally, in particular when there is another methodology that rationally explains *more* historical facts. At the same time, Lakatos was not naïve about rational reconstruction. He made clear that "the history of science is always richer than its rational reconstruction" (105) and that "no set of human judgments is completely rational and thus no rational reconstruction can ever coincide with actual history" (116). There could therefore never be a methodology that would render *all* historical facts rational.

The maximization of rational facts has another advantage that perhaps was not so apparent to even Lakatos himself: the norm-fact divide need no longer be bridged. That is because we are not directly testing philosophical norms against historical facts. Instead, we are asking what philosophical norms can account for the largest number of historical facts. There is thus no longer any need for the invocation of the problematic notion of basic value judgments (see previous section).

An objection one might raise against the maximization-of-rational-facts proposal is that it naively presumes an equality between all historical facts. But of course, some historical facts are more important than others. There will then surely have to be some kind of weighting of the historical facts and competing methodologies will have to be assessed on the basis of those. The weightings themselves may not indisputable, so there is still room for disagreement, of course.

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[11] I am certainly not the first to have noticed this idea of maximization of rational facts in Lakatos's account, although I perhaps give it more prominence here than others have before. See Kuhn (1980), Arabatzis (2017), Kuukkanen (2017), Schindler (2018), and Dimitrakos (2020).

And there might also be situations where one methodology accounts for one important set of facts and another does for another important set of facts, with only a slight overlap between the two. Even when there may be a numerical advantage for one methodology, it would not be clear that we should prefer the methodology with that numerical advantage. Of course, this need not be a devastating criticism of the maximization-of-rational-facts proposal: we simply should not expect ever to obtain some algorithmic procedure for arbitrating philosophical disputes anyway! The general idea of maximisation of rational facts seems sound enough.

There are more challenges to the maximization-of-rational-facts proposal, which have to do with the possibility that the historical facts may not be impartial and therefore not apt to distinguish between different methodologies in the way envisaged by Lakatos. To this and a related issue we will turn now in the context of current discussions in the meta-philosophy of science.

6 Lakatos's meta-philosophy in light of the contemporary literature

There are two major foci in contemporary meta-philosophical discussions concerning the use of historical case studies: one focus has been on the risk of case studies being used in tendentious ways when called upon to support philosophical claims (Pitt 2001, Schickore 2011, Kinzel 2015, Chakravartty 2017, Bolinska and Martin 2020); another focus has been the issue of how the apparent particularity of historical cases can be reconciled with the aspiration of generality of philosophical claims (Pitt 2001, Chang 2011, Bolinska and Martin 2020, Schindler and Scholl 2022). In what follows I want to refer to the first problem as the *problem of impartiality* and to the second problem as the *problem of inductive warrant*.[12]

6.1 The problem of impartiality

There are three versions of the problem of impartiality: bias, distortion, and theory-ladenness. These three aspects should be kept apart, because they have very different implications. As we already mentioned in section 3 of this paper, distortion should have no place in any sound empirical approach, philosophical or otherwise. I do not think that respected philosophers using case studies have made themselves suspect of the charge, even though they have been accused of it. For example, Pitt writes in his highly influential dilemma for the case study approach:

> On the one hand, if the case is selected because it exemplifies the philosophical point being articulated, then it is not clear that the philosophical claims have been supported, *because it could be argued that the historical data was manipulated to fit the point*. On the other hand, if one starts

[12] For completeness's sake, one should mention that there is another major meta-philosophical problem, which received some attention in the 1970s and 1980s, namely the problem of how to bridge the gap between philosophical *norms* and historical *facts*. Giere (1973) was the first to highlight this problem, but the current consensus seems to be that the problem disappears, either when we conceive of philosophical theories akin to scientific theories, that ought to be tested empirically (Giere 1985, Donovan et al. 1988), or when we conceive of methodologies as *instrumental norms*, whereby empirical facts need to tell us whether the set goals are actually achieved by the used methods (Laudan 1986a; 1990). For a criticism of the former option see Schickore (2011); for a criticism of the latter option see Schindler (2018).

> with a case study, it is not clear where to go from there—for it is unreasonable to generalize from one case or even two or three. (Pitt 2001, 373; added emphasis)

Possibly, Pitt means to say something weaker, namely that case studies are being cherry-picked by philosophers to fit their agendas. In that vein, Nickels once commented that “historical case studies can be too much like the Bible in the respect that if one looks long and hard enough, one can find an isolated instance that confirms or disconfirms almost any claim” (Nickles 1995, 141).

