



SERRC
Social Epistemology
Review & Reply Collective

<http://social-epistemology.com>
ISSN: 2471-9560

Each Kuhn Mutually Incommensurable

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Bryant, Amanda. "Each Kuhn Mutually Incommensurable." *Social Epistemology Review and Reply Collective* 7, no. 6 (2018): 1-7.

Short url: <https://wp.me/p1Bfg0-3XM> (provided by WordPress)

This volume is divided into four parts, in which its contributors variously Question, Defend, Revise, or Abandon the Kuhnian image of science. One immediately wonders: what is this thing, *the Kuhnian Image of Science*? It isn't a question that can be decisively or quickly settled, of course. Perhaps one of the reasons why so much has been written on Kuhn's philosophy of science is that it gives rise to such rich interpretive challenges.

Informed general philosophy of science readers will of course know the tagline version of Kuhn's view — namely, that the development of science unfolds in wholesale revolutions of scientific paradigms that are in some sense incommensurable with one another. However, one might think that whatever the image of science at issue in this volume is, it should be a sharper image than that.

Many Thomases Kuhn

But of course there isn't really a single, substantive, cohesive, uncontroversial image at issue. Alexandra Argamakova rightly points out in her contribution, “there exist various images of science belonging to different Thomas Kuhns at different stages of his work life and from different perspectives of interpretation, so the target for current analysis turns out to be less detectable” (46). Rather, the contributors touch on various aspects of Kuhn's philosophy, variously interpreted — and as such, multiple Kuhnian images emerge as the volume unfolds. That's just as it should be. In fact, if the volume had propped up some caricature of Kuhn's views as *the* Kuhnian image of science, it would have done a disservice both to Kuhn and to his many interpreters.

One wonders, too, 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’. In his introduction, Moti Mizrahi emphasizes Kuhn's undeniable influence. Kuhn has, Mizrahi points out, literally tens of thousands of citations; numerous books, articles, and journal issues devoted to his work; and a lasting legacy in the language of academic and public discourse. While all of this signals influence, it's clearly no indication of agreement.

To be fair, Mizrahi acknowledges the “fair share” of Kuhn critics (2). Nevertheless, if the prospect of decisively transforming the Kuhnian image of science were to be a serious prospect, then the image would have to be widely accepted and enjoy a lasting relevance. However, Argamakova again rightly emphasizes that Kuhn's philosophy of science “never fully captured the intellectual market” (45) and “could not be less attractive for so many minds!” (47). Moreover, in a remarkable passage in his contribution, Howard Sankey describes a central component of the so-called Kuhnian image of science as an old battlefield and a dead issue:

Returning to the topic from the perspective of the contemporary scene in the philosophy of science is like visiting a battlefield from a forgotten war. The positions of the warring sides may still be made out. But the battlefield is grown over with

grass. One may find evidence of the fighting that once took place, perhaps bullet marks or shell holes. But the fighting ceased long ago. The battle is a thing of the past.

The problem of incommensurability is no longer a live issue. The present chapter has taken the form of a *post-mortem* examination of a once hotly debated but now largely forgotten problem from an earlier period in the philosophy of science. (87)

If the same holds true for the rest of the Kuhnian image (or images), then the volume isn't exactly timely.

But dead philosophical issues don't always stay dead. Or rather, we're not always right to pronounce them dead. In 1984, Arthur Fine famously proclaimed scientific realism "well and truly dead" (in *The Natural Ontological Attitude*), and clearly he was quite wrong. At any rate, we may find interest in an issue, dead or not, and there is certainly much of it to be found in this volume. I have been asked to focus my comments on the second half of the book. As such, I will discuss the Introduction, as well as Parts I and II in brief, then I will discuss parts III and IV at greater length.

On the Incommensurable

In his Introduction, Mizrahi argues that, far from initiating a historical turn in the philosophy of science, Kuhn was 'patient zero' for *anecdotiasis* — "the tendency to use cherry-picked anecdotes or case studies... to support general claims (about the nature of science as a whole)" (3). Mizrahi argues that anecdotiasis is pervasive, since significant proportions of articles in the PhilSci-Archive and in leading philosophy of science journals contain the phrase 'case study'.

