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, 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 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.
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 coherence 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 coherelist 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 coherelist epistemology. (Kuukkanen, 2007, p. 559; my emphasis)

In other words, the point is not that Kuhn was a rationalist, because he was a coherelist. We are apparently in agreement that Kuhn can be seen as a rationalist with or without coherelist epistemology. 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 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, 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 coherelist 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’. 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

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1 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 be-
come 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 re-
cently by Statthis Psillos (2005) on the other side of the debate.2

The authors note that one obstacle to overcome is Kuhn’s objec-
tion to the correspondence theory. For how otherwise can one
maintain that Kuhn would have been able to agree about the con-
vergence 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 cor-
respondence was fundamentally flawed? The authors are not im-
pressed by my suggestion, and I am not alone in making it (e.g.
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). Unfor-
unately, this suggestion is just bound to add more confusion about
the correspondence theory of truth. It is true that the correspond-
ence 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 theorists have 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 cor-
respondence theory of truth was thus the denial of a mind-inde-
pendent 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 func-
tioned 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 him-
self as an ‘unregenerate realist’ (Kuhn, 2000, p. 203). He also im-
plied 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 meaning-
less 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 medi-
vated 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 be-
yond our reach.

Šešelja and Straßer correctly point out that latterly Kuhn sug-
gested 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 reluc-
tant 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 appar-
ent reason for the rejection is that the nature of objects depends on
our conceptual schemes or epistemic capacities (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 con-
clude that the non-epistemic character of the correspondence the-
ory 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 sci-
cientic 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 sci-
ence 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 rea-
soning 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,

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2 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' ‘remembers 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 actually 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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