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Is There Anything Wrong with Thomas Kuhn?

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Twenty-two years after his death, Thomas Kuhn's work is still able to provoke lively debates, where arguments are exchanged and competing interpretations of his theories are advanced. *The Kuhnian Image of Science* is a good example, as the book brings together ten scholars in a debate for and against Thomas Kuhn's legacy. The question, the edited volume raises, is straightforward:

> "Does the Kuhnian image of science provide an adequate model of scientific change? If we abandon the Kuhnian picture of revolutionary change and incommensurability [...], what consequences would follow from that vis-à-vis our understanding of science as a social, epistemic endeavor?" (7)

In this review I will concentrate on the first two parts of the book, i.e. and in particular on the debate between those who are questioning (Mizrahi, Argamakova, Park, Sankey), and those who are defending Kuhn (Kindi, Patton), since their arguments are closely related. Therefore, I will discuss some of their major arguments in topological order.

Debating Kuhn's Evidence

The editor Moti Mizrahi opens the debate in his introduction with a confrontational thesis: Kuhn, in his opinion, is responsible for an "infectious disease" (3), for "the pathological state of the field of philosophy of science in general, and general philosophy of science in particular" (3). Kuhn's vice is his use of case studies (from the history of science) as arguments, although - according to Moti Mizrahi - they are nothing more than "anecdotal evidence" leading to "hasty generalizations" and "fallacious inductive reasoning" (6).

Hearing the trumpets of the troops ready to battle one is eager to learn how to do it right: How the standards of inductive reasoning within philosophy of science are re-erected. Yet, anticipating one of the results of this review, the "inductive reasoning" intended to refute Kuhn's incommensurability thesis (found in the first part of the book) is actually its weakest part.

However, to understand the intricacy of this difficult task, we have to recognize, that it is not easy to support or falsify inductively a complex theory of science. Broadly speaking, in Kuhn's account we should empirically observe sciences displaying at least four different manifestations: (1.) "proto-science" in the pre-paradigm phase, when there is no general consensus about theories, methods and standards, (2.) "normal science", when scientists are most of the time focused on preserving, but also adapting existing paradigms to new problems and new scientific fields, (3.) sciences in a state of crisis, when more and more "anomalies" occur, which defy explanations in conformity with established procedures, and finally (4.) on rare occasions a "revolutionary" state, when different paradigms compete with each other and scientific theories based on one paradigm are to some extent "incommensurable" with those based on another paradigm.

There are good reasons to suppose that Kuhn's somehow schematic and ideal-typical description of scientific change is too simple compared with the complexities shown by

many historical case studies. Nevertheless, the counter-arguments under consideration brought forward against his model seem, paradoxically, to underestimate the complexity of Kuhn's claims. For example, in *Kuhn's Incommensurability Thesis* Mizrahi decides to discuss scientific change only in general. He claims that Kuhn argues:

> "Scientific change (specifically, revolutionary change) is characterized by taxonomic incommensurability." (33)

The compounded phrase "[s]cientific change (specifically, revolutionary change)" indicates that, in Mizrahi's interpretation, for Kuhn not all scientific change is per definition revolutionary. But then arguments against Kuhn's theory should consider at least two kinds of scientific change separately: revolutionary change and those (commensurable) non-revolutionary scientific changes within "normal science."

Keeping in mind that for Kuhn theory change is possible to a certain degree within normal science (only changing paradigms must be averted)[1], it is not clear, why Kuhn's "image of science" should be dismissed because "as far as theory change is concerned" taxonomic incommensurability "is the exception rather than the rule" (38).[2]

Or another example, in *Can Kuhn's Taxonomic Incommensurability Be an Image of Science?* where Seungbae Park comes to the conclusion that historical evidence shows that "scientific revolution is rare, taxonomic incommensurability is rare, and taxonomic commensurability is common" (61). It is, for similar reasons, unclear why this conclusion should not be commensurable with Kuhn's description of normal science, since Kuhn claimed that normal science is common and scientific revolutions are rare.

However, this is not Park's last argument about scientific change: He asks furthermore if we should not distinguish between the distant scientific past, when scientific revolutions were more common, and the recent past, "since most recent past theories have been stable, most present theories will also be stable" (70). Kuhn's theory of revolutionary paradigm change is, in his opinion, first of all not appropriate for understanding the development of contemporary and future science.