But neither using the phrase 'case study' nor doing case studies is inherently or self-evidently problematic. Case studies can be interesting, informative, and evidential. Of course the challenges are not to ignore relevant problem cases, not to generalize hastily, and not to assign undue evidential weight to them. But if we are to suppose that all or most philosophers of science who use case studies fail to meet those challenges, we will need a substantial body of evidence.

Part I begins with Mizrahi's contribution, which the successive contributions all engage. In it, he defines taxonomic incommensurability as conceptual incompatibility between new and old theories. Against those who claim that Kuhn 'discovered' incommensurability, Mizrahi argues that there are no good deductive or inductive arguments for taxonomic incommensurability. He cites just two authors, Eric Oberheim and Paul Hoyningen-Huene, who use the language of discovery to characterize incommensurability. As such, it isn't clear that the assumption Mizrahi takes pains to reject is particularly widespread.

Nevertheless, even if everyone universally agreed that there are no legitimate cases of incommensurability, it would still be useful to know why they'd be justified in so thinking. So the work that Mizrahi does to establish his conclusion is valuable. He shows the dubious

sorts of assumptions that arguments for the taxonomic incommensurability thesis would hang on.

Argamakova's helpful and clear contribution lays out three general types of critique with respect to Kuhn's view of scientific development — ambiguity, inaccuracy, and limitation — and raises, if tentatively, concerns about Kuhn's universalist ambitions. She might have been more explicit with respect to the force and scope of her comments on universalism — in particular, whether she sees the flaws in Kuhn's theory as ultimately stemming from his attempts at universal generalizations, and to what extent her concerns extend beyond Kuhn to general philosophy of science.

Seungbae Park advances several arguments in response to Kuhn's incommensurability thesis. One such argument takes up Kuhn's analogy in *The Structure of Scientific Revolutions* (henceforth *Structure*) between the development of science and the evolution of organisms. Park suggests that in drawing the analogy, Kuhn illicitly assumes the truth of evolutionary theory. He doesn't consider that Kuhn could adopt the language of a paradigm (for the purposes of drawing an analogy, no less!) without committing to the literal truth of that paradigm.

Park also claims that “it is self-defeating for Kuhn to invoke a scientific theory to give an account of science that discredits scientific claims” (66), when it's not clear that the analogy is at all integral to Kuhn's account. Kuhn could, for instance, have ascribed the same characteristics to theory change without referring to evolutionary theory at all.

Sankey's illuminating contribution fills in the interpretive background on incommensurability — the semantic version of Kuhn's incommensurability thesis, in particular. He objects, with Mizrahi, to the language of discovery used by Oberheim and Hoyningen-Huene with respect to incommensurability. He argues, convincingly, that the purported paradigm shift that allowed Kuhn to finally comprehend Aristotle's physics isn't a case of incommensurability, but rather of comprehension after an initial failure to understand. While this doesn't establish his conclusion that *no* cases of incommensurability have been established (76), it does show that a historically significant purported case is not genuine.

Vasso Kindi fills in some historical detail regarding the positivist image of science that Kuhn sought to replace and the “stereotypical” image attributed to him (96). She argues that Kuhn's critics (including by implication several of her co-contributors) frequently attack a strawman — that, notwithstanding Kuhn's avowed deference to history, the Kuhnian image of science is not meant to be a historical representation, and so doesn't need to be supported by historical evidence. It is, rather, a “a philosophical model that was used to challenge an ideal image of science” (95).

Finally, Lydia Patton emphasizes the practical dimension of Kuhn's conception of paradigms in *Structure*. It ought to be uncontroversial that on Kuhn's early characterization a paradigm is not merely a theory, but a series of epistemic, evaluative, and methodological practices, too. But Patton argues that there has been too strong a semantic tendency in the treatment of Kuhnian paradigms (including by the later Kuhn himself). She argues for the greater

interest and value of a practical lens on Kuhn's project for the purposes of understanding and explaining science.

