The Relation of History of Science to Philosophy of Science in *The Structure of Scientific Revolutions* and Kuhn's later philosophical work

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In this essay I argue that Kuhn's account of science, as it was articulated in The Structure of Scientific Revolutions, was mainly defended on philosophical rather than historical grounds. I thus lend support to Kuhn's later claim that his model can be derived from first principles. I propose a transcendental reading of his work and I suggest that Kuhn uses historical examples as anti-essentialist Wittgensteinian "reminders" that expose a variegated landscape in the development of science.

T. S. Kuhn was a physicist who became a historian, but who also wrote a book, the *Structure of Scientific Revolutions* (1970),[1] that proved seminal in philosophy of science. In that book, unlike what was typical in philosophy of science before the so-called historical turn, there are quite a few references to particular cases from the history of science. After the *Structure,* and all the praise and controversy that it evoked, Kuhn published *Black-Body Theory and Quantum Discontinuity* (1987), a book of history of science. The question that repeatedly arose was: what is the relation of Kuhn's philosophy of science to the history of science? He was asked, for example, whether the *Black-Body* book exhibited the pattern of development that was elaborated in the *Structure.* His commentators wanted to know whether the facts pertaining to quantum theory provided confirmation for his philosophical model, whether they could serve as a testing ground, but also, whether his account of these particular facts was shaped by his philosophical model. They wanted to know, that is, whether his philosophy was

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1. Subsequently referred to as *Structure* or *SSR.*

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dependent upon history but also whether in doing history he was influenced by his philosophy. Kuhn vehemently denied both:

> [. . .] I have myself resisted attempts to amalgamate history and philosophy of science though simultaneously urging increased interaction between the two. History done for the sake of philosophy is often scarcely history at all (Kuhn 1980, p. 183).

> I have said repeatedly, and I will say again: you cannot do history *trying* to document, or to explore, or to apply a point of view (Kuhn 2000d, pp. 313–314 emphasis in the original).

> 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, p. 314, emphasis in the original).

> I'm never a philosopher and a historian at the same time (Kuhn 2000d, p. 316).

Kuhn, with this response, guards himself against a number of objections that will be recounted later. Yet, independently of Kuhn's reaction, the question remains: what is the relation of history of science to philosophy of science in Kuhn's model? Kuhn made use of history; he even claimed that the two disciplines, history and philosophy, can, do and should interact and fertilize each other (Kuhn 2000d, pp. 315-316). If the relation between them is not that of theory to evidence, what is it? I'll confine myself to the exegetical point aiming at a coherent account that would comprise both what Kuhn does and what Kuhn says he does. I will try, in particular, to reconcile Kuhn's historical orientation in the *Structure* with his later claim that his model of science can be derived from first principles.[2] I will begin by illustrating the wide range of different opinions on the issue in the recent and not so recent secondary literature on Kuhn and I will then proceed to propose my own account.

I. How the relation between history and philosophy of science in the case of Kuhn's work is perceived in the recent literature.

In some of the recent literature one finds the following claims as regards the relation between history and philosophy of science in Kuhn's work:

1. Michael Friedman in his *Dynamics of Reason* repeatedly characterizes Kuhn's work as the "theory of the nature and character of scientific revolutions" (Friedman 2001, pp. 19, 41–44, 119). Friedman argues in favor of relativized constitutive *a priori* principles in the sciences and he states that

2. See Kuhn 2000b, p. 95; Kuhn 2000c.

history of science, paradigmatically illustrated in Kuhn's work, provides the confirmation needed. He explicitly calls Kuhn's contribution our "best current historiography of science" (Friedman 2001, pp. 43, 44, 51, 52), and he is not referring to Kuhn's purely historical research. He is alluding to Kuhn's non-cumulative model of science involving paradigm change. Although Friedman draws comparisons and highlights differences between Kuhn's work and the philosophies of Carnap or Quine, he clearly considers the former to be historiographical in nature.

