Kuhn, the correspondence theory of truth and coherentist epistemology

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Abstract

Kuhn argued against both the correspondence theory of truth and convergent realism. Although he likely misunderstood the nature of the correspondence theory, which it seems he wrongly believed to be an epistemic theory, Kuhn had an important epistemic point to make. He maintained that any assessment of correspondence between beliefs and reality is not possible, and therefore, the acceptance of beliefs and the presumption of their truthfulness has to be decided on the basis of other criteria. I will show that via Kuhn’s suggested epistemic values, specifically via problem-solving, his philosophy can be incorporated into a coherentist epistemology. Further, coherentism is, in principle, compatible with convergent realism. However, an argument for increasing likeness to truth requires appropriate historical continuity. Kuhn maintained that the history of science is full of discontinuity, and therefore, the historical condition of convergent realism is not satisfied.

Keywords: Thomas Kuhn; The correspondence theory of truth; The coherence theory of justification; Convergent realism; Rationalism

1. Introduction

There are two themes in Kuhn’s philosophy that have energised a whole generation of philosophers of science ever since the first critical engagement between Kuhn and the philosophers of science. One is the threat to scientific realism posed by Kuhn’s model of scientific development. It has been asked whether the notions of incommensurability and paradigm mean that scientific revolutions cannot produce more truth-like theories but merely different ones. The other concern has been that Kuhn is impugning the rationality of science, because paradigm change allegedly changes all standards of evaluation, making inter-paradigm theory comparison impossible. I am not denying that some of Kuhn’s writings give grounds for the above views. My aim is to show that his philosophy does not necessarily lead to these conclusions. Despite its anti-realist credentials, it offers ingredients for a philosophy compatible with both rationality of science and convergent realism.

Throughout his career, Kuhn argued that we should abandon the correspondence theory of truth. Unfortunately, he failed to understand the nature of the
correspondence theory as a non-epistemic theory, and therefore his argument against it fails. Nevertheless, Kuhn had an important epistemic point to make. He correctly maintained that it is impossible to evaluate correspondence between beliefs and reality. For this reason, Kuhn concluded that the acceptance of propositions, including the presumption of their truthfulness, has to rely on some other epistemic criteria.

Kuhn suggested that we should switch our epistemological perspective from a static and foundationalist one, as in logical positivism and empiricism, to what he occasionally called the ‘historical perspective’. The crux of the latter is that the prevailing or inherited system of beliefs is taken as presumptively justified, and the focus of epistemic evaluation is shifted from beliefs per se to changes of beliefs. During periods of normal science, the aim is to change the system minimally. When beliefs are changed, scientists employ five or six inter-theoretic, or even inter-paradigmatic, values. The most important of these is problem-solving, because, for Kuhn, science is essentially solving problems, and other values can be subsumed under it. I show that problem-solving can be unproblematically connected to a coherentist epistemology. What is more, there are indications in Kuhn’s writings that he might have accepted this conclusion. Surprisingly, this means that Kuhn implicitly agreed that there could be a rational method of inter-paradigmatic theory comparison.

Especially in his later writings, Kuhn argued more or less directly that scientists have attempted to increase coherence in the history of science. However, Kuhn did not make a claim that the total coherence of science has increased in the course of history, and moreover, it is hard to find any historical evidence for it. Kuhn held that scientists’ attempts to improve the coherence of their theories paradoxically tend to decrease the total coherence of science, because that activity leads to the fragmentation of science. This kind of speciation or specialisation of scientific fields is how Kuhn came to understand scientific revolutions in the latter part of his career.

The scientific realist may insist that, whether or not the coherence of science has increased, the success of science needs an explanation, and suggest that the only explanation that does not make the success of science miraculous is the one that says that current theories are (approximately) true, and truer than earlier less successful ones. Yet this argument relies not only on the conceptual explanation of such notions as truth-likeness or verisimilitude, but also on an empirical verification of convergence. Kuhn can be said to have emphasised the importance of the historical record of science in deciding on the validity of convergent realism. Although Kuhn had some reservations with regard to the concept of truth-likeness, he eventually rejected convergent realism on historical grounds.

This paper is structured along the themes presented above. Firstly, I discuss Kuhn’s (flawed) rejection of the correspondence theory of truth. Secondly, I demonstrate in detail how Kuhn’s philosophy fits a coherentist epistemology. Thirdly, I examine how, and on what conditions, coherence and correspondence can be linked. Finally, I evaluate Kuhn’s assessment of convergent realism, arguing that his rejection is empirically motivated, although not necessarily empirically substantiated.

2. Rejection of the correspondence theory of truth

In various texts, Kuhn rejects the correspondence theory of truth. However, it is far from clear what Kuhn’s reason for this apparent rejection was, and even whether he can actually be said to have objected to the correspondence theory in the first place. Bird (2000, pp. 225–227) pays attention to Kuhn’s statement in a paragraph in his Postscript to The structure of scientific revolutions, where Kuhn says that there is ‘no theory-independent way to reconstruct phrases like “really there”; the notion of a match between the ontology of a theory and its “real” counterpart in nature now seems to me illusive in principle’ (Kuhn, 1970, p. 206). Bird agrees with Hoyningen-Huene’s interpretation in thinking that Kuhn’s argument here is actually epistemological. According to Hoyningen-Huene, the problem that Kuhn highlights is that there is no independent access to both theory and reality (Hoyningen-Huene, 1993, pp. 263–264). As Bird correctly remarks, this is a misguided attack if the target is the correspondence theory of truth. Even if we could not assess a match between a theory and reality, it does not make the idea that truth consists in a relationship of correspondence between an independent world and our beliefs, theories, and so on, meaningless. In other words, the correspondence theory is a theory that offers an interpretation of what truth is without any epistemic concern as to whether we can know the truth.

This suggests that Kuhn failed to understand the non-epistemic nature of the correspondence theory of truth. Perhaps a more charitable interpretation is that he did not formulate his argument carefully enough. In any case, however, there is a serious epistemological concern behind Kuhn’s statements. As we see below, in reference to the correspondence theory, Kuhn denies two things: (1) that we can check the correspondence between a truth-bearer and a truth-maker, whatever they are; (2) that we can measure the distance of any truth-bearer from truth at a specific point of time.

