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The Role of Evidence in Judging Kuhn's Model: On the Mizrahi, Patton, Marcum Exchange

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I would like to thank James H. Collier, executive editor of *Social Epistemology,* for the invitation to contribute to the most interesting dialogue which has been occasioned by Moti Mizrahi's paper "Kuhn's Incommensurability Thesis: What's the Argument?" My view is very different from the dominant one in the dialogue regarding Kuhn's account of science as developed in his *The Structure of Scientific Revolutions* and in his later work.

I think that Kuhn did not advance an empirical hypothesis that is in need of empirical support from the history of science in the form of an inductive (Mizrahi) or an abductive argument (Patton). So, in my view, evidence that presumably shows continuity rather than discontinuity (an implication of incommensurability) in the development of science cannot disqualify Kuhn's model. I have presented my account in Kindi (2005), but I will repeat some of the points here in connection to the discussion under consideration. The other charge that there is no valid deductive argument for Kuhn's incommensurability thesis and, in particular, taxonomic incommensurability, fails to undermine Kuhn's model and, what is more, proceeds by ignoring what Kuhn had actually said. This is a common attitude towards Kuhn's work. Rather than taking into account what he actually has said, many scholars prefer to attack a certain stereotype of Kuhn's views that has been shaped by the prevalent controversies in the literature.

No Deductive Support

Let me begin by the charge leveled by Mizrahi that there is no valid deductive argument for taxonomic incommensurability. What would a valid deductive argument establish? It would establish the conclusion that two theories are taxonomically incommensurable if certain conditions hold. The conditions do not have to actually hold. They only need to be assumed to hold. So a valid deductive argument in favor of taxonomic incommensurability would say that if such and such is supposed to be the case (premises), then we would speak of taxonomic incommensurability (conclusion). The whole argument is like an extended definition.

What is the argument that Mizrahi considers? The argument he uses runs as follows: "If competing theories were taxonomically commensurable, then terms would still refer to the same things in new theories" (premise 1) and if "Terms do not refer to the same things in new theories" (premise 2), then "Competing theories are taxonomically incommensurable" (conclusion). In what sense can a valid argument like that be asked to provide support for Kuhn's taxonomic incommensurability? The only thing it can do is to propose a definition of what Kuhn calls taxonomic incommensurability. It can provide support for taxonomic incommensurability if we require it to be not just valid but also sound. In this case the premises would not just be surmised to be true but required to be actually true.

One way to understand Mizrahi's approach is to take him to demand just that, namely, that Kuhn had a sound argument to defend taxonomic incommensurability. Mizrahi's

objection then would raise concerns regarding the truth of the premises. Do terms refer to the same things in new theories? But Mizrahi does not do that. He assumes that incommensurability implies incompatibility. He then finds two theories (folk taxonomy and biological taxonomy) which are said to be compatible despite (or because of) the fact that one and the same term refers to two different things in the two theories—the term 'kid' refers to child in folk taxonomy and to goat in biological taxonomy—and concludes from this that these two theories are not incommensurable.[1] So, Kuhn's argument (which takes incommensurability to imply incompatibility) cannot go through. It cannot support taxonomic incommensurability based on referential change.

It seems to me that Kuhn and Mizarahi understand the connection between incommensurability and incompatibility differently, as Marcum (2015) also notes. When Kuhn says that incommensurable theories are logically incompatible what he means is that one is not a logical implication of the other. Incommensurable theories make different predictions, have different concepts, and one cannot be derived from the other. He also means that incommensurable taxonomies do not match. They cannot be mapped one onto the other. He does not mean to say, as I take it Mizrahi does, that two incommensurable theories cannot coexist. Speaking generally, incommensurable theories can be perfectly compatible (cf. Achinstein 1968, 92) since they talk about different things. An ethical theory (e.g., utilitarianism) which talks about human beings and a scientific theory which also talks about human beings (e.g., Darwinism) are incommensurable (their taxonomies do not match) but they are still compatible in the sense that they do not come into conflict (they chart different areas of discourse) and one can entertain them both.

Two incommensurable theories become logically incompatible in case we want a single account for a certain domain. For instance, in a court of law the legal framework which includes legal judgments and legal concepts is incommensurable and incompatible, when it comes to verdict, with a psychological framework which aims to explain an individual's behavior in psychological terms. The verdict will be considered and issued in legal terms. In the physical sciences (unlike, for instance, in economics), we want in general just one theory to account for physical phenomena. In that case two incommensurable accounts (e.g., Newton's and Einstein's) are also incompatible. They cannot both be used at the same time to account for physical phenomena. And, as Kuhn argued in *Structure* (101-102), Newtonian dynamics cannot be derived from relativistic dynamics because "the physical referents of these Einsteinian concepts [space, time, mass] are by no means identical with those of the Newtonian concepts that bear the same name." The two theories cannot coexist by making one a special case of the other[2]. The old one has to be dropped so that the new one is accepted.

