Chapter 5

The Kuhnian Straw Man

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In the present chapter, I argue that commentators who criticize Kuhn's work are most often fighting a straw man. Their target is a stereotype that is not to be found in Kuhn's texts. I will consider the charge based on the stereotype that the Kuhnian schema is not borne out by historical evidence and will argue that Kuhn's model, which is not actually what his critics take it to be, was not supposed to be based on, or accurately depict, historical facts. It was not a historical representation but a philosophical model that was used to challenge an ideal image of science. I suggest that giving a more accurate account of Kuhn's model will not only do justice to Kuhn's work but also draw attention to issues that, because of the stereotype, have remained in obscurity.[1]

1. INTRODUCTION

Kuhn's *The Structure of Scientific Revolutions*, opens with a promissory sentence: "History, if viewed as a repository for more than anecdote and chronology, could produce a decisive transformation in the image of science by which we are now possessed" (1970a, 1). Kuhn promised to transform, via his book, the image of science that scientists, philosophers, and laypersons were possessed by at the time. What was this image of science? It was the so-called textbook image of science, which was also attributed to the logical positivists.[2] What did this textbook image of science, explicitly, or implicitly involve? It involved the belief that science progresses steadily; that scientists accumulate knowledge by testing theories against nature, discarding falsified and retaining the confirmed ones; that there is uninhibited communication among scientists of all times and of all specialties, guaranteed, if needed, by recourse to a common and readily available pure observational language

distinct from any theoretical ones; that scientists aim at a true depiction of the world which they approximate as they move from one theory to the next; that revolutions in science are not a problem in this steady development as they precipitate progress. Now, what was the image that Kuhn put in its place? Kuhn's account of science was succinctly and poignantly captured, in its stereotypical form, by Barry Barnes (1982, 13): "long periods of dreary conformity interrupted by brief outbreaks of irrational deviance." The long periods of dreary conformity correspond to what Kuhn labeled normal science, that is, to periods of scientific practice where scientists follow, almost dogmatically and unwaveringly, what particular exemplars dictate, while the outbreaks of irrational deviance refer to the Kuhnian revolutions with their attendant implication of incommensurability, which signifies radical discontinuity, lack of communication across the revolutionary divide and problems of comparative rational evaluation. Kuhn's talk of techniques of persuasion, to which scientists of different allegiances resort in order to convert one another when revolutions occur, exacerbated the worries about irrationality.

But, why did Kuhn want to transform the then dominant image of science? Obviously, he must have thought that there was something wrong with it. And what was wrong with it, according to the standard reading of Kuhn's work, was that the textbook image of science did not conform to the historical record and how science works. So, Kuhn had to advance a better image of science that would not only do justice to historical facts but would also be based on historical facts. It would not just be a better philosophical theory that could accommodate and be consistent with the historical facts. Rather, it would also start with the historical facts and be confirmed by historical facts. Kuhn's significant engagement with history of science prior to the writing of *Structure* and the presence of several examples from the history of science in *Structure* added credence to the view that takes Kuhn's image of science to be based on evidence drawn from history.

2. WAS KUHN'S MODEL BASED ON HISTORICAL EVIDENCE?

The standard reading credits Kuhn with a specific model of science, which is said to have been based, rather unsuccessfully, on historical evidence, and which involves the block replacement of frameworks that are separated between them by an abyss.[3] In assuming that Kuhn's model is based on historical evidence, the standard reading seems to ignore some contrary considerations:

- that Kuhn's arguments in *Structure* in defending his model are philosophical, not historical (see Kindi 2005, 504–6; 2015).

- that the logical positivists or logical empiricists who were credited, even if wrongly, with the textbook image of science never cared to have their model of science conform to historical facts. Feigl, for instance, a founding member of the Vienna Circle and a prominent logical positivist talking about the "orthodox view" of scientific theories said that "it should be stressed and not merely bashfully admitted that the rational reconstruction of theories is a highly artificial hindsight operation which has little to do with the work of the creative scientist" (1970, 13). Also, Carl Hempel, a proponent of logical empiricism in the United States insisted that the standard construal of scientific theories "was never claimed to provide a descriptive account of the actual formulation and use of theories by scientists in the ongoing process of scientific inquiry; it was intended, rather, as a schematic explication that would clearly exhibit certain logical and epistemological characteristics of scientific theories" (1970, 148). So, it would not be fair to criticize the logical positivists for failing to depict facts or to attribute to Kuhn such criticism. In fact, Kuhn never used this kind of argument against them. Hence, it is unlikely that he promised to substitute an accurate account of science for the logical positivists' inaccurate one. What is more, he believed that historical facts could be made to confirm any kind of theory: "If you have a theory you want to confirm, you *can* go and do history so it confirms it, and so forth; it's just not the thing to do" (Kuhn 2000d, 314, emphasis in the original).[4]
- that Kuhn and others like him may have initially thought that they were deriving their conception of science from historical facts but, eventually, Kuhn said he realized that this was misleading. "I and most of my coworkers thought history functioned as a source of empirical evidence. That evidence we found in historical case studies, which forced us to pay close attention to science as it really was. Now I think we overemphasized the empirical aspect of our enterprise " (Kuhn 2000b, 95). According to Kuhn, assuming a historical perspective, that is, seeing science as an ever-developing enterprise, was by itself enough to allow them to derive their model from first principles (Kuhn 2000c, 111–12).[5]