Just like science, philosophy has safeguards against bias and cherry-picking. The better journals of the field have (pretty stringent) peer-review procedures that ensure that people not positively inclined to the author’s thesis get a chance to criticize the author’s bias. Even if peer-review fails to catch biases, there is a critical community of philosophers which is bound to either bring the bias to the attention of their peers or to present their own case studies challenging the thesis in question. And if peer-review and community criticism is apt to detect biases, then this should be even more the case for distortion. Of course, there is no guarantee that all biases and distortions will be caught by the community, but there is also no reason whatsoever to think that the very project of using history in support of philosophical theorizing is so skewed that all hope is lost.

A more serious concern than bias is theory-ladenness. Theory-ladenness is best understood not just as the theoretically biased selection of facts, but rather as something deeper, namely, as the impingement of theoretical presuppositions on the very way that the facts are described. Even when philosophers would take great care not to distort the facts, historical case studies could then never provide neutral support for any philosophical view, because the same historical facts could be seen in this or that light, depending on one’s philosophical views (see Kinzel 2015). In fact, that was a concern that Kuhn expressed in his review of a collection of essays discussing historical cases in the spirit of Lakatos’s theoretical framework of research programmes (Howson 1976):

> 'actual history' of the sort Lakatos requires is a myth. … the data in most parts of the pool are not, until after much interpretation, the facts which appear in historical narratives. … It is by no means clear, however, that proponents of those [compared] methodologies would accept the elements of his narrative as simply factual, and it is upon that agreement that his demonstration depends. History is interpretative throughout. (Kuhn 1980, 184)

Interestingly, despite this concern, Kuhn concluded that “Lakatos is, I think, clearly right to suggest that improved historical narratives are often the ones that give a central role to a larger body of evidence” (Kuhn 1980, 184-185). Kuhn just thought that the writing of history should be left to the historians, so as to ensure that philosophers’ theories of science could not impinge on the writing of history in the problematic and distortive ways that Lakatos, regrettably, had suggested. Lakatos was not alive anymore, so he had not chance to respond this this suggestion by Kuhn. But I think we can be confident that he would have rejected this proposal, as he made clear already at the beginning of his meta-philosophical essay that he considered history without philosophy “blind”.

How severe a threat to Lakatos's idea of maximization of rational facts is the theory-ladenness of historical facts then? How severe a threat is theory-ladenness to *any* philosophy seeking to use history as evidence? Perhaps not much more severe than theory-ladenness of observations in science (see Kinzel 2015). For example, for the latter it has been argued that even when descriptions of observations are influenced by the vocabulary of the relevant philosophical theories, evidence can still be recalcitrant and act back against our theories. Kinzel is less optimistic that philosophical disputes can be arbitrated, because she believes that the very criteria for what may count as a good historical case study are likely to be theory-laden, for example, whether one thinks that social factors ought to be taken into consideration when constructing a case, or not (Kinzel 2015, 55).

Perhaps argument criteria such as coherence and cogency can help us settle even such more fundamental disputes, at least in principle (Bolinska and Martin 2020). In principle, it is also a matter of fact whether social factors are or are not required to explain a particular historical episode (ibid.). Furthermore, there are philosophical debates, in which the basic presuppositions are fairly widely shared. In such debates, the focus of discussion can be even be put more squarely on the relevant historical facts. For example, in the realism debate, it is widely agreed between realists and antirealists that the success of scientific ought to be explained by epistemic factors and *not* by social factors. Now, of course, there is still room for disagreement, but I think it would be wrong to suggest that *therefore* the historical facts are evidentially toothless (Chakravartty 2017). On the contrary, there are clear signs of progress that can be made by engaging with the historical details. For example, Psillos first argued that theorists of the caloric theory of heat did not embrace the reality of caloric as a substance when explaining heat phenomena and when deriving novel predictions (Psillos 1999). But *on the basis of a further engagement with the historical facts* it has been shown that this is actually incorrect (Chang 2003, Stanford 2006). This is widely accepted.