2. Alexander Bird in his book *Thomas Kuhn* says: "Kuhn's *The Structure of Scientific Revolutions* is not primarily a philosophical text. Rather it is a work in what I call 'theoretical history'" (Bird 2000, p. viii). By "theoretical history" Bird understands an empirical investigation into the history of science, which, by being theoretical, requires, as he says, a deep engagement with philosophy (Bird 2000, p. 29). Bird does not explain how philosophy turns an empirical investigation into a theoretical project, but advances the view that Kuhn acts as a scientist.[3] He claims that Kuhn investigates the pattern of historical development and provides both a description and an explanation. Scientific change is the puzzle to be solved and after observing a regularity in nature, Kuhn presents a hypothesis to explain it (Bird 2000, pp. 67, 141). However, the description offered by Kuhn, which distinguishes between successive phases of normal and revolutionary science is, according to Bird, inaccurate and so, the explanation which involves the "theory of paradigms" is, if not redundant and mistaken, rather "inadequate to the facts" (Bird 2000, p. 49). Bird believes that Kuhn took the wrong turn[4] when he supposedly abandoned his empirical studies—which are considered to be not only historical but also psychological—in favor of a more explicitly Kantian, critical, standpoint that draws conclusions from first principles.

3. Steve Fuller in his *Thomas Kuhn: A Philosophical History for Our Times* (2000) advances a convoluted reading. On the one hand, he credits Kuhn

3. One may infer from Bird's suggestion that, even in the sciences, theoretical endeavors require the assistance of philosophy. However given Bird's description of Kuhn's project (i.e., as scientific in the inductivist, Baconian manner) one cannot see how philosophy intervenes. The only allusion to philosophy by Bird is when he credits Kuhn with unstated conservative philosophical presuppositions, empiricist, positivist and Cartesian, which, allegedly, did not allow him to break with the tradition.

4. It seems that the wrong turn is not solely the turn to philosophy but the turn away from a particular philosophy associated with externalist epistemology and externalist and causal theories of reference (Bird 2000, pp. 278–279). See also Bird 2002. Surprisingly, Sharrock and Read note in their book, *pace* Bird, "Kuhn's apparently deepening commitment to 'scientific naturalism' as his career progressed" (Sharrock and Read 2002, p. 223, n. 2, also pp. 202–203).

with a normative ideal, which qualifies him [Kuhn] for inclusion in what Fuller takes to be the Platonist cult.[5] On the other, he criticizes Kuhn for "marginalizing his prescriptivism" in order to maintain modern science's status quo. Actually, it is claimed that Kuhn may have entered a "Faustian bargain": According to Fuller, Kuhn saw that science in its present form, even if it fell short of Kuhn's own normative ideal, provided "a stable military-industrial infrastructure and virtually the only source of legitimate authority for an increasingly fragmented and volatile populace" (Fuller 2000, p. 74). Kuhn, then, in Fuller's view, became "strategically vague" as regards the status of his book, wavering between prescription and description. Thus, he managed to "ward off the drastic calls for the disestablishment of science" (ibid.). What is more, in order to preserve the authority of science, Kuhn, according to Fuller, restricted the historical basis of his model to examples drawn from the period between 1620–1920, so that his normative ideal is not challenged by different developments before and after this period (Fuller 2000, p. 73). This "hopscotching across the centuries" serves to give the impression that it is possible "to understand the scientific turn of mind, regardless of the time and place in which science is practiced" (Fuller 2000, p. 215).

Fuller's concern is mostly political (i.e., that Kuhn does not question science's present status quo) and does not at all address the philosophical problems that would arise had Kuhn based his model on historical facts, irrespective of whether they were few or many. He does not consider the *is/ought* divide, nor is he concerned with the problems of underdetermination and induction. His non-political objections focus on Kuhn being a counterfeiter of history, but also an inventor of mythical constructs, or on a more charitable tone, of Weberian ideal types, in order to sensitize us to salient features of the object of inquiry (Fuller 2000, p. 195). Fuller believes, for instance, that Kuhn developed the concept of normal science by superimposing different perspectives of science from different moments in history in order to construct a mythical image that he then treated as accurate.[6] So, in Fuller's view, Kuhn had undertaken a covert conservative political mission which he served by being unfair to the facts and by being vague as regards the status of his philosophy.