Kuhn's critics ignore all these considerations that speak against the view that Kuhn's model is empirical and go ahead to give evidence against it. So, it has been argued, on empirical grounds, that there are no Kuhnian revolutions, that scientists do communicate despite their different allegiances, that crises do not always precede revolutions, and that there are no conversion-like phenomena.[6] All these claims presuppose and target the stereotypical understanding of Kuhn's model, which, I propose, functions as a straw man. It can easily be attacked, but it is not the real thing.

Let us consider the case of revolutions. According to the stereotypical model, Kuhnian revolutions are rare, abrupt, dramatic, and transformative

events, which mark sharp breakdowns that affect a large number of people. But Kuhn actually speaks of the discoveries and the inventions of theories that bring about revolutions as processes that are not "isolated events" but "extended historical episodes" (1970a, 2–4, 7, 52, 55). He believes that revolutions occur frequently and may affect "perhaps fewer than twenty-five people" (1970a, 181; cf. Kuhn 1970b, 249–50).[7] And, finally, Kuhn says that he has an "extended conception of the nature of scientific revolutions" covering different kinds (1970a, 7). So, arguably, Kuhn would not necessarily object to calling revolutions the intellectual changes in the "long seventeenth century" that Garber (2016) contrasts to what he understands as Kuhnian revolutions. According to Garber, there were no Kuhnian revolutions during this period since the Aristotelian paradigm was not challenged by a single theory but by a diverse group of alternatives. However, there is no such requirement in Kuhn's work. Kuhn himself compared revolutions that involve small paradigm changes to the "Balkan revolutions of the early twentieth century" and said that they may affect a small number of people and may be considered part of the normal course of events by outsiders (Kuhn 1970a, 92–93). He also said that he resisted pronouncing on whether a certain development in the history of science was "normal or revolutionary" unless he had done the historical work. "I usually have to answer that I do not know. [. . .] Part of the difficulty in answering is that the discrimination of normal from revolutionary episodes demands close historical study" (Kuhn 1970b, 251).[8] So, the Kuhnian revolutions found in Kuhn's texts hardly resemble the stereotype of Kuhnian revolutions.

References to techniques of persuasion and to conversion in *Structure* are also used to build the stereotypical model. They are taken to imply that, according to Kuhn, scientists change allegiance from one paradigm to the next, not by rational argument but by beguiling rhetoric and/or some mystical "thunderbolt intuition" (Daston 2016, 128). But Kuhn did not have this view. He never excluded arguments from the repertoire of scientific communication; he actually spoke of persuasive *arguments*. He explicitly said that scientists are reasonable and that "one or another *argument* will ultimately persuade many of them" (Kuhn 1970a, 158, emphasis added). What Kuhn denied was that scientists are compelled, either by logic or by experiment, to accept a particular paradigm. They may use arguments, not rhetorical tricks, to persuade, but when they advocate different paradigms, they may not share the premises of these arguments and may end up talking past each other. Thus, interlocutors, according to Kuhn, need to be persuaded about premises first, in order to proceed and accept what logically follows from them. And what will persuade them about premises are further arguments that will elaborate on the advantages, for instance, fruitfulness, of the new paradigm and the promise it holds for them (Kuhn 1970a, 199).[9]

