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## Discussion

## Closing the door to cloud-cuckoo land: a reply to Šešelja and Straßer

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## ABSTRACT

Šešelja and Straßer's critique fails to hit its target for two main reasons. First, the argument is not that Kuhn is a rationalist because he is a coherentist. Although Kuhn can be taken as a rationalist because of his commitment to epistemic values, coherence analysis provides a more comprehensive characterisation of cognitive process in scientific change than any of these values alone can offer. Further, we should understand Kuhn as characterising science as the best form of rationality we have outside logic, which rules out algorithmic rationality and allows non-cognitive factors to play a role in theory change. Second, Šešelja and Straßer overemphasise the importance of a priori reasoning in Kuhn, which was only an alternative to his earlier historical-empirical approach. My suggestion is that Kuhn's neo-Kantian historical cognitivism integrates the earlier empirical and the later a-prioristic orientations. According to it, that any understanding of the world is preconditioned by some kind of mental module that is liable to change, detected as a discontinuity in the historical record of science.

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## 1. Introduction

Šešelja & Straßer (2009) put forward many worthwhile suggestions of how to understand Kuhn's philosophy in their extremely interesting critical paper. However, their analysis is problematic for a number of reasons and their critique fails to hit its target. First, incorporating Kuhn into coherentist epistemology is not the ultimate or only reason to regard him as a rationalist, and it is a mistake to take me to argue for a 'strong notion of rationality'. The most fruitful interpretation of Kuhn is to take him as characterising rationality in science: that is, as outlining the best form of rationality we have outside logic. The point of coherence analysis is to offer a more comprehensive characterisation of cognitive process than any of Kuhn's cognitive values alone can offer, in a way that also sheds light on the nature of scientific change in general. Further, it would be misguided to crave for ideal algorithmic rationality in studies of science. Instead, we have to be content with a weaker form of rationality, according to which both cognitive and non-cognitive factors determine outcomes in science.

Second, although Šešelja and Straßer correctly point out that the later Kuhn wished to use a priori reasoning in order to reach the conclusions of his historical perspective, they overstate their

a-prioristic reading of Kuhn. A priori reasoning only represented an alternative to Kuhn's earlier empirical approach that relied on studying the history of science. Further, Kuhn's alleged rejection of the mind-independent world cannot be the rationale to reject the correspondence theory for the reason that an idealist can be a correspondence theorist. In actuality, it is doubtful that Kuhn was inclined to any kind of full-blooded idealism that discounts the existence of the mind-independent world. My suggestion is that latterly Kuhn's neo-Kantian cognitivism integrates the earlier empirical and the later a-prioristic approaches. He suggested that we stop referring to the world *an sich*, because it is beyond our reach. Kuhn argues that any understanding of the world is preconditioned by some kind of mental module that is liable to change, manifested as discontinuity in the historical record of science.

## 2. Rationality in science and coherence analysis

Šešelja and Straßer argue that the coherentist interpretation does not offer us any interesting notion of rationality, which would not be found in Kuhn's texts anyway. Specifically, coherentism does not guarantee or lead to a strong notion of rationality, according to which a unique theory choice is strictly determined

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via an application of theory evaluation criteria. If Kuhn's writings already offer us a weak notion of rationality without a strict determination of theory choice and Kuhn already explicated his view on theory choice, they ask, why refer to coherentism in the first place?

The problem is that I never said that 'Kuhn's philosophy does not necessarily lead to an abandonment of rational inter-paradigm theory comparison, *since* it can be incorporated into coherentist epistemology' (Šešelja & Straßer, 2009, Sect. 3; my emphasis). In order to get matters straight, I hope I may be forgiven for quoting the crucial piece of textual evidence. In reference to epistemic values, I wrote:

We can see that the talk of Kuhn as an outright irrationalist is a misrepresentation. Even if these standards are not 'point-by-point' in the way that the comparison of unambiguous truth-values requires, Kuhn clearly recognises some common theory comparison criteria. I wish to *go further* than this and show that his philosophy contains elements that make it fit well with a coherentist epistemology. (Kuukkanen, 2007, p. 559; my emphasis)