In two of his later important essays, ‘The trouble with the historical philosophy of science’ and ‘The road since Structure’ (both in Kuhn, 2000), Kuhn describes what it means to make a move from a static epistemological perspective—where beliefs per se are the focus of evaluative practices, such as the one by logical positivism and empiricism—to his dynamic historical perspective, where the focus is on changes of beliefs. Three consequences arise from this shift of perspective: the first consequence is an abandonment of the traditional Archimedean platform; the second is that evaluation becomes comparative; and the third is that it becomes impossible to judge whether
our theories or beliefs correspond to reality (ibid., pp. 111–116, 94–96).

It should be noted that the third consequence actually derives from the first. Because ‘no Archimedean platform is available for the pursuit of science other than the historically situated one already in place’, it follows that ‘what is fundamentally at stake is . . . the correspondence theory of truth, the notion that the goal, when evaluating scientific laws and theories, is to determine whether or not they correspond to an external, mind-independent world’ (ibid., p. 95). Never mind that Kuhn calls this notion ‘the correspondence theory of truth’; his point is that any evaluation of correspondence is impossible. Kuhn says that in ‘the previous tradition in philosophy of science, beliefs were to be evaluated for their truth or for their probability of being true, where truth meant something like correspondence to the real, the mind-independent external world’. He notes that we can seldom or never carry out such an evaluation directly to reality (ibid., p. 114).

We need to follow a couple of more steps of Kuhn’s reasoning in order to see why he also concludes that any assessment of convergence to the truth is impossible. The abandonment of the Archimedean platform and its consequence, the impossibility of direct evaluations of correspondence, means that any judgement of the truth (of anything) has to be reached indirectly—in other words, we have to use some other epistemic criteria when evaluating our beliefs and theories. According to Kuhn, ‘they [i.e. these other epistemic criteria] provide a replacement for the traditional Archimedean platform’ (ibid., p. 96). But, Kuhn tells us, ‘there is a price to be paid for’ this indirect (and comparative) evaluation: ‘A new body of belief could be more accurate, more consistent, broader in its range of applicability, and also simpler without for those reasons being any truer’ (ibid., p. 115; original emphasis).

It is important to realise that Kuhn does not deny that scientific theories and laws could be closer to the truth: that ‘successive scientific law and theories grow close and closer to the truth . . . could be . . . the case’ (ibid., p. 115). The problem is (again) that only a ‘fixed, rigid Archimedean platform could supply a base from which to measure distance between current belief and true belief. In the absence of that platform, it’s hard to imagine what such a measurement would be’ (ibid.). Kuhn’s concern here is an epistemic one—that in the absence of an Archimedean platform, it is impossible to measure directly the distance from the truth of any given belief, theory or law. While it is far from certain that our indirect evaluative criteria lead to truth, we cannot in any case know whether this is or is not the case.

Is Kuhn’s epistemological concern something that is worth taking seriously? The answer has to be ‘yes’. Let us for the moment ignore the specific form of his argument (that is, his reference to the absence of an Archimedean platform), and instead focus on the general problem that is expressed by it. The main point in Kuhn’s argument is that there is no direct and unproblematic access to truth, and therefore, the evaluation (of the truthfulness) of beliefs and theories necessitates the employment of some other epistemic values. This is something about which, I believe, consensus prevails among contemporary philosophers. In any case, Kuhn’s point is recognised as a fundamental issue in epistemology by two prominent epistemologists. Laurence Bonjour puts it as follows:

If truth were somehow immediately and unproblematically accessible (as it is, on some accounts, for God) . . . then the concept of justification would be of little significance . . . But this epistemically ideal situation is quite obviously not the one in which we find ourselves. We have no such immediate and unproblematic access to truth, and it is for this reason that justification comes into the picture. (Bonjour, 1985, p. 7)

In parallel with this, Nicholas Rescher notes that as a definitional account of truth the correspondence theory is superior, but as a criterion of truth the correspondence theory encounters drastic difficulties (Rescher, 1973, pp. 5–9). He writes,

. . . due to the inherent impossibility of ‘confronting the facts’—the correspondence theory is criteriologically of no avail in coming to a decision as to whether or not a given proposition is to be classed as true. (Ibid., p. 185)

For this reason, Rescher concludes, we need to consider some other ways to form a judgment as to whether a proposition is true, and suggests that the coherence analysis is best for this purpose.

Expressed in Rescher’s terminology, Kuhn’s point is that criteriologically the correspondence theory of truth is not viable. It cannot offer us a workable criterion that could be applied in an evaluation whether a belief is true or not. Compare this to Kuhn’s conviction 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 shared by all’ (Kuhn, 2000, p. 99). Therefore, as long as we are concerned with how actual scientists function, we cannot refer to correspondence between beliefs and reality as an explanatory notion in their decisions on theory choice. That would constitute a completely non-natural claim, implying the sort of access to reality that only some privileged being could have. However, to repeat, this, of course, is not an argument against the correspondence theory as such. Even if Kuhn was right about its (in)applicability, the correspondence theory may well be the best theory to pin down what it means to say, ‘P is true’.

3. Kuhn and the coherence theory of justification

Kuhn variably called his anti-foundationalist epistemological point of view the ‘historical perspective’ (ibid., p. 113), the ‘developmental perspective’ (ibid., p. 91), or the ‘developmental view’ (ibid., p. 95). This part of Kuhn’s philosophy is best understood against the backdrop of logical positivism and empiricism, whose significance for
Kuhn’s thinking Bird has especially emphasised (e.g. Bird, 2000, p. x, pp. 278–280; Bird, 2002, 2004). Kuhn wished to abandon the epistemological foundationalism of these previous traditions in the philosophy of science. For example, after having expressed his view that no Archimedean platform is available, Kuhn tells us that this conclusion is ‘pretty generally accepted: I scarcely know a foundationalist any more’, and that the correspondence theory ‘must vanish together with foundationalism’ (Kuhn, 2000, p. 95). Further, Kuhn declares that, in his early career, he ‘was primarily motivated by widely recognised difficulties . . . in positivism and logical empiricism’ (ibid., p. 106) and makes clear that the abandonment of these traditions leads to his historical perspective.