Conversion is another battered concept in relation to Kuhn. It is usually understood as instantaneous and mystical and, therefore, rejected, but for Kuhn, it is a process that involves the entire scientific community for an extended period of time.[10] As he says in *Structure* (1970a, 152), the conversion may sometimes require a generation to be effected. He did not say that a community undergoes, in unison, a dramatic transformation overnight.[11] He did bring up the metaphor of Gestalt switch and spoke of "the 'lightning flash' that 'inundates' a previously obscure puzzle," of flashes of intuition that may come to scientists even in sleep, of "scales falling from the eyes"[12] (Kuhn 1970a, 122–23) but only to contrast all these to deliberation and interpretation. His target was the view that there are fixed and naked data that are variously interpreted via an inferential process. Against this view, Kuhn maintained that a scientist's perception is shaped by a paradigm and needs to be reeducated and reshaped when a revolution occurs (Kuhn 1970a, 112). An anomaly, according to Kuhn, is not terminated by fetching a new interpretation for the same data, an interpretation formed in an inferential and piecemeal fashion, but by transforming the experience gained with the old paradigm to a different bundle of experience (Kuhn 1970a, 123). "The process by which either the individual or the community makes the transition from constrained fall to the pendulum or from dephlogisticated air to oxygen is not one that resembles interpretation" (Kuhn 1970a, 121–22). Kuhn did not mean to turn scientific development into a mystical affair.[13] He spoke of conversion in order to stress that scientists do not move from one paradigm to the next the way one infers a conclusion in a deductive argument. Confronted with the same constellation of objects, scientists may reshuffle them and see them differently (Kuhn 1970a, 122–23). That is why Kuhn finds the transition from Newtonian to Einsteinian mechanics a very clear illustration of scientific revolutions: no new objects, no new concepts, but rather "a displacement of the conceptual network through which scientists view the world" (Kuhn 1970a, 102). Conversion is just a metaphor for a non-inferential process that would render successive paradigms commensurable by making one the logical implication of the other. Actual conversion in science is not miraculous, as the Pauline version has it, but takes time and involves arguments, education and training.

Kuhn's critics seem to be arguing as follows: Kuhn aimed to transform the image of science by which we were possessed. He tried to substitute an empirically adequate model for the empirically inadequate one found mostly in textbooks. However, he failed: his revolutionary model is not corroborated by historical evidence, or otherwise defended, so it needs to be abandoned and replaced by one that does not highlight discontinuity and dramatic change.

In opposition, I contend that Kuhn did, in fact, aim to transform the image of science by which we were possessed, but he did not offer an empirically

adequate model in lieu of an empirically inadequate one. He did not base his model on historical evidence, so any criticism that simply aims to show that it clashes with historical facts cannot by itself damage the model. Historical facts can be made to conform to different philosophical accounts.

This dialectic raises, in turn, a number of questions: (1) If Kuhn did not offer to substitute a historically corroborated account of science for the one found in textbooks, what was he doing? What is the status and role of his model? (2) If the stereotypical sketch of Kuhn's model of science, that is, as a radically discontinuous and rationally unaccounted for succession of unrelated frameworks, does not find support in Kuhn's writings, as I have pointed out, was Kuhn's model closer to the traditional model of continuity and cumulative progress? (3) Why and how were Kuhn's critics led astray and built the straw man I am criticizing? (4) What is to be gained if the stereotypical model is dismantled and a more faithful account of Kuhn's work is painted? In the two remaining subchapters of this chapter, I will address these questions, in turn, starting with the first two.

3. THE STATUS AND ROLE OF KUHN'S MODEL

If Kuhn did not base his model on historical evidence, what is the status and role of his model? I tried to answer this question in Kindi (2005). I considered Kuhn's claim that his model can be derived from first principles and I argued that in *Structure*, he offered the conditions of possibility of the practice of science. These conditions involve the use of paradigms/exemplars to set rules that are followed dogmatically to shape normal science, which, in turn, defines what is normal and what is anomalous. An anomaly is eliminated when it is assimilated in a new set of rules, laid down by a new paradigm. The two sets, however, are not logically related, since what is anomalous in the first set is made "lawful" in the second. The move from one paradigm to the next constitutes a Kuhnian revolution. As Kuhn put it (1970b, 233), "[t]he existence of normal science is a corollary of the existence of revolutions [. . .] If it did not exist (or if it were non-essential, dispensable for science), then revolutions would be in jeopardy also."[14]

In Kuhn's model, revolutions presuppose the existence of normal science, which is necessary in order to provide the background of normalcy against which anomalies are to be detected and recognized for what they are—deviations from normalcy. "Novelty ordinarily emerges only for the man who, knowing *with precision* what he should expect, is able to recognize that something has gone wrong" (Kuhn 1970a, 65, emphasis in the original). Anomalies (and crises), in their turn, are bound to occur as normal science constantly generates puzzles extending the reach of paradigm and makes it

more precise.[15] The greater the number of puzzles, the more articulate and complex the paradigm; the greater the precision it achieves through normal science, the more sensitive it becomes to the emergence of anomalies. Anomalies are not prompted by the world in the way falsifications are, given that normal science does not test paradigms against the world, but they surface when the apparatus provided by the paradigm to solve its puzzles (the paradigm's concepts, models, experimental devices, etc.) falls short of the needs and expectations it breeds. Some of the puzzles it generates may not be possible to solve by its own means; they may need a totally new approach. Thus, normal science, a highly conservative and dogmatic enterprise, is paradoxically the indispensable condition for novelty and radical change; it is the mechanism that makes change possible.[16] Scientific revolutions, which depend on normal science, need to occur—they do not just happen to occur—in order for science to develop and progress.[17] This is the reason chapter IX of *Structure* is entitled "The Nature and Necessity of Scientific Revolutions."