So, finding a term with different referents in two taxonomies, does not by itself show that the taxonomies are compatible and, therefore, not incommensurable in the sense that

[1] Mizrahi (2015a,15, n. 11) writes: "Although 'kid' refers to a young goat in animal taxonomy and to a child in ordinary language or folk taxonomy, it also refers to both in these conceptual frameworks, which is why there is no conceptual incompatibility, and hence no TI, in this case, either".

[2] According to Kuhn (*SSR,* 101-104), Newtonian dynamics can be seen as a special case of relativistic dynamics only if it is reinterpreted and transformed from the perspective of Einstein's theory.

Kuhn understands the terms. Also, incommensurable theories need not be in actual conflict as Mizrahi assumes[3]. What two incommensurable theories lack is a common measure and this judgment about them can be made irrespectively of whether they were ever in actual conflict.

Referential Discontinuity

In the same section of the paper where the absence of valid deductive support is considered, Mizrahi discusses one other possibility: that deductive support may come from considering referential discontinuity rather than referential change. Mizrahi's objection is that in such a case, i.e., in case old concepts and terms are dropped and replaced by others, we don't have to infer that two successive theories are incompatible and therefore incommensurable. The newer theory may just supplement the older one. For instance, even if we accept that phlogiston exists, "it could still be the case that there is oxygen as well" (2015a, 6). According to Mizrahi, Lavoisier's theory of combustion, which makes use of the term 'oxygen', may actually be "a useful addition" to Stahl's theory of combustion which uses the term 'phlogiston' which is dropped in Lavoisier's theory and has ceased to refer.

Now Kuhn, already in *Structure* considers this possibility. Distinguishing between new discoveries and new theories which may usher in scientific revolutions, he acknowledges that:

> In principle, a new phenomenon might emerge without reflecting destructively upon any part of past scientific practice (*SSR,* 95).
>
> [A] new theory does not have to conflict with any of its predecessors. It might deal exclusively with phenomena not previously known (*SSR,* 95).

Kuhn, however, dismisses this possibility:

> Cumulative acquisition of unanticipated novelties proves to be an almost non-existent exception to the rule of scientific development (*SSR,* 96).
>
> [C]umulative acquisition of novelty is not only rare in fact but improbable in principle (*SSR,* 96).
>
> Though logical inclusiveness remains a permissible view of the relation between successive scientific theories, it is a historical implausibility (*SSR,* 98).

Why does Kuhn say this? What are his arguments? First he raises the following questions:

[3] In the arguments Mizrahi gives he speaks of "competing theories" and questions whether Aristotelian physics and Newtonian physics are competing theories.

> Granting that paradigm rejection has been a historic fact, does it illuminate more than human credulity and confusion? Are there intrinsic reasons why the assimilation of either a new sort of phenomenon or a new scientific theory must demand the rejection of an older paradigm (*SSR,* 95)?

Kuhn wants to examine why one would insist on discontinuity rather than continuity in science, of replacing rather than supplementing a previous theory or paradigm, given that accumulation is not logically precluded. One would expect that since Kuhn acknowledges that accumulation is not an impossibility, his arguments in favour of discontinuity would be empirical; that he would resort to history and say that logical possibility notwithstanding, he has historical evidence to show that new theories displace old ones and do not supplement them. In fact, Kuhn starts out like this. He says that "[t]hough logical inclusiveness remains a permissible view of the relation between successive scientific theories, it is a historical implausibility" (*SSR,* 98).

He proceeds, however, to give not historical but *a priori* reasons in favour of discontinuity. The stress on a priori reasons is also highlighted in a later paper (Kuhn 2000a, 111-112) where Kuhn says that his generation of philosophers were under the impression that they were building their philosophical theories based on the observation of historical facts. Only later, did he realize, he says, that this picture is misleading. They could derive many of their central conclusions from first principles. This is virtually the same thought expressed in *Structure* (96): "If, however, resistant facts can carry us that far [to question cumulativeness], then a second look at the ground we have already covered may suggest that cumulative acquisition of novelty is not only rare in fact but improbable in principle,"

So what are the *a priori* arguments Kuhn gives in *Structure* in favour of discontinuity in science, which is another way of saying that scientific revolutions occur and that consecutive paradigms or theories in scientific development are incommensurable? He begins by saying that "there is increasing reason to wonder whether [the ideal image of cumulation in science] can possibly be an image of *science"* (emphasis in original). What he means is that it is in the nature of science to develop discontinuously[4]. That is why Chapter IX in which these issues are discussed is entitled "The Nature and Necessity of Scientific Revolutions". He explains what scientific revolutions are and argues why they have to occur—they don't just happen to occur—in science. So, why do they have to occur?