4. Finally, Wes Sharrock and Rupert Read maintain that Kuhn's objectives are overwhelmingly philosophical (Sharrock and Read 2002,

5. Besides Plato and Kuhn, the Platonist cult includes, according to Fuller, Auguste Comte, Leo Strauss, Max Planck and "probably all [. . .] incarnations [of positivism]" (Fuller 2000, p. 38).

6. It is here that Fuller brings in the discussion Panofski's iconography from the history of art (Fuller 2000, p. 61).

pp. 106–109). Commenting on the first section of the *SSR,* which seeks a role for history, they write:

> [A] main purpose of *The Structure of Scientific Revolutions* is to make a case as to how scientists *do in fact* come to replace one theory with another. This makes it sound as though *SSR* is one of his historical studies, but it is not that. How, then, is the historical stuff the "stalking horse" for the philosophical; how is the latter aspect dominant in *SSR? SSR* differs from Kuhn's properly historical studies for he is not here primarily concerned to detail what occurred in various specific episodes in the history of science, but, instead, to say how the events in such episodes should be philosophically construed (Sharrock and Read 2002, p. 27, emphasis in the original).

In this passage Sharrock and Read claim that Kuhn's interest in historical facts is philosophical. What they mean by that is that Kuhn needed a properly done history of science to enable him to understand scientific change "in a way that directly conflicted with the American philosophy of science Kuhn inherited and which he thought *basically mistaken*" (Sharrock and Read 2002, p. 6 emphasis in the original).[7] They take his aim to be the revision of a particular philosophical image of science, and they present Kuhn's project of reconstructing the history of science as "a pretext" (Sharrock and Read 2002, p. 10). It is not that in their view Kuhn was not really interested in history, only that his real purpose, apart from possibly misleading appearances, was philosophical. The historical objective was subjected to the philosophical. According to Sharrock and Read, Kuhn's historical studies clarify Kuhn's philosophical arguments and it is an "overreaction" to say that his model can be derived wholly from first principles (Sharrock and Read 2002, p. 199-200).

The two authors claim that they put forward a Wittgensteinian, deflationary reading of Kuhn. From their perspective, Kuhn, albeit not always consistently, aimed, on the one hand, to dissolve philosophy of science or even cure us from it (Sharrock and Read 2002, p. 211), and on the other, with respect to science, he aimed at leaving everything as it is (Sharrock and Read 2002, p. 209). So, the status of Kuhn's project is taken to be philosophical in a particular sense, i.e., in a quietist, Wittgensteinian sense. In their view, it is not philosophical in a robust normative sense because it does not meddle with scientific practice, it does not aim at issuing standards of validity and method.

7. "We have portrayed Kuhn as continuously concerned largely with one issue—spelling out the meaning of properly historical studies of episodes in the history of science for the philosophy of science" (Sharrock and Read 2002, p. 199).

Now, one could say that Kuhn's project need not be philosophical to leave science as it is. Even if it were merely historical, it would again not interfere with what scientists do. History of science, even when it is properly done, (i.e., the way Kuhn suggested), is not really relevant to science proper. So, Sharrock and Read need to advance an argument in defense of their claim that Kuhn's work is philosophical and not historical in character.