Two issues immediately emerge. The first issue is the following: (a) Is the Kuhnian model of science, with its interlocking concepts of *paradigm*, *normal science*, *anomaly*, *revolution*, *incommensurability*, a mere tautology?[18] Hanson (1965) already criticized Kuhn for wavering between a possibly false historical claim and putting forward an unfalsifiable set of definitions and asked Kuhn to disambiguate. The dilemma described by Hanson, however, does not allow for a third possibility, which will be explained later in the chapter, namely, the possibility of using Kuhn's pattern as a model. Kuhn did not propose an empirical theory nor did he pontificate from his armchair offering either a speculative developmental schema or a metaphysical truth. Kuhn's model aims to show that science is not one thing but many different things, which become possible by having scientists following rules that are set by particular paradigms. Since paradigms differ, rules will differ and scientific practice and traditions, which are shaped by these rules, will differ.[19]

The second issue is the following: (b) If scientific revolutions are necessary, why does Kuhn say in *Structure* that "cumulative acquisition of novelty is not only rare in fact but improbable in principle" (1970a, 96)? Isn't this an indication that his account is, after all, empirical and an acknowledgment that it may turn out to be wrong? If it is not empirical, why doesn't he say that continuity in science is "impossible" rather than "improbable"?[20] One possible answer is that he does not want to rule out the possibility of continuity in scientific development since there is incremental acquisition of modest novelty during normal science. But I think a more appropriate answer can be gleaned from the character of Kuhn's model. The necessity of which Kuhn speaks is conditional, not metaphysical necessity. Kuhn does not pronounce some necessary truth about the world whose denial is an impossibility. He focuses on the conditions that make science, as we know it, and

its progress possible and offers a model of how these conditions work. This is not an invented model, arbitrarily imposed upon historical facts; rather, it is a model that has been informed by Kuhn's experience as a scientist and a historian. Kuhn knew firsthand what scientific training involves and learned from his historical work how variegated scientific practice is.[21] The pattern he outlined, *paradigm*, *normal science anomaly/crisis*, *revolution*, has not been inferred from history, but has been used more as an "object of comparison," which is laid against facts in order to highlight particular features of them, for example, scientific education and training on the basis of particular exemplars rather than rules, so that differences between scientific traditions are brought forward. Kuhn's model suggests a way of looking at facts. It does not tell us that facts have to conform to the proposed pattern in order to qualify as science.[22]

Does this mean that Kuhn's model is optional, that we can ignore it and use some other model to illuminate different aspects of science? Wouldn't the logical positivists' reconstructions be equally legitimate candidates? What does Kuhn's model have to recommend itself? Kuhn set himself a very specific goal: to transform the image of science by which we were possessed. This was an image that "held us captive" (PI section 115), that "[held] our mind rigidly in one position" (Wittgenstein 1960, 59) and could not let us see science differently. These are Wittgenstein's metaphors and are used by him in order to show how some preconceived ideas about meaning block us from seeing how varied language use is. So, Wittgenstein employed not only real but also imaginary examples of language use in order to combat an essentialist idea of meaning. Wittgenstein said to his student and friend Norman Malcolm (1984, 43):

> What I give is the morphology of the use of an expression. I show that it has kinds of uses of which you have not dreamed. In philosophy one feels *forced* to look at a concept in a certain way. What I do is to suggest, or even invent, other ways of looking at it. I suggest possibilities of which you had not previously thought. You thought that there was one possibility, or only two at most. But I made you think of others.

In a parallel move, Kuhn targeted an essentialist idea of science and used concrete cases from its history to show how varied scientific practice is. Unlike Wittgenstein, however, who invented fictional examples for his purposes, Kuhn had to appeal to actual cases to shake the deeply ingrained preconception that science is always one. Imagining mere possibilities in the case of science would not be as effective.

