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The (Lack of) Evidence for the Kuhnian Image of Science

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Whenever the work of an influential philosopher is criticized, a common move made by those who seek to defend the influential philosopher's work is to claim that his or her ideas have been misconstrued. This is an effective move, of course, for it means that the critics have criticized a straw man, not the ideas actually put forth by the influential philosopher. However, this move can easily backfire, too.

For continued iterations of this move could render the ideas in question immune to criticism in a rather *ad hoc* fashion. That is to say, shouting "straw man" every time an influential philosopher's ideas are subjected to scrutiny is rather like shouting "wolf" when none is around; it could be seen as an attempt to draw attention to that which may not be worthy of attention.

The question, then, is whether the influential philosopher's ideas are worthy of attention and/or acceptance. In particular, are Kuhn's ideas about scientific revolutions and incommensurability worthy of acceptance? As I have argued, along with a few other contributors to my edited volume, *The Kuhnian Image of Science: Time for a Decisive Transformation?* (2018), they may not be because they are based on dubious assumptions and fallacious argumentation.

In their reviews of *The Kuhnian Image of Science: Time for a Decisive Transformation?* (2018), both Markus Arnold (2018) and Amanda Bryant (2018) complain that the contributors who criticize Kuhn's theory of scientific change have misconstrued his philosophy of science and they praise those who seek to defend the Kuhnian image of science. In what follows, then, I would like to address their claims about misconstruing Kuhn's theory of scientific change. But my focus here, as in the book, will be the *evidence* (or lack thereof) for the Kuhnian image of science. I will begin with Arnold's review and then move on to Bryant's review.

Arnold on the Evidence for the Kuhnian Image of Science

Arnold (2018, 42) states that "one of the results of [his] review" is that "the 'inductive reasoning' intended to refute Kuhn's incommensurability thesis (found in the first part of the book) is actually its weakest part." I am not sure what he means by that exactly. First, I am not sure in what sense inductive reasoning can be said to *refute* a thesis, given that inductive arguments are the sort of arguments whose premises do not *necessitate* the truth of their conclusions, whereas a refutation of *p*, if sound, supposedly shows that *p* must be false.

Second, contrary to what Arnold claims, I do not think that the chapters in Part I of the book contain "'inductive reasoning' intended to refute Kuhn's incommensurability thesis" (Arnold 2018, 42). Speaking of my chapter in particular, Chapter 1 (Mizrahi 2018b, 32-38), it contains two arguments intended to show that there is no deductive support for the Kuhnian thesis of taxonomic incommensurability (Mizrahi 2018b, 32), and an argument intended to show that there is no inductive support for the Kuhnian thesis of taxonomic incommensurability (Mizrahi 2018b, 37).

These arguments are *deductive*, not inductive, for their premises, if true, guarantee the truth of their conclusions. Besides, to argue that there is no evidence for p is not the same as arguing that p is false. None of my arguments is intended to show that p (namely, the Kuhnian thesis of taxonomic incommensurability) is false.

Rather, my arguments show that there is no evidence for p (namely, the Kuhnian thesis of taxonomic incommensurability). For these reasons, as a criticism of Part I of the book, Arnold's (2018, 42) claim that "the 'inductive reasoning' intended to refute Kuhn's incommensurability thesis (found in the first part of the book) is actually its weakest part" completely misses the mark.

Moreover, the only thing I could find in Arnold's review that could be construed as support for this claim is the aforementioned complaint about straw-manning Kuhn. As Arnold (2018, 43) puts it, "the counter-arguments under consideration brought forward against his model seem, paradoxically, to underestimate the complexity of Kuhn's claims."

In other words, Kuhn's theory of scientific change is so complex and those who attempt to criticize it fail to appreciate its complexity. But why? Why do the criticisms fail to appreciate the complexity of Kuhn's theory? How complex is it such that it defies interpretation and criticism? Arnold does not say. Instead, he (Arnold 2018, 43) states that "it is not clear, why Kuhn's 'image of science' should be dismissed because [...] taxonomic incommensurability 'is the exception rather than the rule' [Mizrahi 2018b,] (38)."