To that end, they observe, first, that Kuhn's account of science is "largely unevidenced" (Sharrock and Read 2002, p. 107), that we have to take his claims "on trust" (ibid.), that his historical examples offer "precious little" (Sharrock and Read 2002, p. 108). The import of the historical cases cited is "to exemplify and dramatize the progress of philosophical revolutions-but that is perhaps their only philosophical relevance" (Sharrock and Read 2002, p. 109). Then, Sharrock and Read ask whether any of the above reflect negatively on Kuhn. And they answer: "As a matter of fact: not at all. Rather, we take it as a strong indication that one of our central claims is true: that Kuhn is a *philosopher* of science" (ibid., emphasis in the original). They end the discussion by citing Kuhn's own declaration of his philosophical goals. And they comment: "What is striking is first the degree of self-identification as a philosopher [. . .]; and second the surprising and indeed trumpeted willingness to dispense with the density of actual historical examples in favor of the abstraction of 'first principles'. To say it again, Kuhn is philosopher above all (Sharrock and Read 2002, p. 110). So, what Sharrock and Read are virtually saying is that Kuhn's "essential identity" is philosophical because, first, he fails to provide evidence for his claims and second he declares it to be so. They seem to argue as follows: If Kuhn were making empirical generalizations, he would have provided evidence. He does not provide evidence, hence, he is a philosopher, since he also says so himself.

II. Early assessments of the relation of history of science to philosophy of science in Kuhn's model

From early on, the empirical basis of Kuhn's model of science was taken to be an issue of concern. Israel Scheffler maintained "there can be no appeal to ostensibly paradigm-neutral factual evidence from history in support of Kuhn's own new paradigm" (Scheffler 1972, p. 367).[8] If Kuhn were to claim that, he says, he would be guilty of self-refutation. Similarly, Shapere has noted that historical facts are not open to direct inspection and they can equally bear out opposing philosophical views concerning

8. See also Scheffler 1967, pp. 21–22, 53, 74.

scientific development (Shapere 1980, p. 31). He says that Kuhn got carried away by "the logic of his notion of paradigm," and that the relativism that ensues is not the result of empirical historical research but "the logical outgrowth of conceptual confusions" (Shapere 1980, pp. 37-38).

N.R. Hanson (1965) detects a "logical imperfection" in Kuhn's methodology. He says that Kuhn wavers between putting forward a genuine historical thesis, on the one hand, and an elaborate set of definitions, on the other. In the first case we have an informative and, yet, possibly false thesis and in the second, an unfalsifiable exposition of the meanings that the terms "paradigm" and "revolution" have or are given by Kuhn.

> This methodological issue would seem to affect our entire conception of the historiography of science. We are as much desirous in being illuminated about the *facts* as in being illuminated by Kuhn's decisions concerning how he will use certain expressions. We may become better historians from information of either kind; but it would nevertheless, be a help for Professor Kuhn to make unambiguously clear which of these two endeavors did inform his very important book (Hanson 1965, p. 375, emphasis in the original).

Hanson acknowledges Kuhn's contribution in the historiography of science and asks solely for the disambiguation of the use of Kuhn's expressions.

Paul Feyerabend having read a draft of *The Structure of Scientific Revolutions,* sent to Kuhn two letters that were subsequently published by Hoyningen-Huene (1995). In these letters Feyerabend criticized Kuhn for using a double talk: "every assertion may be read in two ways, as the report of a historical fact, and as a methodological rule. You thereby take your readers in" (Hoyningen-Huene 1995, p. 355). This is a "bewitching way of presentation" (ibid.), Feyerabend says, that covers up a "questionable monolithic ideology" (Hoyningen-Huene 1995, p. 367)[9]—that of the conservative character of normal science—in the form of history. He charges that Kuhn never states clearly that his model amounts to "an ideal" but insinuates instead that this is what historical research teaches him (Hoyningen-Huene 1995, p. 360).[10] But history, according to Feyerabend is irrelevant to methodology (Hoyningen-Huene 1995, p. 366). "Is" does not imply "ought" and Kuhn should refrain from putting forward mere beliefs as if they were indisputable and inescapable facts

9. See also Hoyningen-Huene 1995, p. 355.

10. Similar remarks are made in Feyerabend (1970). Kuhn is blamed of intentionally blending ambiguous descriptive and prescriptive claims (Feyerabend 1970, p. 199).

(Hoyningen-Huene 1995, pp. 355, 368). Historical facts, Feyerabend says, admit of alternative interpretations and Kuhn should not hide this from his readers (Hoyningen-Huene 1995, p. 355). He should not be afraid of their criticism and he should make his point of view explicit (Hoyningen-Huene 1995, pp. 366, 355).