Kuhn's historical research showed him that the image of science by which we were possessed at the time he wrote *Structure* was not based on facts but

was rather imposed on facts. It was a philosophical ideal that required that science be defined by a certain method and advance continuously and cumulatively. Kuhn proposed a different concept of science. This other concept was not discovered in history but emerged from the change of emphasis, from science as theory to science as practice. In the science-as-theory model, scientific theories were linguistic constructs, which acquired meaning and were assessed to be true or false through their relation to observation sentences that were supposed to link theories to experience and the world. It was an abstract model that applied equally to all scientific theories, whatever the time and place. The problems addressed in that context (confirmation, reduction, explanation) were logical problems dealing with relations between sentences, irrespective of whether these sentences represented actual statements scientists made. Now, when attention was drawn to science-as-practice, that is, to what scientists do, how they are educated and trained, questions regarding how practices are constituted and how they develop acquired prominence. Kuhn said that practices are formed around a paradigm, that is, an exemplar that is being imitated and followed.[23] By being followed, rules specific to the particular paradigm are set and scientists are trained to use language, handle instruments, conduct experiments, etc., in accordance with them. In this context, meaning does not seep upward into vessel-like concepts from the soil of experience (Feigl, 1970) but is determined by use in accordance with specific rules in each particular practice. So, practices are individuated by the particular paradigms that govern them, which means that the landscape of science becomes varied. By focusing on scientific practice as formed around paradigms (which breed normal science, give rise to anomalies and revolutions), Kuhn's model of science brought into relief a built-in mechanism for differentiation and radical change. In that sense, Kuhn's model is particularly fit for the purpose Kuhn had set, namely the transformation of the ideal image of science. This does not mean that it can serve any other purpose. For instance, if one does history of science, as Kuhn himself did, one does not have to apply or look for the Kuhnian categories in one's field of study.

Now, if Kuhn's model is not to be identified with the stereotype that requires it to depict radical discontinuity in the history of science by the block replacement of incommensurable paradigms, should we continue to see it as a revolutionary model? Have I turned Kuhn from a revolutionary to a Social Democrat, as Newton-Smith (1981, 102–24) put it? No. Kuhn's work continues to be revolutionary, not because he substituted a radically different image of science for the one we were familiar with for a long time, but because he did away with the very idea of an ideal image for science. In his model, the different ways of doing science do not fall short of the ideal and cannot be explained away by appealing to human weaknesses and idiosyncrasies. Differentiation in scientific activity, small or big, made possible by adopting

different paradigms, is what makes science going. Kuhn's new image of science, which is actually a mosaic of different traditions, was not put together by generalizing from instances; it emerged once attention was drawn to what makes scientific practice possible, namely paradigms and what follows from them (normal, science, anomalies, revolutions). In accordance with Kuhn's own understanding of scientific revolutions, his revolution in the perception of science did not have to summon new facts or make new discoveries; it only needed a new perspective. Mary Hesse, in her review of *Structure* (1963, 286), captured nicely what Kuhn did:

> This is an important book. It is the kind of book one closes with the feeling that once it has been said, all that has been said is obvious, because the author has assembled from various quarters truisms which previously did not quite fit and exhibited them in a new pattern in terms of which our whole image of science is transformed.[24]

A change of perspective can bring about a completely new view of familiar things.

4. WHY WAS KUHN'S MODEL MISINTERPRETED AND WHAT IS TO BE GAINED FROM A MORE FAITHFUL READING?

I have argued earlier in this chapter that Kuhn's critics usually attack a stereotype of Kuhn's views. But why was his model misinterpreted and what will be gained if we try to redress things? Kuhn's critics misinterpreted him because they measured his model against the criteria and presuppositions of the so-called received view. In the received view, science is taken to mean scientific theories. Scientific theories are understood as sets of statements. These statements enter into relations of logical inference. In order for these inferences to go through, as in reducing one theory to another, the terms used in the statements should have a fixed, well-circumscribed, and stable meaning. The meaning of terms is acquired from the soil of experience on the one hand, and the theoretical postulates that connect them to other theoretical terms on the other. Even if there are changes in the theoretical part of meaning when revolutions occur, there always remains the observational part to guarantee continuity across theories and mutual understanding among scientists advocating different paradigms. When Kuhn, following N. R. Hanson, challenged the fixed and neutral nature of sensory experience and tied meaning to particular practices shaped by different exemplars and rules, the meaning of terms changed with the change of practice. For Kuhn's critics,

who ignored practice and took meaning to include a theoretical and observational part, this meant that nothing common remained between theories. Consequently, communication across different frameworks was not any more possible and rationality was undermined as the transition from one theory to the next could not be mapped onto a deductive inference.

Kuhn's account was forced into the received view framework and found inadequate as it was taken to yield the above undesirable consequences.[25] These consequences would not follow, however, had Kuhn's account been seen outside the box of the received view. If the dimension of practice were highlighted in Kuhn's model of science, then meanings and concepts would emerge from following rules and would not be seen as vessels to be filled with observational and theoretical content.[26] Meanings and concepts would be uses of words in imitation of paradigms and would be, thus, de-hypostasized, extended in time, open-ended, and flexible. The important consequence of this shift of vision is that if concepts are seen as uses of words, then attention is drawn to what agents do rather than to the role of concepts in logical inference. This means that, in assessing the rationality of transition from one theory to the next, one need not consider arguments in the abstract, but the circumstances of word use in particular actual practices in order to review the options available to the scientists and the decisions they made. Evaluating the transition becomes a practical rather than an abstract theoretical matter.