Incommensurable Paradigms of Language?

After a discussion of the critical reception of Thomas Kuhn's and Paul Feyerabend's work and the objections raised against their claim that scientific theories or paradigms are incommensurable, Howard Sankey admits in *The Demise of the Incommensurability Thesis* that:

> "the idea that there is conceptual change in science now seems commonplace. But the much-feared consequences, such as incomparability, communication breakdown, and irrationality now all seem to have been greatly overblown." (88)

[1] Kuhn discusses this type of theory change, for example, as divergent „articulation(s) of the paradigm" (Kuhn 1996, 83; cf. Kuhn 1996, 23, 29-34, 122).

[2] Always on condition that, like Moti Mizrahi in this argument, we accept the concept of „incommensurability" as defined by referential semantics. On some problems with „referential continuity" as main argument against incommensurability see further below.

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Prima facie it seems like a self-critical admission of an inappropriate former reception of Kuhn's theory of incommensurability, especially by those philosophers of science who tried to fight "irrationality" with the means of referential semantics. However, Sankey seems to think that the dissolution of the exaggerated accusations of Kuhn's critics somehow makes now Kuhn's theory of incommensurability obsolete. Hence, Sankey can summarize:

> "with the demise of the incommensurability thesis, the debate about scientific realism is free to proceed in a manner that is unencumbered by the semantic concerns about wholesale referential discontinuity that were prompted by the incommensurability thesis." (88)

For Sankey, Kuhn's concept of incommensurability is dead (87). He seems to blame Kuhn for the misguided interpretations of his opponents. It comes down to the argument: if it's not possible to criticize Kuhn's concept of incommensurability as "irrational" anymore, then Kuhn's concept cannot claim any relevance for future discussions.

However, more importantly: These arguments against Kuhn are based on referential semantics, i.e. semantic concerns about referential continuity. Hence, what their objections against Kuhn's incommensurability theory inadvertently show is, paradoxically, the incommensurability of competing paradigms of language. This becomes apparent, for example, when Mizrahi criticizes Kuhn's sometimes-vague formulations, especially in his early *Structure*. Mizrahi refers to statements where Kuhn argues with caution:

> "The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often [sic] actually incommensurable with that which has gone before." (Kuhn 1996, 103)

Formulations such as this prompt Mizrahi to ask: If taxonomic incommensurability (TI):

> "is not a general thesis about the nature of scientific change, then what is its *explanatory* value? How does (TI) help us in terms of understanding the nature of scientific change? On most accounts of explanation, an *explanans* must have some degree of generality [...] But if (TI) has no degree of generality, then it is difficult to see what the explanatory value of (TI) is." (37)

Kuhn could have responded that his arguments in *Structure* are explicitly based on Wittgenstein's theory of "language games" with its central concept of "family resemblance", which by definition does not allow the assumption that there are unambiguous conceptual

boundaries and a distinguishing characteristic, which all or even most of the phenomena aligned by a concept have in common.[3]

Indeed, understanding Wittgenstein's concept of "family resemblance" is central to understand Kuhn's theory of "paradigms", "paradigm shifts", and the meaning of "incommensurability".[4] Yet, it is possible to come to similar conclusions without referring to the late Wittgenstein: For example, Alexandra Argamakova despite of her negative evaluation of many of Kuhn's arguments, unlike Mizrahi, is closer on this issue to Kuhn where she claims in *Modeling Scientific Development*: "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" (54).

Defending Kuhn's Epistemology

In two of the book's most interesting discussions of Kuhn's epistemology, Vasso Kandi's *The Kuhnian Straw Man* and Lydia Patton's *Kuhn, Pedagogy, and Practice*, the allegation that Kuhn developed his theory on the basis of selected historical cases is refuted. Furthermore, Kindi, defending the innovative character of Kuhn's work asks "for a more faithful reading":

> "Kuhn's new image of science, which is actually a mosaic of different traditions, was not put together by generalizing from instances; it emerged once attention was drawn to what makes scientific practice possible, namely paradigms and what follows from them (normal science, anomalies, revolutions). In accordance with Kuhn's own understanding of scientific revolutions, his revolution in the perception of science did not have to summon new facts or make new discoveries; it only needed a new perspective." (104)