There are other prominent examples of cases where concerted efforts by the community of philosophers have resulted in deeper and richer understanding of selected historical cases and arguably also in a sense of philosophical progress. Take for example the famous case of Semmelweis and his discovery of the cause of child bed fever. The case has been discussed by several philosophers, for several purposes. Hempel first took it to nicely illustrate the hypothetico-deductive method (Hempel 1966). Lipton instead argued that Semmelweis in fact used the inference to the best explanation (Lipton 1991/2004). Others disagree. Scholl (2013) argues that Semmelweis's method is best described in non-explanationist terms, namely Stuart Mill's method of agreement and concomitant variation. Superficially, it may look as though the history lends itself to almost any philosophical interpretation one pleases. And yet, the concerted study of the Semmelweis case has allowed philosophers to compare their abstract ideas in relation to a concrete case, with the succession of authors making arguments for their accounts to be a *better* representation of the actual case and to explain *more* historical facts (see Schindler and Scholl (2022) for a discussion). Even if one disagrees that we know more now about the case and about what philosophical method best represents the case, the historical case provides a focal point which has enriched the philosophical discussion, and without which the discussion may have remained rather sterile and removed from actual practice.[13]

Cases like these should give us confidence that there can be a fruitful interplay between philosophical theorizing and engagement with the historical facts, and that philosophical biases can be corrected by aiming for a fuller historical picture. This is not to say, though, that simply doing a rich historical analysis, involving analysis of contextual factors, will always give us a "truer" picture of science.[14] On the contrary, without a focus on a specific philosophical question, I believe with Lakatos (and Hanson) that we are bound to move philosophically "blindly" in historical territory: it is just much harder (and sometimes impossible) to address philosophical problems with history that is written without some philosophical question in mind. Hanson once put it, perhaps not fully charitably, but not entirely off target either: "To the philosopher, histories of science are often unilluminating because, as a result of their chaotic diffuseness, they never reflect monochromatically: only spectra of concepts and arguments result" (Hanson 1962, 582).[15]

6.2 The problem of inductive warrant

Let us now turn to the problem of inductive warrant: on what grounds are philosophers warranted to claim inductive support from just a few selected historical case studies (also see Pitt's dilemma cited in the previous section)? On the face of it, the problem seems much more challenging than the problem of impartiality: even when all the safeguards of the community work well, there is still an issue of how philosophers are warranted to infer from few cases to general philosophical claims about science.

Philosophy is not science. Philosophy thus cannot help itself to the same, established means that science can, such as statistics, for tackling inductive inferences. True, experimental philosophers are using statistics when collecting intuitive judgments from non-philosophers (Knobe and Nichols 2017), but it is harder to see how this could work out with historical case studies. Obviously, not all cases are to be weighed equally, and constructing a single case in the first place, is very time-consuming and dependent on choices that are subject to disagreement. As we have just seen, though, even when there is focus on just a few or even a single case, engagement with the history of science can be very fruitful for philosophical arguments and insight.

There is a less demanding look upon the kind of inductive support philosophy of science requires. Philosophical theories are often taken to be "generalizing", as in over-generalizing without inductive support (Pitt 2001). But in reality, philosophers are not as naïve as they are sometimes made out to be by the critics. For example, in the literature on explanation, causal

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[13] For another example of the same sort, see the discussion of the discovery of the weak neutral current in high energy physics (Schindler 2014).
[14] This is what both Pitt himself, and Burian in his reply to Pitt's dilemma, seem to imply (Burian 2001, Pitt 2001).
[15] In all fairness, Hanson had similarly nice things to say about the work of philosophers: "To the historian such philosophy of science is often unilluminating because it does not enlighten one about any *thing*: nothing in the scientific record book is treated in such symbolic studies" (Hanson 1962, 582).

models of explanation have enjoyed an enormous popularity in the past few decades (Woodward 2003). But even though proponents of these models obviously take causal explanation to be a widely used form of explanation in science, there is no pretence that causal explanation is the only explanation there is in science (Reutlinger and Saatsi 2018). The same is true for the inference to the best explanation, which, as mentioned, has been motivated through historical case studies. Again, even though the inference to the best explanation is considered to be a widely used and important mode of inference, no philosopher would be silly enough to claim that it is the only kind of inference used in the sciences.

Sometimes it is claimed that the history of science is so particular that it allows for no generalizations *whatsoever*.[16] But this is a claim that requires at least as much argument as the claim that there are at least *some* patterns and generalities that can be drawn from the history of science. Indeed, it would be surprising if the success of science was somehow based on complete happenstance and chance events. One is of course free to throw up one's hands and give up on the project of historically informed philosophy of science. But it is surely much more constructive to assume that the pessimistic view is false and to go to investigate philosophical theses about science under consideration of the history of science.

