

Summary of some criticisms (from others) of Thomas Kuhn on science

Summarized by Terence Rajivan Edward, 7th Sep 2024

History of science and paradigm shift. Kuhn says that conversion to a new paradigm in science does not happen by deliberation, rather is like a religious conversion, but he offers evidence for his new paradigm in the history of science for readers to deliberate over. If conversion by deliberation cannot occur in science, then it cannot occur in history of science, argues Israel Scheffler (1967; 1972: 366-367).

Low puzzle-pressure revolution. Kuhn thinks that a dominant theory, or paradigm in one of his uses, is replaced only after a build up of anomalies: unsolved puzzles for the theory. The anomalies produce intolerable pressure and scientists lose confidence in the theory. However, John Watkins asserts that there are examples in the history of science of once dominant theories being replaced while there is low pressure from anomalies. He gives the example of Kepler's laws being replaced by Newton's theory (1970: 30-31).

Why value normal science? Watkins suggests a criticism that Kuhn lacks good reasons for holding in esteem plodding, uncritical Normal Science. Its commonness, compared to the rarity of Extraordinary Science, is not a good reason, for what is rare may be more important. Watkins thinks Kuhn sees scientific communities by analogy with a religious community and sees science as the scientist's religion. "Extraordinary Science corresponds, on the religious side, to a period of crisis and schism, confusion and despair, to a spiritual catastrophe." (1970: 33)

Theory of matter: no normal science. Watkins reports Karl Popper observing a long period of scientific history with no clear paradigm and without the typical symptoms of normal science. In the theory of matter, from pre-Socratics to the present day, there has been unending debate between "continuous and discontinuous concepts of matter, between various atomic theories on the one hand, and ether, wave and field theories on the other." (1970: 34)

The Instant Paradigm Thesis: psychological discreditation. Watkins identifies some theses Kuhn is committed to concerning paradigm change. (1) A paradigm enjoys monopoly on a scientist's thinking. (2) There is little or no interregnum between the end of the old paradigm's reign over a scientist's mind, and the beginning of the new paradigm's reign. (3) A new paradigm is incompatible with the old one. Given (1) to (3), (4) the change over must be swift and decisive. And given (1) to (4), (5) the new paradigm must be invented in one piece - the instant paradigm thesis. This is not psychologically plausible (1970: 36).

The Instant Paradigm Thesis: historical counterexamples. "The Inverse Square Law was an important component of Newtonian theory...; and Pierre Duhem has traced the long evolution of the Inverse Square Law back through Hooke, Kepler, and Copernicus, to Aristotle's idea that bodies seek the centre of the earth." (Watkins 1970: 36)

Incompatible and incommensurable? Kuhn thinks that earlier and later paradigms are both incompatible and incommensurable, but if two theses are incommensurable, how can they be incompatible? "If someone holds that, say, Biblical myths and scientific theories are incommensurable, belong to different universes of discourse, he presumably implies that the Genesis account of the creation should not be regarded as logically incompatible with geology, Darwinism, etc." (Watkins 1970: 36; see also Scheffler 1967: 82)

Micro-revolutions. Stephen Toulmin proposes that Kuhn's distinction between long periods of normal science and short extreme revolutionary periods will have to be replaced by a portrait of science in which something like these revolutionary periods, but less extreme, happen more regularly: micro-revolutions (1970).

Twenty-one senses. Margaret Masterman does not present this as a criticism, but it could readily be taken as such. Kuhn uses the word "paradigm" in twenty-one different senses, according to her (1970: 61-65).

Description-prescription ambiguity. Paul Feyerabend finds Kuhn ambiguous regarding whether he is telling the scientist how to proceed or whether he is merely giving a description of those activities called "scientific." (1970: 198)

Organized crime and puzzle-solving. Kuhn tries to distinguish science from non-science as involving a puzzle-solving tradition. Feyerabend responds with this counterexample: "For organized crime, so it would seem, is certainly puzzle-solving par excellence. Every statement Kuhn makes about normal science remains true when we replace 'normal science' by 'organized crime'; and every statement he has written about the 'individual scientist' applies with equal force to, say, the individual safebreaker." (1970: 200)

Early proliferation. Paradigms are overthrown because of the way in which alternatives focus on existing anomalies, Kuhn thinks. But when are these alternatives formulated? For Kuhn, there is a lengthy period of normal science and then, when a build-up of anomalies undermines confidence in the paradigm, alternatives are searched for. But Feyerabend thinks that there is a proliferation of alternatives from an early stage and so there is no such thing as normal science as described by Kuhn (1970: 207-208). He also attributes this early proliferation view to Imre Lakatos.

The puzzleless Pre-Socratics. "It might be objected that the puzzle-solving activity, though not sufficient for bringing about a revolution, is certainly necessary as it creates the material which eventually leads to trouble: puzzle solving is responsible for some of the conditions on which scientific progress depends. This objection is refuted by the Presocratics who progressed (their theories did not just change, they were also improved) without paying the slightest attention to puzzles." (Feyerabend 1970: 208)

Translates the supposedly untranslatable. This is from Donald Davidson (1982 [1973-4]: 67). Kuhn describes the scientific language before the revolution as untranslatable into the language after the revolution, and vice versa, but then he goes on to tell us about the language before the revolution using our post-revolutionary language, thereby succeeding with translation.