The first argument he gives makes a logical point. He says that "there must be a conflict between the paradigm that discloses anomaly and the one that later renders the anomaly lawlike". Anomalies emerge, according to Kuhn, only against a background of normalcy that has been established by following a paradigm in normal science. Only people who have been trained to know what to expect, who know what is normal, can detect irregularities. These irregularities will be eliminated in the new theory, which means that they will be rendered normal by being assimilated and by becoming part of the new

[4] This statement may be taken to imply that Kuhn is engaged in an essentialist project trying to find the nature of science. In Kindi (2005) I argue, instead in favour of a transcendental account.

system. The two frameworks then, old and new, cannot be logically connected since anomalies in the old become normal cases in the new. [5] This means the two theories are incommensurable and we can speak of revolution. Kuhn also notes that "the successful new theory must somewhere permit predictions that are different from those derived from its predecessor. That difference could not occur if the two were logically compatible. In the process of being assimilated, the second must displace the first." (*SSR,* 97) As mentioned above, what Kuhn means by logical incompatibility here is again that one theory cannot be derived from the other because in that case, the predictions made by the one would also have to be predictions made by the other.

The second argument is again philosophical, not empirical. He says that logical positivists, in their effort to ensure that theories are not replaced but built upon, "would restrict the range and meaning of an accepted theory so that it could not possibly conflict with any later theory that made predictions about some of the same natural phenomena" (*SSR,* 98). But Kuhn continues:

> … [T]o save theories in this way, their range of application must be restricted to those phenomena and to that precision of observation with which the experimental evidence in hand already deals. Carried just a step further (and the step can scarcely be avoided once the first is taken), such a limitation prohibits the scientist from claiming to speak "scientifically" about any phenomenon not already observed. Even in its present form the restriction forbids the scientist to rely upon a theory in his own research whenever that research enters an area or seeks a degree of precision for which past practice with the theory offers no precedent. These prohibitions are logically unexceptionable. But the result of accepting them would be the end of the research through which science may develop further (*SSR,* 100).

Kuhn says that this point "is virtually a tautology". Without commitment to a paradigm there can be no normal science. Normal science is supposed to produce puzzles that have not been solved before. If the paradigm is not extended into new areas and new degrees of precision but is confined to already known applications, there will be no surprises and anomalies which when assimilated and normalized in a new framework make possible fundamental change. Hanson (1965), very early on, had also noted that Kuhn's schema may be understood as an elaborate set of definitions and asked him to disambiguate his position: whether he advanced a historical hypothesis or an unfalsifiable set of meanings. Hanson meant it as criticism but I have tried to develop a different interpretation in Kindi (2005) in which I take seriously Kuhn's contention that his model is/can be derived from first principles but also find a place for his historical examples.

It seems clear from the above presentation of Kuhn's arguments that Kuhn considers the possibility that a new theory supplements the old one instead of replacing it, and

[5] 'Anomaly' literally means lack of normalcy. It derives from the Greek term 'omalos' which means even, normal, regular and the privative prefix 'a' (and not from 'nomos'-law-, as Ian Hacking writes in his Introduction in the 50th anniversary edition of *Structure*).

dismisses it on *a priori* grounds. So, Mizrahi's objection that Lavoisier's theory can be thought to supplement, rather than to replace Stahl's is already answered by Kuhn.[6] One may of course not find Kuhn's arguments convincing. But they exist. And since the issue under consideration is whether *Kuhn* had any arguments to deductively support his incommensurability thesis, I think Kuhn's own arguments should be considered and assessed.

No Inductive Support

Given that Kuhn offered *a priori* reasons and not historical as grounds for his discontinuity and, therefore, incommensurability thesis, I think the charge that no inductive support can be found is immaterial. He was not interested in offering inductive support. One can still, of course, raise the question whether empirical evidence can be found for this view independently of what Kuhn did. But then the argument would be different. Here we are considering whether *Kuhn* offered inductive support. He did not because he was not interested in that. And he was not interested in using historical evidence to provide inductive support because he thought that historical evidence can be manipulated to confirm any theory. "If you have a theory you want to confirm, you *can* go and do history so it confirms it, and so forth; it's just not the thing to do" (Kuhn 2000b, 314, emphasis in the original).

Kuhn repeatedly said that he did not want to amalgamate history and philosophy. In fact, whenever he was asked whether a particular development was 'normal or revolutionary, he used to answer "I do not know". He thought that this is a matter that requires historical research (Kuhn 1970, 251).