What is to be gained, apart from hermeneutical accuracy, if we lift the stereotype which screens Kuhn's work? First of all, we would not have to address bizarre suppositions, such as, that scientists advocating incommensurable paradigms cannot meet in the same world and have lunch together,[27] or that the same individuals are cut off from and do not understand their previous selves should they change allegiance in the course of their careers as scientists. Second, we would not devote our efforts to find or establish all kinds of common elements between successive paradigms in order to vindicate continuity and rational progress in accordance with a dated philosophical ideal. Instead, we would be more inclined to turn our attention away from the theoretical contemplation, which has stiffened our thinking in a particular abstract mode, to how scientists reason and work. We would then be more prepared to recognize diversity and view not only science but also rationality or experiment, as more malleable concepts. More importantly, we would be able to reexamine all these issues anew, from a fresh perspective. For example, instead of accounting for communication in terms of common content and shared elements in the classical definition of concepts,[28] elements that we try to detect or devise, we could attend to how scientists and scientific communities employ particular words, what rules and routines they follow, what goals they pursue, what synergies they forge to be able to understand each other. One might think that this is turning philosophy into sociology or

anthropology, but this is not necessarily so.[29] As in the case of Kuhn, who used historical facts to revise our conception of science, empirical considerations may be used to revisit other epistemic and, in general philosophical, issues. For instance, from this new perspective that takes concepts to be uses of words, one may want to examine how concepts are individuated, whether they are always evolving, what makes reconceptualization possible, how to understand radical change, how to differentiate between an aberrant development and an innovative approach, and so on. Or, one may also want to explore the implications of convergent thinking that is promoted during the Kuhnian normal science. Educators and psychologists usually think that convergent thinking inhibits creativity and prefer to encourage divergent thinking. Kuhn, however, considered convergent thinking a condition for creativity and innovation in science.

5. CONCLUSION

The Kuhnian straw man has been an obstacle for recognizing and appreciating the innovative character of Kuhn's work. It has distracted attention from what Kuhn has actually said and restricted the debate to worn-out arguments and the reiteration of standard topoi. If it were removed, we would be in a better position to assess what Kuhn's work has to offer. According to the present reading, Kuhn's model should not be understood as relying on historical evidence or as an unfounded schema but, rather, as a lens (or object of comparison) that highlights discontinuity and diversity in the history and the practice of science and focuses on what scientists do rather than on abstract theoretical arguments that formalize logical problems. Despite the misinterpretations of the model, some of which I have discussed, it has already succeeded in undermining the ideal image of science and in opening up new fields of study. So, I submit that before we move on to reject it as outdated, unfounded, or problematic, it would be worthwhile to first study it as what it is in order to explore whether there are aspects of it that have remained in obscurity and have not yet been taken advantage of. The usefulness of the model or its appropriateness will depend on the task we would like to undertake.

NOTES

1 I would like to thank Moti Mizrahi for his comments and suggestions that helped me to improve the chapter and both him and James H. Collier, executive editor of *Social Epistemology,* for giving me the opportunity to participate in this new dialogue on Kuhn's work.

2 Irzik (2012) shows that Kuhn's target in *Structure* was the textbook image of science and not logical positivism as it is commonly believed.

3 Galison (1997, 12) compared paradigms to "island empires."

4 Cf. Kuhn (1987, 363): "It is too easy to constrain historical evidence within a predetermined mold."

5 In Kindi (2005, 519–20), I argue, *pace* Kuhn, that assuming a historical perspective is not by itself enough to yield his model and I offer a different account of what Kuhn got from history.

6 Two of the most recent examples are the following: Lorraine Daston (2016, 128), criticizing Kuhn, says that being initiated into a new paradigm, learning how to reason with exemplars, "is a gradual process that proceeds in fits and starts, neither a thunderbolt intuition nor a conversion experience," while Daniel Garber (2009, 2016)–and quite a few other scholars–challenge the view that the scientific revolution, which is so much used to illustrate Kuhnian revolutions, is really a revolution as Kuhn described it.

7 Kuhn, in response to Ernan McMullin's distinction between shallow and deep revolutions, said (1993, 337): "Though revolutions do differ in size and difficulty, the epistemic problems they present are for me identical."