In other words, the point is not that Kuhn was a rationalist, *because* he was a coherentist. We are apparently in agreement that Kuhn can be seen as a rationalist with or without coherentism. But one should remember that until recently any rationalistic reading of Kuhn was very contentious in philosophy of science. The great rationality debates of the 1960s and 1970s persistently placed Kuhn beyond any form of rationality, that is, he was seen as an outright irrationalist. It is sufficient here to remind ourselves of Lakatos's words, 'in Kuhn's view scientific revolution is irrational, a matter for mob psychology' (Lakatos, 1970, p. 178). This is the general background of my argument, not any attempt to portray Kuhn as an advocate of an algorithmic kind of rationality, which he clearly wasn't (see Kuukkanen, 2007, p. 562). To say that Kuhn can be seen as a rationalist at all is news, as I suspect there is no consensus on this even today.

However, the authors are right that there is need for further explication of the notion of rationality. Their contribution is to emphasise that there is looseness in the application of Kuhn's epistemic criteria, and that his account of theory choice allows the employment of both cognitive criteria and non-cognitive factors. Let's talk about rationality then. I have a suggestion to make.

Šešelja and Straßer suggest that it might be desirable to assess science with the measuring rod of ideal rationality. As they state, any weak form of rationality implied by coherence analysis is not satisfactory for 'some philosophers of science' (2009, Sect. 1 and Sect. 3). Well, so much the worse for those philosophers. It is somewhat pointless to crave the strong notion of rationality in science, and I agree with the authors that this would represent a wrong track in terms of reading Kuhn's philosophy. The most fruitful view of Kuhn and rationality in science is not the one that despairs of the 'rationality gap' between an algorithmic sort of rationality and what Kuhn says about science, or that reads Kuhn as an irrationalist, but the one that takes Kuhn to characterise science as the best form of rationality we have outside logic. As Kuhn somewhat colourfully put it, 'to suppose, instead, that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land' (Kuhn, 2000, p. 159). There is by now an avalanche of works in history and sociology of science that show that the application of strong rationality or pure internal logic is nothing but hopeless. It just may be the case that we have to be content with some weaker form of rationality, according to which not only cognitive criteria but also social and historical factors have a role in determining outcomes in science.

So what's the point of coherence analysis, if it doesn't imply a stronger account of rationality that is within our reach in Kuhn anyway? The answer is implicit above, but let's spell it out. The point is to go 'further' (Kuukkanen, 2007, p. 559) than the standard rationalist reading of Kuhn via single epistemological values, or to interpret Kuhn as a rationalist 'even to the extent' (ibid., p. 562) that he might have accepted a more general epistemological method of evaluation. To say that theory choices are made on the basis of coherentist criteria tells us more about the dynamics of scientific change than, for example, a claim that consistency or accuracy is one of the main cognitive factors behind scientists' theory choices. In other words, coherentism is a more comprehensive characterisation of the cognitive process behind theory change than any of the five or six criteria alone can offer. As Šešelja and Straßer make clear, there is looseness in the preference order of different criteria and in the exact application of values. In fact, trade-offs between them are frequent. One may emphasise more simplicity and problem-solving potentiality on one occasion, and consistency and problem-solving capability on another. And there is no guarantee that everyone agrees about the degree of simplicity of a certain theory, for example. This is why science is human-made and, up to a certain point, scientists are free to choose and judge. But to suggest that coherentism guides scientific activity implies that all the values that Kuhn mentions are linked so that they function as part of a more general epistemological framework. If my analysis is correct, the value of the coherence analysis is that it tells us more about scientific change itself and therefore more about *rationality in science* as well.