Lydia Patton's suggestion that Kuhn offered an abductive argument in defense of his incommensurability thesis is also problematic. First, because I do not think Kuhn was engaged in forming hypotheses to explain historical data. There is no such evidence and nowhere do we find Kuhn testing his presumed explanatory hypotheses. On the contrary, he repeatedly said that he did not even use his philosophical concepts to do history. Abductive arguments are indeed widely used in science. Yet, Kuhn was not doing science but philosophy.

Second, if we were to understand Kuhn's move as an Inference to the Best Explanation (IBE), incommensurability would be neither the explanans nor the explanandum. As Mizrahi says (2015b), taxonomic incommensurability cannot explain paradigm shifts because it is the mark of paradigm shifts. Paradigm shifts and incommensurability go together. In that sense incommensurability cannot be the explanandum either, since it is an inseparable part of Kuhn's model.

[6] I am not considering here a different kind of response, namely, that there is historical research (e.g., Chang 2012) which disputes Mizrahi's supposition regarding Lavoisier's and Stahl's theories and shows that the two theories are indeed incommensurable and the transition from phlogiston to oxygen was indeed a revolution. I am not using this argument for two reasons: first, because historical evidence can be differently interpreted and, second, because Mizrahi's objection to Kuhn's incommensurability thesis in this part of his paper is *a priori* (he says that Lavoisier's theory could supplement Stalh's) and not empirical.

Lastly, if Kuhn uses a definition of paradigm shift as a criterion to identify examples, as Patton maintains (2015, 55), then Kuhn is not giving an abductive argument but rather an illustration of his definition-thesis. Patton commenting on Mizrahi's reply sounds rather inconclusive. She says that Kuhn could be giving different kinds of argument (abductive, transcendental, regressive,…) or propose historical narratives. But the issue under consideration, which is tricky and crucial as she admits, is exactly that: what exactly is he doing? [7]

Mizrahi presents a particular episode from the history of science as a *rebutting defeater* of the view that there is discontinuity and replacement of theories in scientific development. The *anastomoses* episode is supposed to show conceptual continuity and supplementation rather than replacement. As Mizrahi acknowledges, this strategy is not new. Ever since *Structure* was published, the effort was to show that certain episodes in the history of science, which were typically used to illustrate Kuhnian scientific revolutions, were not really examples of radical discontinuity.

More recently the trend, coming mostly from historians of science who value locality and detailed description, is to go even further and derevolutionize more or less the history of science. I agree with Patton who says that these historical studies do not affect Kuhn's model. Not because, *pace* Patton, Kuhn did not want to demonstrate that all theory changes are paradigm shifts, but rather because his model is not based on historical evidence. As I explain (2005), historical research which concentrates on the details of particulars gives him the sensitivity for difference but does not offer inductive support. Besides, as Marcum's reading of the same historical episode illustrates, historical evidence can be interpreted differently.

Mizrahi (2015a, 10-11) cites Friedman as also supporting the view that there is continuity in scientific development. I don't think this is so. Friedman agrees with Kuhn that there are radical conceptual breaks in scientific development and seeks a way to bridge them. His statement, cited by Mizrahi (2015a, 11), that "earlier constitutive frameworks are exhibited as limiting cases, holding approximately in certain precisely defined special conditions, of later ones", is supposed to refer to what is going on retrospectively and from the perspective of the later paradigm. According to Friedman (2001, 98), what is recovered retrospectively in the case of Newton's theory from the perspective of Einstein's "is not the classical constitutive framework as such, but only an empirical counterpart to this classical framework formulated within an entirely different constitutive framework.". This is exactly the position that Kuhn defends in *Structure* (pp. 103-104) where he says that "[t]hough an out-of-date theory can always be viewed as a special case of its up-to-date successor, it must be transformed for the purpose. And the transformation is one that can be undertaken only with the advantages of hindsight, the explicit guidance of the more recent theory".[8]

[7] Lydia Patton (2015) also claims that the metaphor from ancient Greek mathematics to elucidate incommensurability in science is terrible. It's not clear to me why this is so since Kuhn makes use of geometric incommensurability found in ancient Greek mathematics where the comparison holds (as Patton also admits) and not of incommensurability as it is dealt with in analytic geometry where it does not hold.
[8] For more extensive discussion of Friedman's position see Kindi (2011).

Conclusion

In my view, Kuhn's model is not supposed to depict the actual pattern of scientific development. He was making a logical point; namely, that incommensurability results from the necessity to "normalize" an anomaly in a new network. What was abnormal (an anomaly) before becomes normal once the framework changes. The two frameworks are then incommensurable.

Kuhn's model is the lens through which we can look at the history and the practice of science to highlight discontinuity and diversity in order to combat that image of science which stressed accumulation and uniform method. Detecting continuities or discontinuities between paradigms or taxonomies should not be taken as evidence against or in favor of Kuhn's model.

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