Vectors of Glory

Andrew Aberdein's contribution deals with the longstanding and intriguing question of whether there are revolutions in mathematics. He imports to that discussion distinctions he drew in previous work among so-called *glorious*, *inglorious*, and *paraglorious* revolutions, in which, respectively, key components of the theory are preserved, lost, or preserved with new additions. *Key* components are, he says, "at least all components without which the theory could not be articulated" (136).

He discusses several examples of key shifts in mathematical theory and practice that putatively exemplify certain of these classes of revolution. The strength of the paper is its fascinating examples, particularly the example of Inter-Universal Teichmüller theory, which, Aberdein explains, introduces such novel techniques and concepts that some leading mathematicians say its proofs read as if they were "from the future, or from outer space" (145).

Aberdein doesn't falsely advertise his thesis. He acknowledges that "it is not easy to determine whether a given episode is revolutionary" (140), and claims only that certain shifts "may be understood" as revolutionary (149) — that the cases he offers are *putative* mathematical revolutions. As to how we should go about identifying putative mathematical revolutions, Aberdein suggests we look directly for conceptual shifts (or 'sorites-like' sequences of shifts) in which key components have been lost or gained.

A fuller discussion of these diagnostics is needed, since the judgment of whether there are revolutions (genuine or putative) in mathematics will hang largely on diagnostics such as these. Is any key conceptual shift sufficient? If so, have we really captured the spirit of Kuhn's view, given that Kuhn seems to ascribe a certain momentousness to revolutions? If the conceptual shift has to be substantial, how substantial, and how should we gauge its substantiality? Without some principled, non-arbitrary, and non-question-begging standards for what counts as a revolution, we cannot hope to give a serious answer to the question of whether there are, even putatively, revolutions in mathematics.

The paper would also have benefited from a more explicit discussion of what a mathematical paradigm is in the first place, especially as compared to a scientific one. We can infer from Aberdein's examples that conceptions of number, ratio, proportion, as well as systems of conjecture and mathematical techniques belong to mathematical paradigms — but explicit comment on this would have been beneficial.

Moreover, Aberdein sees an affinity between mathematics and science, commenting toward the end of the paper that the methodology of mathematics is not so different from that of science, and that "the story we tell about revolutions [should] hold for both science and mathematics" (149). These are loaded comments needing further elaboration.

The Evolution of Thomas Kuhn

In his contribution, James Marcum argues that Kuhn's later evolutionary view is more relevant to current philosophy of science (being 'pluralistic and perspectival') than his earlier revolutionary one. On Kuhn's later evolutionary view, Marcum explains, scientific change proceeds via "smaller evolutionary specialization or speciation" (155), with a "gradual emergence of a specialty's practice and knowledge" (159). On this view, scientific development consists in "small incremental changes of belief" rather than "the upheaval of world-shattering revolutions" (159).

Marcum uses the emergence of bacteriology, virology, and retrovirology to illustrate the strengths and weaknesses of Kuhn's evolutionary view. Its main strength, he says, is that it illuminates the development of and relationships among these sorts of scientific specialties; its weakness is that it ascribes a single tempo — Darwinian gradualism — and a single mode — speciation — to the evolution of science. Marcum adopts George Gaylord Simpson's "richer and more textured approach" (165), which distinguishes several tempos and modes. Since these refinements better enable Kuhn's view to handle a range of cases, they are certainly valuable.

According to Marcum, current philosophy of science is 'pluralistic and perspectival' in its recognition that different sciences face different philosophical issues and in its inclusion of perspectives from outside the logico-analytic tradition, such as continental, pragmatist, and feminist perspectives (166). Marcum seems right to characterize current philosophy of science as pluralistic, given the move away from general philosophy of science to more specialized branches.

If this pluralism is to be embraced, one might wonder what role (if any) remains for general philosophy of science. Marcum makes the interesting suggestion that a general image of science, like Kuhn's evolutionary image, while respecting our contemporary pluralistic stance, can at the same time offer "a type of unity among the sciences, not in terms of reducing them to one science, but rather with respect to mapping the conceptual relationships among them" (169).