We may anticipate what follows in this paper at this point and consider what is the major alternative theory of epistemic justification to foundationalism: coherentism. I will show below in detail that Kuhn’s philosophy indeed fits with a coherentist epistemology.

The historical perspective stems from research into the history of science, but is an epistemological framework specifying how beliefs ought to be evaluated. Its fundamental premise is that scientific activity should be embodied in narrative that starts with a description of what people believed at the beginning. Further, it holds that the beliefs described are already in place and provide the basis for ongoing research, which may result in changes in the body of accepted beliefs. Everyone is bound up with a certain historical situation involving a particular relevant body of theory that is used for the evaluation of knowledge, and the evaluation is thus carried out against a background of accepted beliefs.

The focus is not on the evaluation of beliefs as such, because the whole inherited system of belief is more or less just taken for granted, as if presumptively justified, making it pointless or even impossible to ask justification of individual beliefs. Therefore, ‘what’s to be evaluated is the desirability of a particular change-of-belief’ (ibid., pp. 95–96) or ‘small incremental changes of beliefs’ (ibid., p. 112; original emphasis). A central problem is ‘why . . . given the body of belief with which they began, do the members of a scientific group elect to alter it’ (ibid.). On one occasion Kuhn compares the traditional investigation of ‘the rationality of belief’ to his proposal to focus on ‘the rationality of incremental change of belief’ (ibid.).

For everyone who has taken Kuhn as a philosopher of scientific revolutions, there is an element of surprise hidden in the implications of the historical perspective: with respect to scientific change, later Kuhn’s attitude appears moderate up to the point that he may be described as an epistemological conservative, one who has adopted a piecemeal approach to theory change. The case is actually more complicated than this, but it is still true that later Kuhn’s theory of theory choice has the flavour of epistemological conservatism. If the whole system of beliefs is taken as presumptively justified, it is rational to attempt to improve the justification of the old system, rather than to reject the whole system and try to construct an alternative one. Construction of any new system of beliefs is clearly a laborious task, and it is, without some pre-developed alternative, probably impossible.2 This means that any evidence that suggests radical changes to the accepted system is likely to be resisted. Indeed, Kuhn’s ‘developmental view’ dictates that the maxim adopted in revising an old theory is to change it with ‘minimum disruption’ (ibid., p. 96). He says that the transformation of our webs of belief should be seen as piecemeal.

Is the claim here, then, that the philosopher who became famous by destroying the myth of cumulative progress of science in favour of the revolutionary model, is really an advocate of a conservative piecemeal approach to scientific change, or that he arrived at this conclusion in his later career? The full response to this question must wait a little longer, but neither of these suggestions is strictly speaking correct. Kuhn did not abandon his idea of scientific revolutions even in his later thinking, although he came to modify his account significantly (e.g. ibid., pp. 97, 119, 250).

To repeat, Kuhn’s polemic against the correspondence theory was that any examination of correspondence between beliefs and reality is beyond the reach of scientists, and therefore, correspondence with facts or reality cannot be a criterion for the acceptance of beliefs. Consequently, Kuhn’s historical perspective requires the employment of such epistemic values in explanations of the history of science as can properly be said to be available to practising scientists. Kuhn writes:

Nothing about the rationality of the outcome of the current evaluation depends on their [beliefs], in fact, being true or false. They are simply in place, part of the historical situation with which this evaluation is made . . . Justification does not aim at a goal external to the historical situation but simply, in that situation, at improving the tools available for the job at hand. (Ibid., p. 96)

This quotation raises the question of what epistemic values are used for justification of belief changes, that is, how do the scientists evaluate and judge whether there should be a change of belief(s) in the system?

First of all, any evaluation of the desirability of a change of belief is comparative. Judgement is based on the comparison of two theories or ‘two bodies of knowledge’ and what is asked is whether ‘the original or the proposed alternative—is better for doing whatever it is that scientists do’ (ibid., p. 96; original emphasis). Kuhn’s view since The structure of scientific revolutions had been that scientists try to solve puzzles, and the choice between two theories turns, therefore, to the question of whether the suggested alternative manages to solve a puzzle that the old theory

could not, or whether it can solve more puzzles than the old one.³ It is worth pointing out that problem-solving retains its role as a primary factor of consideration in theory-choice in Kuhn’s later writings. The quotation from ‘The road since Structure’ (presented and published in 1990) immediately above continues as follows: ‘And that [the comparative judgment] is the case whether what scientists do is solve puzzles (my view) ...’ (Kuhn, 2000, p. 96). Further, in ‘Afterwords’ (a conference presentation from 1990), Kuhn writes,

the answer supplied in Structure still seems to me the right one: whether or not individual practitioners are aware of it, they are trained to and rewarded for solving intricate puzzles ... That is what they are trained to do and what ... they spend most of their professional lives doing. (Ibid., p. 251)

We may add that that in an article from the turn of the 1990s (‘The natural and the human sciences’ in Kuhn, 2000) Kuhn ponders whether human sciences could ever become puzzle-solving research like natural sciences. The implication is clearly that the latter is best described as a problem-solving activity.

Kuhn also offers a more specific characterisation of the criteria for the evaluation of theories. There is a whole set of apparently intertheoretical, or even interparadigmatic, criteria used in evaluation: accuracy, consistency, breadth of applicability, simplicity and fruitfulness. In the comparative evaluation of a change of belief(s) we ask: ‘which of two bodies of beliefs is more accurate, displays fewer inconsistencies, has a wider range of applications, or achieves these goals with the simpler machinery’ (ibid., p. 114; original emphasis). Kuhn presents on several occasions similar, yet slightly variable lists of criteria that can be used in comparison.⁴

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 makes it fit well with a coherentist epistemology. Although Kuhn did not suggest this directly (some occasional allusions can be found), I believe this extension of his philosophy does not distort his thinking. My suggestion is based on two inherent elements in his thinking with which we are now familiar: epistemological conservativeness, and the idea that science is fundamentally problem-solving.