While Lydia Patton forcefully argues that:

> "Kuhn's original work did not restrict 'paradigm' to 'theoretical framework', nor did he restrict the perspective of scientific practice to the content of propositions with a truth-value. And it is mainly because Kuhn's arguments in *Structure* are outside the semantic view, and focus instead on the practice of science, that they are interesting and fresh." (124)

Both, Patton and Kindi, offer a close reading of Kuhn's work, trying to give new perspectives on some of the more contested concepts in Kuhn's epistemology.

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[3] "Instead of pointing out something common to all [...], I'm saying that these phenomena have no one thing in common in virtue of which we use the same word for all - but there are many different kinds of affinity between them" (Wittgenstein 2009, § 65) "I can think of no better expression to characterize these similarities than "family resemblances"; for the various resemblances between members of a family - build, features, colour of eyes, gait, temperament, and so on and so forth - overlap and criss-cross in the same way." (§ 67)

[4] Cf. Kuhn 1996, Ch. 5. Later, Kuhn argued explicitly against referential semantics but then on the basis of a hermeneutic (holistic) theory of language (Kuhn 2000; but cf. Kuhn 1996, 128f.).

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The Social in Social Epistemology

One explicit aim of this edited volume is, as the editor asserts, to outline what consequences would follow from this debate for "our understanding of science as a social, epistemic endeavor" (7). But for this reviewer it is not obvious how the strong emphasis on discounting Kuhn's incommensurability thesis in the first part of the book should lead to a better understanding of science as a social practice.

Kuhn's theory of incommensurability of competing paradigms is precisely the point within his epistemology where value judgments and social decisions come into play. While traditionally those who defended the "progress of science" (cf. Sankey: 87) against what they saw as Kuhn's "anti-realist" position were often those who wanted to defend the objectivity of science by excluding "external" influences, like the "social" and the political, from the scientific core.[5]

It is therefore important when talking about incommensurability of paradigms, and the possibility of a "communication breakdown", to distinguish between two distinct meanings: (a) the impossibility to communicate at all because people do not understand each other's language or paradigms and (b) the decision after a long and futile debate to end any further communication as a waste of time since no agreement can be reached. It is this second meaning, describing a *social* phenomenon, which is very common in science. Sankey argues against the first meaning when he declares:

> "Given that scientists are able to understand what is said by theories whose terms are untranslatable into their own, no insuperable obstacle stands in the way of full communication between the 'proponents of competing paradigms.'" (87)

While Sankey "wonders what all the fuss was about" (87), he has only shown (in accordance with Kuhn: cf. Kuhn 2000) that in theory full communication may be possible, but not that communication breakdowns are not common between scientists working with different paradigms. While on a theoretical level these workday problems to communicate may seem, for some philosophers of science, trivial. However, on the social level for working scientists, such communication breakdowns are often not only the reason for fraught relations between colleagues, but also for disciplinary segmentation and sometimes for re-drawing boundaries of scientific disciplines.

Perhaps it is no coincidence that in this volume those who discuss social as well as epistemological practices of scientists are not those who criticize incommensurability from a semantic point of view. Social and epistemological practices are considered in one way or the other by those defending Kuhn, like Kindi and Patton, and those whose main concern is to revise certain aspects of Kuhn's image of science, like James A. Marcum, Barbara Gabriella Renzi & Giulio Napolitano, and David P. Rowbottom.

[5] This, despite the fact that Kuhn himself tried to restrict the relevant „social" factors in his epistemology to social dynamics within scientific communities.

However, as I confined this review to the discussion of the first six articles I can only point out that the four remaining articles go beyond the topics discussed thus far and would deserve not only attentive readers but also a thorough discussion. They analyze, for example, scientific revolutions in mathematics (Andrew Aberdein), the role of evolutionary metaphors (Gabriella Renzi/Napolitano, Marcum) and of methodological contextualism in the philosophy of science (Rowbottom). Hence, although this edited volume has some weaknesses, there are several contributions, which open new avenues of thought about Kuhn, and are worth reading for those interested in Kuhn and in philosophy of science.

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