Taking coherentism as an aim in science would mean that one of the most important driving forces in science is to achieve as coherent an account of nature as possible (even if the total coherence of all sciences may decrease as a result of the speciation of scientific fields). If one is interested in scientific change, this ought to be an informative assertion. To take one example, the phenomenon where new theories cannot explain all the problems of old theories has sometimes been called 'Kuhn-loss'.<sup>1</sup> Kuhn-loss raises the question why it would be reasonable to accept a new theory if it is accompanied by such an explanatory loss. This is something that happened in the Copernican revolution, where it took no fewer than 100 years for Copernicanism to become the dominant paradigm (Kuhn, 1957, p. 227). The coherence analysis might offer an answer, which is that the loss in the immediate problem-solving capability was compensated by an improvement with respect to other epistemic values, such as harmony and simplicity, promising a more coherent system as a whole. In actuality, this is one of the cases where Kuhn refers directly to coherence as an explanatory factor in scientific change, but of course there were other (extrascientific) influencing factors, such as the need for calendar reform as well (Kuhn, 1957, p. 171; see also pp. 11–12, 124–126).

### 3. Rejection of which theory of truth?

Let's now move on to the second challenge. This time Šešelja and Straßer get the interpretation right. I still believe that Kuhn's argument against the idea that science convergences on the truth was motivated by empirical observations, because he didn't find continuity in the history of science to justify that position. Kuhn's famous Aristotle experience, which made him realise that there is a conceptual and ontological break between Aristotelian and Newtonian sciences, was probably the most important single event that shaped Kuhn's thinking and one to which he referred often (Kuukkanen, 2008, pp. 51–55). The really interesting question is, had Kuhn not experienced that there is no 'historical evidence for

<sup>1</sup> For a list of references on 'Kuhn-loss', see Gattei (2008), p. 69 n. 203.

a process of zeroing in' (Kuhn, 2000, p. 206), would he have become an advocate of the truth-progressive image of science? My view is that he quite possibly would have done so. In this sense, Kuhn was part of the tradition that perhaps somewhat naively looked for empirical validation or falsification of metahistorical views, best continued by Larry Laudan (e.g. 1981) and most recently by Stathis Psillos (2005) on the other side of the debate.<sup>2</sup>

The authors note that one obstacle to overcome is Kuhn's objection to the correspondence theory. For how otherwise can one maintain that Kuhn would have been able to agree about the convergence of science to the truth, that is, a view of being true in the sense of correspondence, if he thought that the whole idea of correspondence was fundamentally flawed? The authors are not impressed by my suggestion, and I am not alone in making it (e.g. Bird, 2000, pp. 225–227; Hoyningen-Huene, 1993, pp. 263–264), that Kuhn might have misunderstood the correspondence theory and taken it as an epistemic theory. Šešelja and Straßer claim that Kuhn's argument against convergent realism was a priori and that Kuhn offered a principled rejection of the correspondence theory.

In their view, a necessary condition for being a correspondence theorist about truth is to assume that there is a mind-independent world. 'Kuhn's attack on the correspondence theory of truth is an attack on one of its main constitutive ideas—the notion of the mind-independent world' (Šešelja & Straßer, 2009, Sect. 4). Unfortunately, this suggestion is just bound to add more confusion about the correspondence theory of truth. It is true that the correspondence theory is often associated with realism, or more precisely with a realist theory of truth, which says that the state of affairs that makes a truth-bearer true has to be mind-independent (e.g. Kirkham, 1995, pp. 73–79). However, a commitment to a mind-independent world is not necessary for being a correspondence theorist. All the correspondence theorist has to hold is that our chosen truth-bearers correspond in some sense to a certain state of affairs, not that the state of affairs is mind-independent. In other words, an absolute idealist who holds that the whole world is God's creation can be a correspondence theorist. She may hold that truth consists in correspondence to mind-dependent facts (e.g. Kirkham, 1995, pp. 131–134). If Kuhn's reason for rejecting the correspondence theory of truth was thus the denial of a mind-independent world, his rejection cannot be said to be well grounded.