As I argue in Chapter 1, however, the fact that taxonomic incommensurability "is the exception rather than the rule" (Mizrahi 2018b, 38) means that Kuhn's theory of scientific change is a *bad theory* because it shows that Kuhn's theory has neither explanatory nor predictive power. A "theory" with no explanatory and/or predictive power is no theory at all (Mizrahi 2018b, 37-38). From his review, however, it is clear that Arnold thinks of Kuhn's image of science as a theory of scientific change.

For instance, he talks about "Kuhn's epistemology" (Arnold 2018, 45), "Kuhn's theory of incommensurability" (Arnold 2018, 46), and Kuhn's "complex theory of science" (Arnold 2018, 42). If Kuhn's thesis of taxonomic incommensurability has no explanatory and/or predictive power, then it is a bad theory, perhaps not even a theory at all, let alone a general theory of scientific knowledge or scientific change.

In that respect, I found it rather curious that, on the one hand, Arnold approves of Alexandra Argamakova's (2018) criticism of the universal ambitions of Kuhn's image of science, but on the other hand, he wants to attribute to Kuhn the view that "scientific revolutions are rare" (Arnold 2018, 43). Arnold quotes with approval Argamakova's (2018, 54) claim that "distinct breakthroughs in science can be marked as revolutions, but *no universal system of criteria for such appraisal can be formulated in a normative philosophical manner*" (emphasis added).

In other words, if Argamakova is right, then there can be no philosophical theory of scientific change in general, Kuhnian or otherwise. So Arnold cannot be in agreement with Argamakova without thereby abandoning the claim that Kuhn's image of science is an "epistemology" (Arnold 2018, 45) of scientific knowledge or a "complex theory of science" (Arnold 2018, 42).

Arnold (2018, 45) also asserts that "the allegation that Kuhn developed his theory on the basis of selected historical cases is refuted" by Kindi (2018). Even if that were true, it would mean that Kuhn's theory has no inductive support, as I argue in Chapter 1 of the book (Mizrahi 2018b, 32-38). So I am not sure how this point is supposed to help Arnold in defending the Kuhnian image of science. For if there is no inductive support for the Kuhnian image of science, as Arnold seems to think, and there is no deductive support either, as I (Mizrahi 2018b, 25-44) and Park (2018, 61-74) argue, then what evidence is there for the Kuhnian image of science?

For present purposes, the important point is not *how* Kuhn "developed his theory" (Arnold 2018, 45) but rather what *supports* his theory of scientific change. What is the evidence for a Kuhnian theory of scientific change? If I am right (Mizrahi 2018b), or if Park (2018) is right, then there is neither deductive support nor inductive support for a Kuhnian theory of scientific change. If Argamakova is right, then there can be no general theory of scientific change at all, Kuhnian or otherwise.

It is also important to note here that Arnold (2018, 45) praises both Kindi (2018) and Patton (2018) for offering "a close reading of Kuhn's work," but he does not mention that they offer incompatible interpretations of that work, specifically, of the evidence for Kuhn's ideas about scientific change. On Kindi's reading of Kuhn, the argument for the Kuhnian image of science is a deductive argument from first principles, whereas on Patton's reading of Kuhn, the argument for the Kuhnian image of science is an inference to the best explanation (see Patton 2015, cf. Mizrahi 2018a, 12-13; Mizrahi 2015, 51-53).

Bryant on the Evidence for the Kuhnian Image of Science

Like Arnold, Bryant (2018, 1) wonders whether Kuhn's views on scientific change can be pinned down and criticized or perhaps there are many "Thomases Kuhn." Again, I think we do not want to make Kuhn's views too vague and/or ambiguous (Argamakova 2018, 47-50), and thus immune to criticism in a rather *ad hoc* fashion. For that, in addition to being based on dubious assumptions and fallacious argumentation, would be another reason to think that Kuhn's views are not worthy of acceptance.