6.3 Lakatos and the two problems

Prima facie, Lakatos's core idea of the maximization of rational facts is threatened both by the problem of impartiality and the problem of inductive warrant, which both have been discussed extensively by the contemporary meta-philosophical literature. Yet, as we have seen in this section, bias is well-manageable by the profession of philosophers. It was Lakatos's view anyway, that bias could easily be controlled for by the number of facts that could be accounted for by a methodology's rational, internal history. Distortion, despite Lakatos's earlier slip of the tongue, should have no place in any respected academic discipline, as Lakatos later himself seemed to realize. Theory-ladenness is by far the most serious threat not only to Lakatos's project of rational reconstruction, but to *any* meta-philosophical view that seeks to combine philosophical theorizing and historical facts. Yet, as we have seen, there is reason for optimism: if theory-ladenness in science is no reason for despair, then it should not be in philosophy either. And even if philosophy should somehow be worse off than science, there are still some examples where the study of history *has* given us a better understanding of the philosophical questions posed. Finally, the problem of inductive warrant seems more severe than the problem of impartiality, but it of course besets again not only Lakatos's approach, but *any* approach seeking to combine philosophy of science and history of science. There is also reason to think that philosophers are much more careful in their generalisations than the critics often have it.

7 Rationality in Lakatos and Kuhn

In the previous two sections we have been concerned with Lakatos's idea of the maximization of rational facts and how it compares to current work in the meta-philosophy of science. Throughout

[16] Pitt (2001) calls this the "Heraclitan view" of the history of science. See also Bolinska and Martin (2020) for a discussion.

this paper, we have been concerned with the question of how philosophical accounts of science may be assessed on the basis of historical facts. However, one may also ask about whether a philosophical account is internally coherent and what kind of rationality it entails by itself. This is the kind of question we shall now turn to. To assess Lakatos's account with regard to this question, we shall compare it to Kuhn's account of science, which, as we shall see, Lakatos was quite substantially inspired by. Contrary to what Lakatos claimed, his account cannot be said to be an advance over Kuhn's in terms of rationality.

7.1 Resistance to falsification, again

In section 5 we saw that what we referred to as "resistance to falsification" played a central role in Lakatos's own assessment of philosophical methodologies, and more specifically, both in his arguments against falsificationism and in his arguments favour of his own methodology of research programmes. It is interesting to note that the importance of resistance to falsification seems to have been impressed upon Lakatos by Kuhn. There is some circumstantial evidence for this, which can be found the precursor of his seminal "Falsification and the Methodology of Scientific Research Programmes" (Lakatos 1970), namely a paper with the title "Criticism and the Methodology of Scientific Research Programmes", which was published in the *Proceedings of the Aristotelian Society* (Lakatos 1968). In the first section of this precursor, titled "Popper vs. Kuhn", Lakatos contrasts Popper's account of science to Kuhn's. The former he describes thus: "Boldness in conjectures on the one hand and austerity in refutations on the other: this is Popper's recipe" (150). "Commitment" to a theory despite counterexamples, on the other hand, Lakatos describes as being "an outright crime" for Popper (ibid). Kuhn, on the other hand, took commitment (to a paradigm) to be central to science. Lakatos explains that for Kuhn "the transition from criticism to commitment marks the point where progress -and 'normal' science-begins" (ibid.).[17] Lakatos concludes that the debate between Popper and Kuhn is "not merely over a technical point in epistemology" but rather "'over our central intellectual values" (151).

In the second section of the precursor to the *Methodology* paper, Lakatos sought to develop less "naïve" versions of Popper's account (which he characteristically referred to with subscripts: $Popper_1$, $Popper_2$, $Popper_3$), ultimately resulting in his methodology of research programmes. As the "main difference" between Popper's falsificationism and his own account Lakatos tellingly described that the "criticism [of theories] does not – and must not – kill as fast as Popper imagined" (Lakatos 1968, 183). Later, in his final version of his *Methodology* paper, Lakatos would attribute to Kuhn the insight that falsificationism fails as a "rational account of scientific growth" (Lakatos 1970, 93).