8 Cf. how Kuhn remembers his reaction when somebody from the audience in a lecture asked him whether he had found incommensurability in his historical research for the *Black Body* book: "I thought, 'Jesus! I don't know, I haven't even thought about that.' Now yes, I mean I *had* found it, and I later recognized what it was [. . .] It was a perfectly good question; I later realized how to answer it, but it just floored me at the time, and I sort of stammered around" (2000d, 314). Kuhn's reaction shows that he did not derive his concept of incommensurability from his historical work. Incommensurability was an implication of his model and he could retrospectively recognize it in history.

9 Cf. Kuhn (1970b, 234): "To say that, in matters of theory-choice, the force of logic and observation cannot in principle be compelling is neither to discard logic and observation nor to suggest that there are no good reasons for favouring one theory over another."

10 This is actually something that holds in general about conversions. William James (1997, 160), for instance, distinguishes between gradual and sudden processes of conversion. Cf., also what Lewis Rambo (2003, 214) writes about religious conversion which is usually taken to be radical, total, and sudden: "In fact, most human beings change incrementally over a period of time; even after a long process, often the change is less than a complete 180-degree transformation." Finally, the anthropologists Jean and John Comaroff (1991, 250), talking about the conversion of African peoples to Christianity, question whether the concept of conversion in "its commonsense European connotations" can grasp well "the highly variable, usually gradual, often implicit" transformations of social identities, cultural styles, and ritual practices.

11 Under the pressure of criticism, Kuhn felt, in his later writings, that he had to clarify and qualify his claims about conversion. He said that only individuals and not communities undergo Gestalt switches and using this term for communities was only metaphorical (2000a, 88).

12 This particular phrase alludes to St Paul's conversion which was sudden and transformative and was described in the same manner "and immediately there fell from his eyes as it had been scales" (Acts 9.18).

13 "It is emphatically not my view that 'adoption of a new scientific theory is an intuitive or mystical affair, a matter of psychological description rather than logical or methodological codification'" (Kuhn 1970b, 261). Kuhn is criticizing here Israel Scheffler's understanding of his work.

14 Cf. Kuhn (1970b, 249): "If there are revolutions, then there must be normal science."

15 According to Kuhn, anomalies would not appear only if theories were restricted to apply to phenomena that were already treated by the theory and presented no problems. But, that "would be the end of the research through which science may develop further" (Kuhn 1970a, 100). Kuhn says that the logical positivists tried to save theories in this way (e.g., by presenting Newtonian dynamics as a special case of Einsteinian dynamics, given certain restrictive conditions).

16 Kuhn (1970a, 97) says that revolutionary discoveries do not confront us "with mere historical accident." He also says that commitment to a paradigm is not only a prerequisite of normal science but also a prerequisite to surprises, anomalies, crises, and radical change (1970a, 100–101).

17 In his later work, Kuhn spoke more of speciation rather than of scientific revolutions comparing scientific development, with its proliferation of special disciplines, to biological evolution (Kuhn 2000b, 98; 2000c, 119). The process of speciation toward greater specialization, just like that of the scientific revolutions, is for Kuhn "inescapable, a consequence of first principles" (2000b, 98).

18 Kuhn himself raised the issue. After criticizing the logical positivists for restricting the range of applications of a theory to known phenomena so as to protect it from coming into conflict with any later theory, he says (1970a, 100–101):

> By now that point too is virtually a tautology. Without commitment to a paradigm there could be no normal science. [. . .] Besides, it is not only normal science that depends upon commitment to a paradigm. If existing theory binds the scientist only with respect to existing applications, then there can be no surprises, anomalies, or crises. But these are just the signposts that point the way to extraordinary science. If positivistic restrictions on the range of a theory's legitimate applicability are taken literally, the mechanism that tells the scientific community what problems may lead to fundamental change must cease to function.

19 Some philosophers have contrasted exemplars with rules in the Kuhnian scheme. For instance, Alexander Bird (2000, 71) writes: "It is with *rules* that Kuhn wants explicitly to contrast exemplars." Actually, Kuhn does not contrast exemplars with rules but speaks of the priority of paradigms over rules (1970a, 43–51). What he means is that scientists need to be acquainted with paradigms/exemplars first, in order to learn how to follow their lead, how to imitate them. Following any exemplar means following the rules that the exemplar sets, for example, how to model puzzles