To begin with, conservatism in theory choice is something that takes us smoothly to coherentism in epistemology. Suppose that a highly coherent system is constructed. A person does not have any incentive to change it, should coherence decrease as a consequence. To the contrary, s/ he has an incentive not to change it in such a case. And so, in the absence of an alternative (more coherent) system, or of a change that improves the coherence of the old system, a person should not change his/her beliefs. Naturally, the principle of coherentism works in the other direction as well: if there is a good reason for a change, that is, if coherence can clearly be increased, then the system ought to be changed. In a case where one or more anomalies appear for which there is no satisfactory explanation available, then (conservatively) amending the existing system or occasionally even devising a new conceptual system may offer such an explanation, and thus increase overall coherence. Consequently, Bonjour writes that achieving a high degree of coherence may sometimes require a significant conceptual change: ‘in this way the progress of theoretical science may be plausibly viewed as a result of the search for greater coherence’ (Bonjour, 1985, p. 100).

Furthermore, coherentism fits in general with Kuhn’s insistence that we should concentrate on the justification for changes of beliefs rather than on the justification of the beliefs themselves. If our beliefs are holistically justified, that is, if the beliefs, in a set, mutually justify each other, then we have to understand the whole set as given and try to improve it, rather than to try to find a justification for individual beliefs on a one-by-one basis or (even less) to start the construction of a new system from scratch. Justification is dependent on such a large set of interconnections that it is impossible to ask for a complete justification for each belief. Rather, it is the case that when a belief is changed, the rest of the beliefs stay unchanged. For each change one has to ask for a specific justification, and improved coherence counts as such.

Now, let us remind ourselves why we are preoccupied by coherentism. The first thing to take note of is that

³ There is a statement in Kuhn (1970, p. 157) that seems to contradict this account: ‘... paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms’. However, the context makes clear that it is not problem-solving as an epistemological value which is in doubt, but whether scientists are always able to compare directly the relative problem-solving capacity of two theories. It is crucial to be able to anticipate future problem-solving ability: ‘A decision between alternate ways of practicing science is called for and in the circumstances that decision must be based less on past achievement than on future promise’. Also, compare this statement to numerous claims that emphasise the importance of problem-solving.

⁴ Cf. Kuhn (1970), pp. 152–155, ‘Postscript’, p. 199; Kuhn (1977), pp. 322–324. However, there appears to be a subjective element in theory choice because the values on which theory choice is based may be applied differently by different individuals. Yet, this does not make theory choice arbitrary or irrational. The shared values, however differently shaped, seem to lead to the same theory choice by community members, as ‘most members of the group will ultimately find one set of arguments than another decisive’ (Kuhn, 1970, p. 200). See Hoyningen-Huene (1993), pp. 147–154, pp. 239–245, for more references to Kuhn. Elsewhere, Kuhn concludes that the values in question form the standard list of rational criteria for evaluation of scientific beliefs and confirms the universality of these values (Kuhn, 2000, pp. 251–252; see also ibid., p. 118; cf. Laudan, 1984a, pp. 91–92).
coherentism is a theory of justification. On the ground that it is impossible for scientists to have direct access to truth, Kuhn plunged into a classification of values that could be used in theory evaluation by scientists. There are now clear indications that Kuhn’s historical perspective can be accommodated to a coherentist theory of justification. However, in order to demonstrate the match between Kuhn’s epistemology and the latter in more detail, we urgently need to clarify further the notion of coherence from the merely intuitive level. I will explicate the notion of coherence with the help of Bonjour (1985, Ch. 5), who offers a clear account of the notion. 6

The customary way to characterise coherence is by stating one or two conditions: consistency or/and explanatory unity. Consistency means that no coherent set can contain both beliefs that P and not-P. Explanatory unity means that each belief P of a set is explained by other beliefs in the set. Consistency is also Bonjour’s first condition for coherence:

(1) A system of belief is coherent only if it is logically consistent. 7

A further factor to be considered is that of the points of contact between the components of the system. We need to require that there be some positive connections between the beliefs that make up the system. If they are totally unconnected, we cannot say that the set is very coherent. Bonjour writes that a natural idea is that connections are inference relations. There should be such relations that a belief or a set of beliefs can serve as the premise(s) of an argument for a further belief. Furthermore, if a system has subsystems, their coherence is higher the more they are connected with each other. It is important to notice that both the relations between beliefs inside a set and the relations between sets that form a larger system can be understood to be a matter of degree. Bonjour formulates two further conditions for coherence, as follows:

(2) The coherence of a system of beliefs is increased by the presence of inferential connections between its component beliefs, and increased in proportion to the number and strength of such connections.

(3) The coherence of a system of beliefs is diminished to the extent to which it is divided into subsystems of beliefs that are relatively unconnected to each other by inferential relations.

Bonjour argues that we need to highlight one kind of inferential relation for our purposes, namely the explanatory relation. In science and other epistemological tasks the goal is to explain a scope of phenomena, including phenomena of different kinds, by a relatively small number of explanatory principles. A coherentist system tries to connect an anomalous event or fact to known ones by finding inferential connections between it and the rest of the system in the course of looking for an explanation for it. Anomalies threaten the status of the explanatory principles of the system as general and basic, and thus decrease the coherence of a system. Bonjour adds, therefore, one more condition for coherence:

(4) The coherence of a system of beliefs is decreased in proportion to the presence of unexplained anomalies in the believed content of the system.

This concludes our exposition of the notion of coherence. There are three criteria to be taken into account in determining the coherence of a system: its consistency, the degree of inferential connections it contains and the number of anomalous instances it exhibits.

Now we come to the crucial part. We have to assess how epistemological coherentism meshes with Kuhn’s characterisation of science as a whole, and specifically, how it agrees with the criteria that he suggests are used in theory choice. As we saw above, the feature of science that Kuhn elevated above any other is problem-solving. It is true that sometimes Kuhn lists it next to other criteria of evaluation; yet, it also clear that problem-solving is more important than his other criteria. It is something that is the distinguishing mark of the whole scientific period, that is, normal science. It is ‘what scientists do’ (Kuhn, 2000, p. 96)—‘whether or not individual practitioners are aware of it’ (ibid., p. 251).