Not radically interpretable. This is also from Davidson (1982 [1973-4]: 77-79). The meaning of a sentence is the meaning that would be attributed to it by a radical interpreter, who must learn the language from scratch with evidence of when sentences are asserted. But a radical interpreter would always interpret the assertions of other scientists so that there is considerable common ground of shared belief. They would never find scientists whose assertions are so different to ours as to be incommensurable.

One world. Davidson responds to Kuhn's claim that scientists before and after a revolution live in different worlds by asserting that there is at most one world (1982 [1973-4]: 69).

Relations between communities. Maurice Mandelbaum identifies a different possible cause of a scientific revolution than a build up of anomalies which a puzzle-solving community ignores, until the pressure on their guiding theory is unbearable (1977). What is going on in other communities, which members are aware of, can be a cause. (The demand to do fieldwork in social anthropology came at a time when professional fieldwork was being pioneered in various other disciplines. See Kuklick 1997: 47.)

A duck-rabbit disanalogy. Maurice Mandelbaum criticizes Kuhn for using reversible figures to analyse what goes on in scientific perception in general. "In looking at the photographic plate, the astronomer can simultaneously recognize what he sees "as a point of light" and also "as a star," but what he sees is not a reversible figure. Rather, that which is seen can be described in either of two ways, each of which is equally applicable at the same time. In a reversible figure, on the contrary, what I see at any one time is either a rabbit or a duck, not both." (1982 [1979]: 47)

Vision-centric. In a footnote, Mandelbaum doubts the value of using reversible figures to understand all perception, because "reversibility as it occurs in vision does not occur in any of the other sense modalities." (1982 [1979]: 58; see also Scheffler 1972 for criticism of Kuhn's appeal to reversible figures).

The impossible historian. Mandelbaum thinks that given his claims of the deep effect of training in a scientific language and incommensurability, Kuhn cannot explain the position of the historian of science, who is able to describe the world from the points of view of an old and a new theory. Such a historian would be impossible if Kuhn's claims were true (1982 [1979]: 50).

Epistemological tension. On the one hand, Kuhn appeals to a standard set of criteria for when one theory is more adequate than another. On the other hand, the standard interpretation of these criteria relies on a traditional epistemological doctrine which gives observations a foundational role, contrary to Kuhn (Mandelbaum 1982 [1979]: 53). Kuhn does not address how to interpret the criteria without this traditional epistemology.

Successful paradigms. John A. Moore argues that some paradigms are successful, for example we are not looking for new continents now (1980). We can rely on such paradigms and solve most or all of the puzzles that arise, putting an end to the sequence of normal science, revolution, normal science, revolution, etc.

Modular perception. Kuhn thinks theory-neutral observation is impossible, but Jerry Fodor argues that there is theory-neutral observation by drawing attention to how some beliefs do not affect experience: believing that a figure gives an illusory appearance does not make the illusion go away. Fodor argues that perception is informationally-encapsulated: the process that produces a perceptual experience does not have access to some beliefs/theories (1984: 35).

Analytic-synthetic problem. The semantics which goes with Kuhn's claims of meaning change with revolutionary theory change seems to be inferential role semantics, according to which the meaning of a word is determined by its role in inferences. A revolutionary change

in theory is a change in inferential roles for some terms. But not all inferences are meaning-constitutive, Fodor and Lepore argue (1993). Inferential role semantics requires a distinction between meaning-constitutive inferences and other inferences, but this distinction entails the distinction between analytic and synthetic truths, when there is no such distinction, they argue.

Not just puzzle-solving. John Earman suggests a criticism when he writes, “I also think that there is more to progress in normal science than puzzle solving. In particular, I would emphasize conceptual advances derived from the exploration of the space of possible theories.” (1993: 32)

Copernican versus Ptolemaic explanation. Ernan McMullin identifies phenomena which the heliocentric model explains “that Ptolemy simply had to postulate as given, as inexplicable in their own right.” (1993: 72) Kuhn asserts that both systems explain the phenomena, but the Copernican one explains it more naturally. McMullin objects, “Here I must disagree. The Ptolemaic system does not explain the phenomena mentioned above at all.” (1993: 73) McMullin’s objection seems the same as Richard J. Hall’s (1970).

Prediction and explanation. McMullin objects that Kuhn equates prediction and explanation (1993: 73). The Ptolemaic system can be predictively equal to the Copernican system but worse in terms of explanation (1993: 74).

Ad hoc and taste. According to Kuhn, “To astronomers the initial choice between Copernicus’ system and Ptolemy’s could only be a matter of taste, and matters of taste are the most difficult of all to define or debate.” McMullin thinks that Copernicans criticized ad hoc moves of Ptolemy and this is not an appeal to taste (1993: 73). (In general, Kuhn has been much criticized for portraying revolutions as non-rational changes.)

Physics-centric/physical-science-centric. Joseph Agassi says that Kuhn’s examples are mostly from physics, which is an unusual case (2002). Steve Fuller writes: “Kuhn’s examples are drawn almost exclusively from the physical sciences” (2003: 21) This (kind of) point was probably made earlier, but I don’t know who said it first.

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