But the fact remains that Kuhn does talk about abandoning the idea of a mind-independent world. But in what sense? The authors provide a clue in one of their Kuhn quotations. Kuhn says that 'no sense can be made of the notion of reality as it has *ordinarily functioned in philosophy of science*' (Kuhn, 2000, p. 115; my emphasis). Kuhn is thus talking about the problems with the notion as it had been applied in philosophy of science, not rejecting it as such. It is difficult to take Kuhn as a full-blooded idealist, as there is plenty of evidence to the contrary. He was inclined to accept what might be called 'fig-leaf realism' (Devitt, 1997, p. 23), according to which something exists independently of the mental although we may not be able to specify what and in what form. Kuhn described himself as an 'unregenerate realist' (Kuhn, 2000, p. 203). He also implied that nature undermines the security of the professional community (ibid., p. 169), that nature has a role to play in shaping our conceptions (Kuhn, 1977, p. 72), that the world 'places rigid constraints' (Kuhn, 2000, p. 102), and that the role of nature cannot be left out of the explanations of science (ibid., p. 317).

The more one reads Kuhn the better one sees that epistemic concerns feature prominently in his thinking. I think Hoyningen-Huene got this right in pointing out that Kuhn thought it meaningless to talk of a theory-free, 'object-sided' world and therefore of a

match between theories and the object-sided world (Hoyningen-Huene, 1993, pp. 263–264). The issue turns on Kuhn's idea that any conception of the world is lexicon-dependent, i.e. it is mediated via some conceptual machinery that makes the experience possible. Let's call this the phenomenal world. However, it is worth pointing out once more that Kuhn never denied that there can be a mind- and lexicon-independent world. There 'must, of course, be something permanent, fixed, and stable ... like Kant's *Ding an sich*' (Kuhn, 2000, p. 104). The problem with that world is that it is beyond our reach.

Šešelja and Straßer correctly point out that latterly Kuhn suggested the concept of truth can be used only internally, inside a lexicon. Interestingly, in contrast to clear-cut rejections of the correspondence theory, it is also possible to find perhaps a reluctant commitment to it or at least to a minimalist version of it. Kuhn says 'What replaces it [the correspondence theory] will still require a strong conception of truth, but not, except in the most trivial sense, correspondence truth', which could be something like a redundancy theory (Kuhn, 2000, pp. 95, 98). The most that we can say is that Kuhn rejected the realist theory of truth when he thought he rejected the correspondence theory, because the apparent reason for the rejection is that the nature of objects depends on our conceptual schemes or epistemic capabilities (see Kirkham, 1995, p. 73).

But we also learn that the essential function of the concept of truth is to 'require choice between acceptance and rejection of a statement or a theory in the face of evidence' (Kuhn, 2000, p. 99), i.e. some kind of epistemic theory of truth (e.g. ibid., pp. 114–115, 95–96). Further, after criticising the correspondence theory, Kuhn usually refers to the comparative epistemic criteria that can be used in theory choices. Šešelja and Straßer reasonably conclude that the non-epistemic character of the correspondence theory is nonsensical for Kuhn (Šešelja & Straßer, 2009, Sect. 4). One wonders whether Kuhn's reason really forms a principled rejection of the correspondence theory of truth or perhaps, as I see it, its abandonment on epistemic-pragmatic grounds.

#### 4. Empirical and a priori approaches integrated

The authors get their a priori argument half-right. In the latter part of his career, Kuhn was fascinated by a priori argumentation. However, this should not be used to conceal the fact that Kuhn's historical inclination in his early career was strong. I take it that the authors don't think that Kuhn achieved his ideas about scientific revolutions and scientific change in *The structure of scientific revolutions* by a priori reasoning? Maybe it is sufficient here to take two sentences from the very first paragraph of *Structure*:

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed ... [The] aim [of this essay] is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself. (Kuhn, 1970, p. 1)

One is not short of similar textual material.

Kuhn's transition from empiricist argumentation to a priori reasoning can actually be seen in the quotations given by Šešelja and Straßer. Kuhn says:

my generation of philosophers/historians saw ourselves as building a philosophy on observations of actual scientific behavior ... Given what I shall call the historical perspective,

<sup>2</sup> It now seems to me unrealistic that any one metahistorical view of science could be proved to be uniquely correct. It is likely that any empirical argument for such a view is irreducibly philosophically laden.

one can reach many of the central conclusions we drew with scarcely a glance at the historical record itself.