Bryant (2018, 1) also wonders "whether the so-called Kuhnian image of science is really so broadly endorsed as to be the potential subject of (echoing Kuhn's own phrase) a 'decisive transformation'." As I see it, however, the question is not whether the Kuhnian image of science is "broadly endorsed." Rather, the question is whether "we are now possessed" by it. When Kuhn wrote that (in)famous first line of the introduction to *The Structure of Scientific Revolutions*, the image of science by which we were possessed was a positivist image of

science according to which science develops “by the accumulation of individual discoveries and inventions” (Kuhn 1962/1996, 2). Arguably, philosophers of science were never possessed by such a positivist image of science as much as they are possessed by the Kuhnian image of science.

This is evidenced by the fact that no positivist work in philosophy of science has had as much impact as Kuhn’s seminal work (Mizrahi 2018a, 1-2). Accordingly, even if the Kuhnian image of science is not “broadly endorsed,” it is quite clear that philosophers of science are possessed by it. For this reason, an “exorcism,” or a “decisive transformation,” is required in order to rid ourselves of this image of science. And what better way to do so than by showing that it is based on dubious assumptions and fallacious argumentation.

As far as the evidence (or lack thereof) for the Kuhnian image of science, Bryant (2018, 2) claims that “Case studies can be interesting, informative, and *evidential*” (emphasis added). I grant that case studies can be interesting and informative, but I doubt that they can be *evidential*. From “Scientific episode *E* has property *F*,” it does not follow that *F* is a characteristic of scientific episodes in general. As far as Kuhn is concerned, it is clear that he used just a few case studies (e.g., the phlogiston case) in support of his ideas about scientific change and incommensurability.

The problem with that, as I argue in Chapter 1 of the book (Mizrahi 2018b, 32-38), is that no general theory of scientific change can be derived from a few cherry-picked case studies. Even if we grant that the phlogiston case is a genuine case of a so-called “Kuhnian revolution” and taxonomic incommensurability, despite the fact that there are rebutting defeaters (Mizrahi 2018b, 33-36), no general conclusions about the nature of science can be drawn from one (or even a few) such cases (Mizrahi 2018b, 36-37).

From the fact that one (or a few) cherry-picked episode(s) from the history of science exhibits a particular property, it does not follow that all scientific episodes have that property; otherwise, from the “Piltdown man” episode we would have to conclude that fraud characterizes scientific discovery in general (Mizrahi 2018b, 37-38).

Speaking of scientific discovery, Bryant (2018, 2) takes issue with the fact that I cite “just two authors, Eric Oberheim and Paul Hoyningen-Huene, who use the language of discovery to characterize incommensurability.” For Bryant (2018, 2), this suggests that “it isn’t clear that *the assumption* Mizrahi takes pains to reject is particularly widespread” (emphasis added). I suppose that “the assumption” in question here is that Kuhn “discovered” incommensurability.

If so, then I would like to clarify that I mention the fact that Oberheim and Hoyningen-Huene talk about incommensurability in terms of discovery, and claim that Kuhn “discovered” it, *not* to argue against it (i.e., to argue that Kuhn did not discover incommensurability), but rather to show that some of the elements of the Kuhnian image of science, such as incommensurability, are sometimes taken for granted. When it is said that

someone has discovered something, it gives the impression that what has been discovered is a fact, and so no arguments are needed.

When it comes to incommensurability, however, it is far from clear that it is a fact about scientific change, and so good arguments are needed in order to establish that episodes of scientific change exhibit taxonomic incommensurability. If I am right, or if Park (2018) and Sankey (2018) are right, then there are no good arguments that establish this.

Not Conclusions, But Questions

In light of the above, I think that the questions raised in the edited volume under review remain urgent (cf. Rehg 2018). Are there good reasons or compelling evidence for the Kuhnian model of theory change in science? If there are no good reasons or compelling evidence for such a model, as I (Mizrahi 2018b), Park (2018), and Sankey (2018) argue, what's next for philosophers of science? Should we abandon the search for a general theory of science, as Argamakova (2018) suggests? Are there better models of scientific change? Perhaps evolutionary (Marcum 2018) or orthogenetic (Renzi and Napolitano 2018) models?

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