That science is problem-solving is a conclusion one arrives at by taking a look at history over a long period of time. It is a metahistorical view, expressing what the nature of science is on the whole. If we exclude problem-solving, which can be used to characterise science both as a criterion as it is actually applied in theory choice by scientists and as a characterisation of the nature of scientific activity in general, the other criteria that are supposedly used in theory choice are descriptions on the former level only, that is, they are only applicable in the actual situation where scientists choose theories. In Kuhn’s philosophy, the

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5 However, terminology varies between different advocates of coherence theories. Rescher’s intention is not to devise a coherence theory of truth to compete with the correspondence theory, despite the name of his 1973 book, The coherence theory of truth. Therefore, I am bound to agree with Bonjour that it is best to call the kind of coherence theory that he and Rescher are interested in the coherence theory of justification (Bonjour, 1985, p. 88 and p. 239 n. 1).


7 Bonjour points out that coherence should not be equated with logical consistency, i.e. the absence of explicit contradiction. There may be cases where a consistent system does have a low degree of coherence. By using the notion of probabilistic consistency, we may imagine a case where a person holds the belief that P and also the belief that it is extremely improbable that P. The set is consistent, although it would be more coherent if those two beliefs would be dropped altogether. Therefore, probabilistic consistency is another feature to be taken into account (Bonjour, 1985, pp. 95–96). I ignore this specification here, but that does not affect in any way the general argument made here.
usage of these standards contributes to greater success in problem-solving. For example, Kuhn sees that improved quantitative precision, that is, accuracy, makes a theory more likely to succeed in problem-solving than its competitors (Kuhn, 1970, pp. 153–154). More generally, Kuhn makes it clear that the function of other values is to indicate the success in problem-solving:

the rationality of the standard list of criteria for evaluating scientific belief is obvious. Accuracy, precision, scope, simplicity, fruitfulness, consistency, and so on, simply are the criteria which puzzle solvers must weigh in deciding whether or not a given puzzle about the match between phenomena and belief has been solved. Except that they need not all be satisfied at once, they are the ‘defining’ characteristics of the solved puzzle. (Kuhn, 2000, pp. 251–252; original emphasis)

My suggestion is that problem-solving is a natural component of a coherentist epistemology. The connection between coherence and problems (or anomalies) is obvious: Problems, that is, phenomena unexplained by the machinery of the set, decrease the number and strength of inferential relations between the components of the set, making the system less coherent. A potential problem is that Kuhn’s criteria—consistency, scope, accuracy, simplicity and sometimes also fruitfulness—do not all appear to correlate directly and unproblematically with a higher degree of coherence. The least problematic is consistency, because the concept of coherence can be directly explicated by it. Also, both scope and simplicity can be taken as values that enhance the degree of coherence in a system. This is because simpler or more powerful principles are used in explanations and the wider the scope of the phenomena they subsume, the more and stronger explanatory connections there are in the system; therefore, less subsystems are needed, that is, the system is more coherent. Accuracy (or precision) might be, then, interpreted as tendency to produce minimal anomalies in the system. That is, in an accurate system, there would be an agreement between predictions made and experimental results. It would be a challenging task to link fruitfulness with the concept of coherence. Perhaps it could be taken as something like a promise of future problem-solving capacity. However, it is not a problem if it, or any other poorly fitting criterion, contributes to greater success in problem-solving, and problem-solving, in turn, makes a contribution to achieving a higher degree of coherence. In that case, all criteria are linked either directly or indirectly via problem-solving to coherence, which makes Kuhn’s philosophy consistently coherentist. Problem-solving is what science is fundamentally, and it is part of the more general search for greater coherence in science.

It is an interesting question whether Kuhn himself would have accepted this description of him as a coherentist. To say, ‘Yes, he would have’, might be too strong; but there are certainly indications scattered in his texts that suggest that he might well have accepted it. An early indication can be found in The Copernican revolution. According to Kuhn, coherence was an important epistemic value in enabling the Copernican revolution to occur:

The ear equipped to discern geometric harmony could detect a new neatness and coherence in the sun-centered astronomy of Copernicus, and if that neatness and coherence had not been recognized, there might have been no Revolution. (Kuhn, 1985, p. 172)

Compare this to Kuhn’s characterisation of what happens in revolutionary change: ‘In revolutionary change one must either live with incoherence or else revise a number of interrelated generalizations together . . . Only the initial and final sets of generalizations provide a coherent account of nature’ (Kuhn, 2000, p. 29; my emphasis). Quite clearly, the implication is that scientists do not tolerate incoherence but try to achieve as coherent an account of nature as possible. Moreover, we can pay attention to how Kuhn comments on Hempel’s description of the aims of science. Hempel says, ‘Science is widely conceived as seeking to formulate an increasingly comprehensive, systematically organised, world view that is explanatory and predictive’. Kuhn remarks, ‘Because it loosens the commitment to any particular prespecified goal like puzzle solving, Hempel’s formulation is an improvement of mine’ (ibid., p. 210). I do not think it is far-fetched to suggest that the terms ‘increasingly comprehensive’ and ‘systematically organised’ could be replaced by the word ‘coherent’.

Furthermore, interestingly, we can also find other evidence, independent of Kuhn, that scientists have attempted to increase coherence. The most forceful argument that increasing coherence has been a significant driving motor behind theory changes arguably comes from Thagard (1992; see also Kitcher, 1993). Thagard shows how the increase of (explanatory) coherence has been a motivating force for theoretical transition behind such well known scientific revolutions as Lavoisier’s chemical revolution, Darwin’s evolutionary revolution, Hess’s theories of seafloor spreading and plate tectonics (over Wegener’s theory of continental drift) and a group of revolutions in physics (the Copernican, Newtonian, Einsteinian and quantum mechanical revolutions).

In conclusion, while Kuhn obviously takes problem or puzzle-solving as the most fundamental characteristic of science, he hints that he would not have objected to a more comprehensive description of the aim of science, such as increasing coherence. Our consideration of the relationship between problem-solving and coherence leads indirectly to the same result: if scientists, as a matter of fact, attempt to solve puzzles willy-nilly, and this approach can be more comprehensively described as coherence-increasing activity, then we are warranted in saying that they try to increase coherence of science. Therefore, we are justified in thinking that Kuhn made an empirical claim regarding the actual practice of scientists. That is, whether always aware of it or not, scientists try to make a theory as
coherent as possible and to choose a more coherent successor if one it is available.