There is even further, more direct evidence that Lakatos was inspired by Kuhn's highlighting of resistance to falsification in the history of science. In the unpublished precursor to the precursor of the *Methodology* paper, dated 1967 and stored in the Lakatos Achieve at the LSE, Lakatos speaks of "Kuhn's thesis that 'theories are born refuted'" (8). He also notes that if the thesis is correct, "then refutations play no dramatic role in science … The [Popperian] slogan 'make sincere

[17] However, see my forthcoming paper *blinded for review.*

attempts to refute your theories', falls flat if any new theory emerges in an ocean of counterexamples" (8).

7.2 Other parallels

Arguably, resistance to falsification was not the only inspiration that Lakatos took from Kuhn. Kuhn in fact once noted, in his comments on Lakatos's meta-philosophical paper – which has been the main subject of the current paper – that "I have read no paper on scientific method which expresses opinions so closely paralleling my own" (Kuhn 1971, 137). In particular, Kuhn believed that Lakatos' hard core, protective belt, and a programme's "degenerative phase" were analogous to his notion of a paradigm, normal science, and crisis, respectively (Kuhn 1970, 256), and he voiced his frustration that Lakatos was "so unable to see" these parallels (Kuhn 1971, 139). Let us have a closer look at these points in turn.

First, Kuhn thought that Lakatos's choice of basic analytical unit of a research programme was not so different at all from his unit of a paradigm (Kuhn 1971, 138), which is fair because both are more encompassing than theories or hypotheses. Kuhn also believed that Lakatos's notion of a hard core of research programs really differed little from how he had described paradigms, namely as containing "elements which are not themselves subject to attack" (Kuhn 1971, 138). Second, although Kuhn did not mention this, Lakatos's notion of positive heuristic bears some similarities to Kuhn's notion of normal science: the former helps to develop a research programme by guiding scientists in their articulation of the 'protective belt'. In normal science, practitioners strive to increase the scope and the precision of a paradigm. Both of these activities are carried out while taken as given the hard core / basic assumptions of the paradigm, and neither activity is unseated by the discovery of anomalies.

Third, with regard to the phenomenon of resistance to falsification, Lakatos thought that as long as research programmes are "progressive", and as long as no better alternatives are available, scientists do not and should not abandon them (Lakatos 1970, 100, 130, and 176-7). How does Lakatos compare here to Kuhn? Kuhn offers *two* (albeit closely related) rationales for why it might be rational for scientists to resist falsification during periods of normal science: (i) scientists would not get much work done if they constantly let themselves bogged down by anomalies, rather than addressing those that the paradigm can solve (Kuhn 1962/1996, 82) and (ii) since, according to Kuhn, *all* theories are contradicted by *some* data, we could never embrace *any* theory if we would view a theory to be falsified whenever the theory's predictions did not match the evidence (Kuhn 1962/1996, 146). We may refer to those two rationales as "efficiency" and "pragmatism", respectively. Lakatos clearly also adopted the second rationale (Lakatos 1970, 120, fn. 1). He does not say anything resembling the first rationale; but he does not say anything that would contradict it either.

Despite these apparent parallels between Lakatos's and Kuhn's accounts of science, Lakatos did not have much positive to say about Kuhn. The few comments that Lakatos makes about Kuhn in his *Methodology* paper are negative and focused on theory change. For example, Lakatos described paradigm change as "a mystical conversion which is not and cannot be governed by rules of reason … [It is] a kind of religious change" (Lakatos 1970, 93). Furthermore, Lakatos noted that "there is no particular rational cause for the appearance of a Kuhnian 'crisis'. 'Crisis' is a psychological concept; it is a contagious panic. … Thus in Kuhn's view scientific revolution is irrational, a matter for mob psychology" (Lakatos 1970, 187).