Another, different, point of criticism, though, is that Kuhn is led by his "hidden predilection for monism (for one paradigm) to a false report of historical events" (Hoyningen-Huene 1995, p. 367, see also p. 381). So, according to Feyerabend, Kuhn not only hides his ideology in the covers of history, but he is also guilty of falsely reporting historical events.

Lastly, Janet Kourany develops a different line of criticism. In her view, Kuhn provides no historical justification for his model. The historical examples are scattered, sketchy, undocumented and sometimes they appear even to refute Kuhn's claims (Kourany 1979, pp. 50, 52). Statements like, Copernicus saw a star where Ptolemy had seen a planet, or Lavoisier saw oxygen where Priestly saw dephlogisticated air are used by Kuhn, according to Kourany, as "a little more than imaginative illustrations of his position rather than items of historical support for it" (Kourany 1979, p. 55). At least part of the justification offered by Kuhn in support of the claim that scientific development is not cumulative, Kourany says, "seems little better that an argument *a priori*" (Kourany 1979, p. 49).

III. Summation of the critical arguments and outline of the account offered by them

In summary, the positions advanced regarding the relation of history of science to philosophy of science in Kuhn's work, in both early and late secondary literature, can be charted out as follows:

1. It is claimed, on the one hand, that Kuhn's work is clearly historiographical (Friedman/Bird) or empirical in general (Bird). In that context it is often claimed that the historical account given by Kuhn is, partly at least, inaccurate (Bird, Fuller, Feyerabend).[11] Kourany, however, maintains that the historical research needed for the factual basis of Kuhn's model is completely absent (Kourany 1979, p. 56), while Sharrock and Read note that Kuhn's account is largely unevidenced.

2. Moving a little further up from the factual level, Bird credits Kuhn with "theoretical history," which can be taken either as a purely empirical inductive generalization inferred from inspected individual cases or in the

11. It should be noted here that although Feyerabend speaks of inaccuracies, he recognizes that facts admit of alternative interpretations.

sense, alluded to by Feyerabend, of a Hegelian-like philosophy of history (Hoyningen-Huene 1995, p. 353).[12]

3. A different line of criticism finds Kuhn's account wavering between description and prescription (Fuller, Feyerabend, Hanson). In particular, according to Fuller and Feyerabend, Kuhn disguises his ideology and propaganda in the covers of history.

4. Finally, a number of commentators (Sharrock and Read, Kourany, Shapere) recognize the *a priori* or philosophical status of Kuhn's model in order either to blame him for unfounded beliefs (Kourany, Shapere) or to credit him with particular philosophical agendas (Sharrock and Read).

In what follows I will give my own account of Kuhn's model, which differs considerably from the above. I will first show that in the *Structure* Kuhn lays out a philosophical project and does not derive his model from historical evidence. Then I will argue that this philosophical project, which draws upon, but is not based on, historical examples, is very much similar to the grammatical investigations undertaken by Ludwig Wittgenstein but also to Strawson's transcendental analysis.

I will not dwell on the critical points made above for a number of reasons. First, because I agree with those of Kuhn's commentators who claim that Kuhn is not always clear or explicit as regards the descriptive or normative status of his work. Secondly, because I believe that the charges of an alleged political agenda on the part of Kuhn or of the intentional bewitchment of Kuhn's readership are highly speculative. Lastly, because I want to give, if possible, a charitable account of Kuhn's historically oriented philosophy of science. Kuhn has been repeatedly described in the literature as "philosopher manqué" (Bird 2002, p. 459), as "[not knowing or understanding] the philosophical heritage he was working in and against" (Bird 2002, p. 460), as "in a state of blissful but perhaps forgivable innocence" (Friedman 2001, p. 19). Yet, I do not want to base my assessment of Kuhn's work on grounds that pertain to his biography. I will not right away dismiss what he says by attributing to him ignorance or elementary logical mistakes.[13] If Kuhn were to base his philosophy on history, for instance, he would have to address the *is/ought* divide, the problems of underdetermination and self-refutation but also his very limited

12. Feyerabend claims that Kuhn, just like Hegel, takes history to be a judge, the difference being that Kuhn refers to the past while Hegel to the future. Strangely, Feyerabend puts Wittgenstein together with Hegel.