This conclusion may appear surprising against the background of the so-called rationality debate, which involved such figures as Popper, Lakatos, Kuhn and Feyerabend. The received wisdom is that Kuhn and Feyerabend were irrationalists, while Popper and Lakatos were the defenders of rationality. Now we can see that this may be too crude a simplification of the actual situation. Kuhn clearly accepts that there are standards that enable rational comparison in scientific change, even to the extent that we might claim that he would have approved of the notion that there is also a rational method of theory comparison operating in science: coherence analysis.

4. Coherence and correspondence linked?

Evaluation of theories via coherence (or the values subsumed under the notion of coherence) was meant to be something that can be used as a criterion when deciding on the acceptability of theories. One may ask whether coherentism has any other virtues in addition to being available in practical situations of theory choice. What reasons, if any, are there to accept the coherence theory as a normative theory in epistemology?

Coherentism could be motivated by maintaining that it is a truth-generating method. Perhaps coherentism can be used to discriminate between beliefs that are and that are not presumptively true. Expressed in Rescher’s terminology, because it cannot be decided directly, or because one cannot be directly and unproblematically aware that P is true, that is, T(P), we need some criterion to give us an adequate rational warrant for classing P as true, that is, C(P) (Rescher, 1973, pp. 12–26). Accordingly, both Rescher (e.g. 1973, pp. 23–24, pp. 39–40) and Bonjour (e.g. 1985, pp. 157–158, pp. 169–179) suggest that the role of the coherence theory is to provide a rationale for thinking that an inquirer whose beliefs are justified according to the standards of the coherence theory is likely to arrive at truth. Adhering to coherentist standards is thus ‘truth-conducive’. Further, Rescher (1985) argues that truth is tantamount to ideal coherence. The attempt to achieve a maximally coherent system may be motivated by an attempt to construct a system that best estimates ideal coherence, that is, the true description of the world.

More generally, the scientific realist is faced with a related problem with truth. He/she holds that the aim of science is truth itself, but a sensible realist would not claim that scientific beliefs or theories are, or have ever been, strictly taken, true. Taking into account the evidence from the history of science, this appears to be the only sensible conclusion. This is also something that Kuhn is keen to highlight. He refers to the pessimistic meta-induction and points out that all previous beliefs have turned out to be false, which makes it likely that our current beliefs will meet the same fate (Kuhn, 2000, p. 115). Yet, the realist won’t be moved by the actual falsity of previous and current beliefs, because the real issue is whether theories are becoming truer. Further, he/she is likely to contend that the success of science indicates that this is indeed the case. Let’s then suppose that we find out, as a matter of fact, that a particular scientific field, or even the whole science, has become more coherent over the long run (=success). The scientific realist would maintain that this increase of coherence indicates that theories are truer. This can be taken as the case where scientists are trying to achieve as coherent a theory as possible, ending up with an approximately true one, and truth therefore turns out to be a by-product of scientific activity (cf. Bird, 2000, p. 214). We may imagine this as being a kind of invisible hand of truth, in the form of justification, which guides scientists in their theory choices.

This debate raises again the question of how the notion of truth itself is understood. An easy answer would be to advocate not only a coherence theory of justification but also a coherence theory of truth. Truth would mean something like an ideal fit or ideal coherence. This would link justification to truth inherently: if a theory increases its coherence, it will also be truer by the same token, because truth is identical to justification-in-the-long-run. However, the realist typically understands truth as correspondence with reality, and such a coherence theory of truth is definitely not their choice. Further, most contemporary coherent theorists agree that the correspondence theory is the best candidate to express what truth means (e.g. Rescher, 1973, pp. 23–24, p. 27, pp. 184–185, Bonjour, 1985, pp. 158).

Unfortunately, there is a problem in respect of the alleged connection between coherence and correspondence: the coherence criterion does not necessitate or logically guarantee truth (e.g. Rescher, 1973, p. 31, Bonjour, 1985, pp. 157–158). C(P) is not equivalent to T(P). No matter how strong a warrant we have that P is true, it is always theoretically possible that P is false. Specifically, it cannot be discounted that there are several equally coherent systems that are either equally true or equally far from the truth. Therefore, if one wants to connect coherence to correspondence, s/he has to offer an argument for it. Bonjour correctly recognises that if the only rationale for the chosen concept of truth (read: correspondence concept of truth) is an appeal to the related standard of justification, then the argument loses it force. It would be circular to say that a certain standard or epistemic justification is correct because it is truth-conducive and that the conception of truth in question is correct because it can connect in this way with the suggested standard of justification (Bonjour, 1985, pp. 109–110). In other words, an argument for the truth-conducivity of the chosen standard of justification has to be motivated independently.

Psillos has recently offered an argument that purports to overcome this circularity problem. The strategy is to distinguish between vicious premise-circularity and non-vicious rule-circularity. According to Psillos, the difference is that in the former, the conclusion is either identical with or a paraphrase of one of its premises, while in the latter, the
argument is an instance or an application of the rule of inference vindicated by the conclusion. He suggests that the principle of Inference to the Best Explanation can be shown to be reliable (that is, truth-tropic) by way of a rule-circular argument, and the argument is therefore persuasive to someone who does not share the intuition of its reliability (Psillos, 2005, p. 82). Psillos’s idea relies on the No Miracles Argument (NMA), that is, the notion that realism, with its claim that theories are approximately true, is the only philosophy that does not make the success of science a miracle. He thinks that by a meta-IBE, we can conclude that the background theories that led to the instrumental and predictive successes of science are approximately true. Further, since these (approximately true) theories have been arrived at by first-order IBEs, this realisation, together with the conclusion of the meta-IBE, entails the reliability of the IBE. Finally, NMA is not premise-circular, because the truth of the conclusion of NMA is only a part of a sufficient condition for taking IBE as reliable (ibid., p. 83).