One of Marcum's central aims is to show that incommensurability plays a key explanatory role in a refined version of Kuhn's evolutionary image of science. The role of incommensurability on this view is to account for scientific speciation. However, Marcum shows only that we can characterize scientific speciation in terms of incommensurability, without clearly establishing the explanatory payoff of so doing. He does not succeed in showing that incommensurability has a particularly enriching explanatory role, much less that incommensurability is "critical for conceptual evolution within the sciences" or "an essential component of... the growth of science" (168).

All a Metaphor?

Barbara Gabriella Renzi and Giulio Napolitano frame their contribution with a discussion of competing accounts of the nature and role of metaphor. They avow the commonly accepted view that metaphors are not merely linguistic, but cognitive, and that they are ubiquitous. They claim, I would think uncontroversially, that metaphors shape how individuals approach and reason about complex issues. They also discuss historical empiricist attitudes toward metaphor, competing views on the role of models and metaphor in science, and later, the potential role of metaphor in social domination.

Renzi and Napolitano also address Kuhn's use of the metaphor of Darwinian evolution to characterize scientific change. They suggest that an apter metaphor for scientific change can be made of the obsolete *orthogenetic* hypothesis, according to which "variations are not random but directed by forces regulated and ultimately directed by the internal constitution of the organism, which responds to environmental stimuli" (184).

The orthogenetic metaphor is a better fit for scientific change, they argue, because the emergence of new ideas in science is not random, but driven by "arguments and debates... specific needs of a scientist or group of scientists who have been seeking a solution to a problem" (184).

The orthogenetic metaphor effectively highlights a drawback of the Darwinian metaphor that might otherwise be overlooked, and deserves further attention. The space devoted to discussing metaphor in the abstract contributes little to the paper, beyond prescriptions to take metaphor seriously and approach it with caution. Much of that space would have been better devoted to using historical examples to compare Kuhn's Darwinian metaphor to the proposed orthogenetic alternative, to make concrete the fruitfulness of the latter, and to flesh out the specific kinds of internal and external pressures that Renzi and Napolitano see as important drivers of scientific change.

Methodological Contextualism

Darrell Rowbottom offers a summary and several criticisms of what he sees as Kuhn's early-middle period image of science. By way of criticism, he points out that it isn't clear how to individuate disciplinary matrices in a way that preserves a clear distinction between normal and extraordinary science, or ensures that what Kuhn calls 'normal science' is really the norm. Moreover, in linking the descriptive and normative components of his view, Kuhn implausibly assumes that mature science is optimal.

Rowbottom suggests a replacement image of science he calls *methodological contextualism* (developed more fully in previous work). Methodological contextualism identifies several roles — puzzle-solving, critical, and imaginative — which scientific practitioners fulfill to varying degrees and in varying combinations. The ideal balance of these roles depends on contextual factors, including the scientists available and the state of science (200).

The novel question Rowbottom considers in this paper is: how could piecemeal change in science be rational from the perspective of methodological contextualism? I have difficulty seeing why this is even a *prima facie* problem for Rowbottom's view, since puzzle-solving, critical and imaginative activities are clearly consonant with piecemeal change. I suppose it is because the view retains some of Kuhn's machinery, including his notion of a disciplinary matrix.

At any rate, Rowbottom suggests that scientists may work within a partial disciplinary matrix, or a set of partially overlapping ones. He also makes the intriguing claim that "scientists might allow inconsistency at the global level, and even welcome it as a better alternative than a consistent system with less puzzle-solving power" (202). One might object that Kuhn's incommensurability thesis seems to block the overlapping matrix move, but Rowbottom proclaims that the falsity of Kuhn's incommensurability thesis follows "as a *consequence* of the way that piecemeal change can occur" (201). One person's *modus ponens* is another's *modus tollens*, as they say.

A Digestible Kuhn

The brevity of the contributions makes them eminently digestible and good potential additions to course syllabi at a range of levels; on the other hand, it means that some of the most provocative and topical themes of the book — such as the epistemic and methodological status of generalizations about science and the role of general philosophy of science in contemporary philosophy — don't get the full development they deserve. The volume raises more questions than it satisfactorily addresses, but several of them bring renewed relevance and freshness to Kuhnian philosophy of science and ought to direct its future course.

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References

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