13. Kuhn remarks: "[P]eople treated me as though I were a fool!" (Kuhn 2000b, p. 315).

empirical base. If, on the other hand, Kuhn were to do theoretical history in the Hegelian manner, he would have to respond to the typical criticism raised against such approaches, namely, unfettered speculation and teleology.

Of course, Kuhn's limited formal philosophical training gives one reason to suppose that logical mistakes and problematic ramifications of some of the views he puts forward may have passed undetected; yet I want to check whether the text itself can sustain a coherent account that also agrees with Kuhn's own explicit remarks on the issue under consideration.

IV. Kuhn's philosophical arguments for the non-cumulative growth of science

In chapter 9 of the *SSR* Kuhn discusses a key contention of the book, namely, the view that science progresses non-cumulatively. He asks why a change of paradigm should be called a revolution, why the emergence of a new paradigm works destructively for the old. He starts with a simile. Just like political revolutions, he says, scientific revolutions require, or presuppose, a period of crisis. People do not just import revolutions. They first have to experience dissatisfaction with the institutions they have, in order to proceed to change them. In a similar manner, in science, the new paradigm emerges only after normal scientific research falls short of the scientists' expectations. Nowhere does Kuhn say that historical research showed him, as a matter of fact, that crisis always precedes revolutions. Reference to historical examples in this respect is made for reasons of illustration.

Then Kuhn proceeds to give arguments why the assimilation of a new sort of phenomenon, or of a new scientific theory requires the rejection of the old paradigm. First, he explains that it is logically possible to add new phenomena or a new theory to an old paradigm. This may happen if the paradigm expands in a new domain or when the developed theory integrates on a higher level previously held beliefs. So, if logic does not prescribe the rejection of the old paradigm what does? Kuhn claims, "there is increasing reason to wonder whether it [the ideal image of science-as-cumulative] can possibly be an image of *science*" (Kuhn 1970, p. 96). What kind of reason is that? Initially, at least, the reason seems to originate in history.

> [T]he assimilation of all new theories and of almost all new sorts of phenomena has *in fact* demanded the destruction of a previous paradigm and a consequent conflict between competing schools of scientific thought. Cumulative acquisition of novelties proves to be an almost non-existent exception to the rule of scientific develop-

> ment. The man who takes historic fact seriously must suspect that science does not tend toward the ideal that our image of its cumulativeness has suggested (Kuhn 1970, p. 96, emphasis added).

After, however, this gesture towards history, Kuhn puts forward another line of argument: "[A] second look at the ground we have already covered may suggest that cumulative acquisition of novelty is *not only rare in fact* but *improbable in principle*" (ibid., emphasis added). Let's see what his arguments are now: The first says that unanticipated novelty emerges only after an anomaly is detected. Anomaly is simply deviation from the normalcy laid out by the old paradigm. When a new paradigm turns an anomaly into a normal, lawful phenomenon it cannot be compatible with the old. And though Kuhn claims that logical inclusiveness, however permissible, is a historical implausibility, he insists "the examples of discovery through paradigm destruction [. . .] did not confront us with mere historical accident. There is no other effective way in which discoveries might be generated" (Kuhn 1970, p. 97).

The second argument is again philosophical. If we accept, Kuhn says, the prevalent conception of scientific theories, as it was formed by the logical positivists and their successors, then no theory can ever be challenged. Adequately interpreted, all theories can be preserved as special cases of subsequent ones and all grievances against them can be attributed to the extravagant and ambitious claims made by the fallible human beings. If that is the case, then science would stop.