on the exemplar and how to reach their solution (Kuhn 1970a, 189). These rules that emerge from following a paradigm and tell scientists what to do need not be explicit. The priority of paradigms/exemplars over rules that Kuhn speaks of is not only temporal but also logical. Exemplars make the specific rules that characterize a practice possible and warrant their application. Kuhn contrasts exemplars, not with rules *simpliciter*, but with those rules that are supposed to be able to dictate what is to be done independently of any concrete application. In his view, the mere expression of a rule in words "taken by itself, is virtually impotent" (Kuhn 1970a, 191). Learning to act according to rules requires prior exposure to concrete examples of use. Kuhn is opposed to the idea that a scientific methodology can be specified in the abstract, in terms of rules, and then given to scientists to follow. This approach has two faults: first, scientists would not know what to do should they be given only verbal statements of rules without concrete applications in practice, even if they understand the words the rules are expressed in. Second, a theoretical specification of rules would have to be generic, which means that all differences in application would have to be attributed to eliminable idiosyncrasies of the particular scientists. Kuhn wanted to say that diversity is an inherent characteristic of scientific practice given that scientific traditions are built around particular paradigms/exemplars instead of generic rules. For more on the relation between paradigms and rules and the influence that Wittgenstein's philosophy had on Kuhn in this respect, see Kindi (2012c).

20 I thank Moti Mizrahi for drawing my attention to these questions.

21 In Kindi (2005, 519–22) I argue that concentration on particulars, a characteristic mark of historical work, made Kuhn more sensitive to differences and helped him recognize the diversity of scientific practice.

22 "Object of comparison" is a Wittgensteinian term and although there are differences, it would be helpful to compare the role of Kuhn's model as I describe it to how Wittgenstein understands this concept. Wittgenstein set up simple language games, or used particular cases, real or invented, as models, to illuminate an issue and dissolve philosophical confusion. We should see a model, he said, "as what it is, as an object of comparison—as a sort of yardstick; not as a preconception to which reality *must* correspond. (The dogmatism into which we fall so easily in doing philosophy.)" (PI, section 131). What Wittgenstein meant was that we should not, as philosophers, be dogmatic and demand that reality conforms to the specifications set by the model of the philosopher as if this was the only appropriate way to look at things. We should use our models to present a way of conceiving things. Oskari Kuusela (2008, 125) explains: "an object of comparison is not used to make an empirical statement about any particular objects in the sense of being valid of only those objects, though perhaps inductively generalizable. Neither is the model used as a basis for a thesis that states that all objects falling under a concept *must* be." According to Kuusela, the necessity expressed by a model characterizes the model and not the objects of the investigation. In an earlier version of the PI section 131, Wittgenstein had, in parentheses, the sentence: "I am thinking of Spengler's mode of examination" (cited in Kuusela 2008, 126; cf. Wittgenstein 1998, 21). Oswald Spengler had proposed a certain organic model of cultural growth and decay in historical development and Wittgenstein criticized him for thinking that history must fit his model. As Northrop Frye (1974, 9)

put it, Spengler was "certain that history will do exactly what he says." If Kuhn is read as offering a developmental schema for the history of science, as Spengler did for cultures, then Wittgenstein's criticism of Spengler would, arguably, also apply to him. But, according to my reading, Kuhn was not doing that. His schema is similar to Wittgenstein's "objects of comparison." The difference with Wittgenstein is that Kuhn does not present his model as one of many. He has confidence that it captures crucial characteristics of the practice of science as we know it and, because of that, he thinks it is particularly effective in carrying out the task of transforming the dominant at the time image of science.

23 This thought was a major breakthrough for Kuhn in his effort to build his model. He wanted to account for the consensus among scientists but, being "enough of a historian," he knew that he could not attribute it to an agreement regarding specific definitions, rules, or axioms. "And that was the crucial point at which the idea of the paradigm as model entered. Once that was in place, and that was quite late in the year, the book sort of wrote itself" (Kuhn 2000d, 296).

24 Kuhn's approach as described by Hesse (1963) can be compared to Wittgenstein's method of assembling reminders (PI, section 127). Philosophers, according to Wittgenstein, do not need to "hunt out new facts," nor should they seek to learn anything new by their investigations (PI, section 89). The philosophical problems, Wittgenstein said, "are, of course, not empirical problems; but they are solved through an insight into the workings of our language [. . .] The problems are solved not by coming up with new discoveries, but by assembling what we have long been familiar with" (PI, section 109).

25 Cf. Kindi and Arabatzis (2012, 3) where we claim that *Structure*'s philosophical reception was shaped by the standards of a philosophy which was itself targeted by *Structure*.

26 Kuhn's emphasis on practice in relation to science has had extensive influence in the social studies of science. But, in this context, the practice of science is studied empirically with little concern for the philosophical implications of this idea.

27 Hempel (1980, 197): "How can [adherents of different paradigms] ever have lunch together and discuss each other's views?"

28 In Kindi (2012b), I discuss the classical view of concepts in opposition to the Wittgensteinian account, which, I think, has influenced Kuhn.

29 I have discussed issues that pertain to the relation between historical and, in general, empirical studies on the one hand, and philosophy on the other, in Kindi (2012a; 2014; 2016).

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