However, it is not clear at all that the argument really retains its probative force for someone who does not already accept that IBE is truth-conducive. Another scientific realist, Peter Lipton, candidly and correctly, I think, suggests that the miracle argument can rather be conceived of having some use in defending against the charge of circularity in internal debates amongst scientific realists (Lipton, 2004, pp. 191–192). Psillos leaves it somewhat unexplained how someone who does not accept IBE as legitimate or and does not believe that IBE is truth-tropic would accept NMA. For example, an instrumentalist, even one who accepts IBE as a form of inference, would at most be ready to concede that the success of science indicates that the observational consequences of the theories in use, not the theories themselves, are true. And if the first step is blocked, then the whole argument attempting to show the reliability of IBE (in the realist’s sense) fails.

How, then, might we argue for truth-convergence in the history of science? Firstly, an advocate of such a view should define the notion of verisimilitude or truth-likeness. It is clear that early attempts, such as Popper’s, failed to make sense of the notion of truth-likeness. Since then, there has been an admirable amount of work carried out around the notion of truth-likeness and verisimilitude by Ilkka Niiniluoto, Graham Oddie and others. A recent suggestion is that truth-likeness equates to truth plus similarity, and similarity is used for measuring distances from the truth (e.g. Niiniluoto, 1999). Perhaps measuring distances from the truth does not necessarily require an Archimedean platform, as Kuhn suggested; Kuhn’s point may be taken as a statement that the measurement cannot be given with certainty or come with an absolute guarantee.

Nevertheless, there certainly are major difficulties in pinning down the concept of verisimilitude or truth-likeness. Consequently, some realists are losing their faith in attempts to formalise the notion of truth-likeness. Psillos would be ready to rely on ‘the intuitive notion of truth-likeness already operating in science’ (Psillos, 2005, p. 278). But this strategy raises the question of how strong the rational appeal of this intuitive approach is for those who do not take the notion of truth-likeness as intuitive, and the intuition does not appear to be universally shared (cf. Kuhn, 1970, 2000; Rescher, 1977; Laudan, 1981, 1984a). For example, for Rescher, it has ‘little rational appeal’ (Rescher, 1977, p. 170). He contends that it is rational to think that our latest theories are better qualified for endowment with the presumption of truth, but one should not ‘equate a tentative claim to truth with a claim to tentative truth’ (ibid., p. 191).

At the moment I would be ready to give the benefit of doubt to those who wish to try to explicate the notion of verisimilitude or truth-likeness. However, in addition, we also need to show that it applies to the history of science. In other words, one should verify empirically that science is converging. Bonjour sensibly expresses the condition for the case of convergence in the history of science. He writes that coherenstist standards may produce two different types of results in the long run. The system of beliefs might involve constant and relatively wholesale changes over time, and thus not approach any stable conception of the world. The other alternative is that the system of beliefs gradually converges on some definite view of the world, and thereafter remains relatively stable (Bonjour, 1985, p. 170). The point is that any reasonable argument for truth-convergence requires that the latter condition prevail. In the absence of continuity and stability of any kind, that is, in the case of progress largely through revolutions and total discontinuity, hitting the truth at some point would be a sudden lucky shot, of which we might be wholly unaware. This option is simply implausible and unreasonable.

Also those impressed by NMA have to show that there is a relevant kind of continuity in the history of science. Laudan (e.g. 1981, 1984a,b) has argued that there have been successful but fundamentally wrong theories (by our current standards). This is clearly unacceptable for a scientific realist, as it cuts the link between success and truth. As a consequence, Psillos correctly suggests that realists should accept Laudan’s historical challenge. According to Psillos, realists should use, what he calls the divide et impera move to show that the theoretical laws and mechanisms which have generated the success of past science have been retained as constituents of our current theories. He calls these constituents ‘truth-like constituent theoretical claims’. Because they have carried over to subsequent theories, they represent the best approximations of the truth (Psillos, 2005, pp. 108–111).

In other words, either the history of science settles on a certain conception of the world and remains stable, or it

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8 This is largely due to the Tichy–Miller refutation of Popper’s definition (e.g. Niiniluoto, 1999 pp. 65–68; Miller, 1974).
keeps constantly changing. If there is constant change in the history of science, it is totally unreasonable to say that there is any empirical-historical evidence for convergence and an increasing correspondence to reality, which makes the whole argument for truth-convergence hollow. Then again, if the history of science converges to a certain view, it raises an opportunity to argue reasonably that it may be best explained by the fact that the theories correspond to reality. Of course, there is no way to make a foolproof case, and no scientific realist is likely to argue that an increase of coherence is an infallible sign of a higher degree of verisimilitude or truth-likeness. Nevertheless, let it be granted that, if the development of science shows continuity, increasing coherence and stability over the long run, an argument for the (approximate) truth of theories has some intuitive appeal. The crucial question is whether these conditions prevail in practice, that is, whether an increasing degree of coherence, convergence and stability are detected in the history of science or not.

First of all, it is not in doubt that the later Kuhn still thought that there are scientific revolutions and periods of normal science in the history of science, although in his later work, revolutions have become local and signify splits of scientific fields to new branches and subspecialties (Kuhn, 2000, pp. 250, 97, 119–120). It is useful, then, to employ the pair normal–revolutionary science in our interpretation. Let us first note that revolutions do not constitute a problem for the coherentist account, because increase of coherence is in no way connected to the stability of the existing scientific ontology or putative references. The view that arises out of Kuhn’s writings is one where the scientists’ aim, to increase coherence of their theories and find as unifying an account of nature possible, leads to scientific revolutions in the new sense, that is, to splits of fields of science. Kuhn writes,

Knowledge is the particular business of subspecialties, whose practitioners struggle to improve incrementally the accuracy, consistency, breadth of applicability, and simplicity of the set of beliefs they acquired during their education, their initiation into practice … Occasionally the process runs aground, and the proliferation and reorganization of specialties is usually part of the required remedy. (Ibid., p. 117; original emphasis)

At the time of normal science, theories are changed piecemeal, but occasionally revolutions force themselves on the scientific community. We may imagine this as a situation where anomalies persist, which gives an incentive to develop an alternative explanation or theoretical system over the years, so that it finally evolves to form a separate scientific field. Kuhn thinks that specialisation is something that is necessary for the development of science, although it is deplorable if one is trying to find a coherent and unifying worldview:

Specialization and the narrowing of the range of expertise now look to me like the necessary price of increasingly powerful cognitive tools … To anyone who values the unity of knowledge, this aspect of specialization—lexical or taxonomic divergence … is a condition to be deplored. But such unity may be in principle an unattainable goal, and its energetic pursuit might well place the growth of knowledge at risk. (Ibid., p. 98)

Scientists are, in other words, forced to introduce new subfields of science. The paradoxical result is that their aim to increase coherence of their fields, which may be in fact achieved by getting rid of the most problematic parts, results in a decrease in the unity of science. Kuhn thus did not make an empirical claim that coherence has increased in science. It seems also most likely that there is no other evidence for that conclusion either. The fact of specialisation suggests that the contrary is the case. For example, Rescher points out that, according to Britannica, there were nine branches of physics with nineteen specialties in 1911, but already sixteen branches with 205 specialties in 1970 (Rescher, 1978, pp. 226–229). Furthermore, the integration of the theory of quantum phenomena and the theory of relativity poses a special problem in physics. It is also likely that the total number of disciplines in general in science has increased in the course of history. Of course, this is not to say that it would be impossible to find relations of reduction between these specialties, or unifying explanations, in the long run, or that there is necessarily some kind of incommensurability between different fields, but it clearly indicates that it is hard to make a case that there has been an overall increase of coherence in science.

The lack of evidence for increase of coherence does not directly threaten the realist’s argument, because success can be understood differently, but the historical challenge remains. Can we, then, historically argue that there is continuity in the history of science? One can, of course, focus on many aspects in this search for continuity (e.g. conceptual, referential or structural continuity, or continuity of truth-content etc.), but if we are arguing for convergence as a kind of ideal final description, that is, as a view or picture of the world, then there should be convergence in the basic concepts and ontologies of theories. Unsurprisingly, Kuhn argues against that. In The Copernican revolution (1985) Kuhn had already expressed his conviction as follows:

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9 If the different knowledge systems in different fields turn out, at the end of all scientific inquiry, to be mutually contradictory, it raises the question whether one has achieved the truth. Let us suppose that these all systems are part of one large system of knowledge. Is it reasonable to suppose that this system corresponds to reality? If the answer is yes, would this mean that reality itself is somehow contradictory? Isn’t it more reasonable to think that all our beliefs in this system cannot be true? This indicates that coherence has to be a feature of any true system of knowledge, just as Rescher has emphasised (e.g. Rescher, 1973, Ch. 7; 1985).

But though achievements of Copernicus and Newton are permanent, the concepts that made those achievements possible are not. Only the list of explicable phenomena grows; there is no similar cumulative process for the explanations themselves. As science progresses, its concepts are repeatedly destroyed and replaced. (Kuhn, 1985, pp. 264–265)

In addition, beside his overall argument that the history of science comprises scientific revolutions, he is ‘as a historian ... impressed with the implausibility of the view’ that looks for a match between the ontology of a theory and nature (Kuhn, 1970, p. 206; my emphasis). Kuhn says further,

I can see in their [Newton’s and Einstein’s mechanics] succession no coherent direction of ontological development. On the contrary, in some important respects ... Einstein’s general theory of relativity is closer to Aristotle’s than either of them is to Newton’s. (Ibid., pp. 206–207)

This relative closeness of relativistic physics to Aristotelian than to Newtonian is a case that ‘stand[s] for many’ (Kuhn, 2000, p. 206). After that, he talks of vain attempts at ‘zero-ing in on nature’s real joints’, and of the need to replace the goal-directed approach with an evolutionary one. Kuhn is thus saying in effect that, in general, there is no convergence in basic ontology or in concepts in the history of science. Therefore, Kuhn urges people to abandon the idea that scientific progress has to be defined as a movement towards some goal, and hints that this is a relic, just like the pre-Darwinian evolutionary theories that took evolution to be a goal-directed process planned by God (Kuhn, 1970, pp. 170–173). In the course of his career, Kuhn becomes ever more convinced that an evolutionary conception should replace the old teleological conception.

It is true that Kuhn did not offer enough historical evidence for his view, which Sharrock and Read (2002) are at pains to emphasise. This may probably be explained by the fact that year-by-year he moved closer to non-empirical philosophical argumentation. There are naturally others who have argued against convergent realism on empirical grounds, such as Larry Laudan. Further, on the other side, Psillos has admirably followed up his divide et impera strategy and attempted to prove that the several stages of the caloric theory of heat and the nineteenth-century optical ether theories retained the essential constituents responsible for their respective successes, and that they, therefore, support the realist’s argument (Psillos, 2005, Ch. 6).

The point here is that the question of whether there is convergence in the history of science has to be settled both by conceptual clarification (of such concepts as truth-likeness or verisimilitude) as well as by empirical studies of the history of science. Although Kuhn had some reservations with the regard to the notion of truth-likeness, he assigned to empirical historical research a central role in deciding the issue of convergence. Despite the fact that the empirical data he offered is too thin to corroborate the conception, Kuhn was convinced of the implausibility of the teleological view because of the historical record.

5. Conclusions

We have seen that Kuhn’s argument against the correspondence theory of truth is epistemological. Because Kuhn thought that it is impossible to assess correspondence between beliefs and reality, he relied on certain inter-theoretic values to explain theory choice. For Kuhn, problem-solving is the most important standard of evaluation, under which all other epistemic criteria can be subsumed. I showed that problem-solving can be incorporated into a coherentist epistemology. This means that Kuhn implicitly accepted that there is a rational method of comparison in science, that is, coherence analysis. Furthermore, coherentism is compatible with a view that maintains that increasing coherence is linked to progress towards truth. Although Kuhn argued that the history of science does not yield support for convergent realism (and for an overall increase of coherence in science), convergent realism is not incompatible with his philosophy because Kuhn’s argument is ultimately empirical. Kuhn may thus be said to have emphasised the role of historical research in the debate on convergent realism. Unfortunately, Kuhn did not offer enough historical data to strongly corroborate his claim. Finally, we may conclude that both conceptual clarification and empirical substantiation are needed in deciding the issue of convergence in the history of science, which therefore requires both philosophical and historical reflection. Kuhn’s career is a testimony of a fruitful interaction between these two perspectives on science.

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References