And he says ‘many of the most central conclusions we drew from the historical record can be derived instead from first principles’ (Kuhn, 2000, pp. 111–112). In other words, Kuhn admits that he and other historical philosophers of science drew conclusions from historical record. He adds that the same conclusion *can* be derived from ‘first principles’. ‘Can’ does not, of course, imply that they must. Kuhn is envisioning an alternative way to reach the conclusions of his historical perspective, which by no means nullifies the earlier empirical attempt.

But, all in all, it is true that Kuhn appeared to undergo this change of mind. He felt that a priori reasoning would offer him a firmer standpoint and thus ‘reduce contingency’ of his arguments. This is well documented also in Kuhn’s publishing record, which consists solely of historical publications at the beginning, but includes only philosophical articles at the end. Kuhn appeared to move year by year from his historical-empirical approach to more and more purely philosophical argumentation.

I have long been of the opinion that Kuhn’s turn to a priori reasoning represented a wrong or at least very strange turning (see Bird, 2002; Andersen, 2001, p. 76) in his otherwise empirically minded approach. What makes this turning especially surprising is that at the same time he adopted a very cognitive scientific vocabulary of mental modules, lexicons, lexical structures, and neural mechanisms (for example Kuhn, 2000, pp. 229, 245) that have subsequently been shown to receive a fruitful interpretation by the machinery of cognitive science (for example Andersen, Barker, & Chen, 1996). However, there may actually be an entirely rational explanation for this double-sided image, which brings the empirical and a priori sides interestingly together.

It is likely that the later Kuhn found an explanation in the form of historical neo-Kantianism for the phenomena of historical discontinuity and changes of conceptual schemes that the young Kuhn detected in the history of science. Further, the new mentalistic notions that underpin his Kantianism were meant to explain how experience of the world is pre-conditioned. For example, he says that a ‘structured lexicon’ ‘resembles Kant’s a priori when the latter is taken in its ... relativized sense’ (Kuhn, 2000, p. 245). We would do well to consider a Kantian interpretation alternative to the full-blooded idealistic one. Kuhn meant not that there is literally no mind-independent world, but that any attempt to express the world has to be constructed via a conceptual scheme. Scientists cannot reach the world-in-itself but are limited to their phenomenal worlds. Any intelligible representation of the world is thus conceptual-scheme-related, which makes it meaningless to refer to the world as such. In this way, the ideas of ‘mental module’, (mind-related) ‘lexicon’, its communal equivalent ‘lexical structure’, and the earlier ‘conceptual scheme’ promised Kuhn an explanation of what he had discovered by first-person observation and corroborated several times using the historical record. That is, all views of the world require some holistic form of pre-conditioning, and different times and cultures have their own systems, seen as radical discontinuity in the historical record of science.

But even after this, the meaningfulness and applicability of this kind of explanation depend on whether the historical record actu-

ally shows such changes and discontinuity. Let us conduct a thought experiment. What if Kuhn had not arrived at the conclusion that the history of science is full of radical transitions but had found only continuity and accumulation of data? This would be a case where scientists operate with one conceptual scheme and keep specifying and correcting it gradually. Would he have insisted that scientists are not getting anything right about the world, or perhaps admit, expressed in Kantian terms, that the phenomenal worlds of scientists are achieving better and better approximations to the world as such? Kuhn answered this question:

The threat to realism is the foremost of the problems I have in mind ... A lexicon acquired ... gives members of the community that employs it conceptual access to an infinite set of lexically stipulated worlds ... As time passes, continuing research excludes more and more possible worlds from the subset that could be actual. If all scientific development proceeded in this way, the progress of science would consist in ever closer specification of a single world, the actual or real one. (Kuhn, 2000, p. 76)

But Kuhn’s point, ultimately, was that it does not.

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