Even though Lakatos's charge of irrationality concerning paradigm change was perhaps not entirely off target, Kuhn vehemently protested against what he took to be a caricature of his views. First of all, he argued that paradigm crisis was in fact not at all dissimilar to degenerative phases of research programmes, because also for paradigm crisis to occur, there needs to be a piling up of anomalies (Kuhn 1971). Second, Kuhn pointed out that, although he indeed did not believe that theory-choice was governed by rules, he believed that theory-choice was guided by standard criteria of theory choice, and based on the value judgments by scientists (Kuhn 1970). Kuhn also pointed out that Lakatos's account required several 'decisions' (in the parlance that Popper and Lakatos preferred) by the scientific community, which he took to be similar to the value judgments in theory choice (Kuhn 1970, 238-240). For example, what are the assumptions that belong to the hard core and which assumptions are part of the protective belt? When is a research programme progressive and when is it degenerating? The second question is particularly problematic, because, even though Lakatos presented a *prima facie* clear-cut criterion for progress in the form of novel success, good research programmes can be degenerating, and thus fail to generate novel success, for a long time (see e.g., Feyerabend 1976). For example, one of the earliest, most impressive scientific achievements, namely the Copernican research program, was degenerating for no less than a hundred years (!) according to Lakatos's own criteria of progress, before it would be salvaged by the likes of Galileo and Newton (Lakatos and Zahar 1978).

Being aware of such cases, Lakatos insisted that his account did not offer "instant rationality" and that research programs must be treated "leniently" in their early stages (Lakatos 1970, 179). He also suggested that "it is perfectly rational to play a risky game: what is irrational is to deceive oneself about the risk" (Lakatos 1971, 104, footnote). In other words, it is not irrational for scientists to pursue a research programme, even when it is degenerative, because the programme might ultimately turn out to be progressive. But one should be aware that one is taking a risk, particularly when another research programme is available that is progressive. This all seems reasonable, but Lakatos thus fails himself to supply the "rules of reason" that he criticized Kuhn for not providing.[18]

In recent years, Lakatos's preferred criterion of measuring success of research programmes, namely novel success, has come under attack. Even Lakatos – perhaps through the works of Zahar on the topic – was not committed to research programmes necessarily having temporarily novel success. But even a weaker criterion like "use-novelty" (Worrall 1989a; 2014), according to which a piece of evidence is novel if it was not used in the construction of a theory, comes with all kinds of

[18] Like Popper, Reichenbach and others, Lakatos thought the context of discovery was the domain of psychology and sociology, but not of the philosophy of science. Instead, he thought philosophers ought to be concerned with finding normative rules for the "appraisal of ready, articulated theories" (Lakatos 1971, 92). Confusingly, though, he spoke of proposals for those rules as "logics of discovery" (ibid.).

problems (Schindler 2018). So it is not clear whether novel success can at all serve the meta-philosophical purpose assigned to it by Lakatos in the assessment of research programmes.

All in all, apart from Kuhn's highly controversial notion of incommensurability of paradigms, I think there is not a single dimension where one could say that Lakatos offered a more rational reconstruction than Kuhn did. There do not seem to be any historical facts that Lakatos, but not Kuhn, managed to explain rationally either. Most notably, both Kuhn and Lakatos rationally explained the resistance to falsification. By his own account of rationality, Lakatos thus failed to provide a better "methodology" than Kuhn.

8 Conclusion

This paper argued that despite his notorious footnote quote, Lakatos actually had quite reasonable things to say about the meta-philosophical question of how to use historical facts in philosophical theorizing. In particular, the idea of the maximization of rationally explainable facts strikes me as a decent project for a historically informed philosophy of science. This kind of philosophy of science has admittedly gone out of fashion. One can only speculate about the reasons why. One concern Lakatos and his contemporaries shared was the demarcation problem: what is it that characterizes science? Popper obviously had his answer, and so did Kuhn and Lakatos (Lakatos 1978). The consensus today appears to be in many quarters that solving the demarcation problem is hopeless and misconceived: there are just too many dissimilarities between the sciences for one to look for an answer that "fits all".[19] This concern is related to another common contemporary view, namely that the methods used by science are very diverse *even within a single discipline*, so it may appear hopeless to assess the rationality of "the" scientific method (Laudan 1977).[20] Second, philosophers of science seem to have largely lost interest in the diachronic dimension of science, to which the study of history is indispensable.[21] Whether one thinks that these trends are to be welcomed or regrettable, Lakatos's work still provides a rich resource for thinking about how the philosophy of science could fruitfully be engaged with the history of science.

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[19] But see Pigliucci and Boudry (2013) and Schindler (2018) for recent revival attempts.

[20] But see the intriguing discussion between Laudan and Worrall, who defends the view of "fixed methodology" (Worrall 1988, Laudan 1989, Worrall 1989b).

[21] One exception to this trend is the realism debate, and in particular the pessimistic meta-induction, where philosophers of science have quite closely engaged with the history of science (although, again, without much focus on the diachronic element of theories).

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