> But to save theories in this way, their range of application must be restricted to those phenomena and that precision of observation with which the experimental evidence in hand already deals. Carried just a step further (and the step can scarcely be avoided once the first is taken), such a limitation prohibits the scientist from claiming to speak "scientifically" about any phenomenon not already observed. [. . .] But the result of accepting them would be the end of the research through which science may develop further (Kuhn 1970, p. 100).

Kuhn says that if we accept the standard image of cumulative growth in science (an image that has no room for the challenge and rejection of theories), then we end up with an absurdity. Not a logical absurdity, but an absurdity nevertheless, if we consider how science is practiced. Kuhn characterizes this point a tautology (ibid.). Without unrestricted commitment to a paradigm there could be no normal science. Without normal science there could be no surprises, anomalies, crises. Without crises there could be no extraordinary science. Without all these, there is no 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. And when that occurs, the community will inevitably return to something much like its pre-paradigm state, a condition in which all members practice science but in which their gross product scarcely resembles science at all (Kuhn 1970, p. 101).

There is one final argument against cumulative growth in science, again philosophical. It is the argument about meaning change, which is supposed to show that one cannot derive Newtonian from Relativistic dynamics as the positivists surmised.

So, in this crucial part of Kuhn's account of science, reliance on history is only supplementary. The reasons he gives for the non-cumulative development of science are mainly philosophical and not factual. Robert Westman (1994, p. 82), who revisits Kuhn's *The Copernican Revolution,* reinforces that point. He writes that "[w]hen Kuhn argues [in the *SSR*] for the radical, transformative character of 'seeing' that occurs in the aftermath of a revolution, he produces a fictive speech delivered at an undetermined moment by a 'convert,' rather than the specific utterance of a historical agent." Westman's observation that Kuhn "produces fictive speech" instead of concrete historical evidence lends, I think, further support to the claim that Kuhn did not use history to ground his philosophical claims.

I agree, then, with commentators like Sharrock, Read and Kourany who recognize the philosophical aspect of Kuhn's work. But I disagree that this follows from the description of his account as "unevidenced." Sharrock and Read argue invalidly that Kuhn is a philosopher because he does not provide evidence for his claims whereas Kourany speaks reproachfully of *a priori* arguments after she showed the thin and questionable historical basis of Kuhn's model. Sharrock and Read go further and credit Kuhn with a therapeutic philosophy, which, much like Wittgenstein's, is supposed to cure philosophical misconceptions, but they do not elaborate on how history features in this context. Their suggestion, that historical cases simply exemplify or dramatize philosophical abstractions, casts on Kuhn a very traditional philosophical light (any philosophy of science can use historical examples), does not account for the historical bend of Kuhn's approach and leaves unexplained where his so-called philosophical abstractions are derived from. Moreover, their claim cannot account for the fact that Kuhn insists that he does not write history for philosophical purposes.

My view is that Kuhn is engaged from the start in a philosophical enterprise because his target is philosophical. He aims at bringing down an ideal image of science that may have been drafted by the philosophers, but held nearly everybody captive. This ideal image is attacked with philosophical arguments, as it was shown above, but also with the help of the history of science, as it will be shown below. The question, of course, is how exactly Kuhn viewed and used history and how important it was, given that later in his work he contends that "many of the most central conclusions we drew from the historical record can be derived instead from first principles" (Kuhn 2000c, p. 112).

V. Kuhn's later account of the relation between history and philosophy of science

The sentence cited immediately above is taken from a longer passage in which Kuhn describes clearly how he views his project retrospectively:

> [M]y generation of philosopher/historians saw ourselves as building a philosophy on observations of actual scientific behavior. Looking back now, I think that that image of what we were up to is misleading. Given what I shall call the historical perspective, one can reach many of the central conclusions we drew with scarcely a glance at the historical record itself. The historical perspective was, of course, initially foreign to all of us. The questions which led us to examine the historical record were products of a philosophical tradition that took science as a body of knowledge and asked what rational warrant there was for taking one or another of its component beliefs to be true. Only gradually, as a by-product of our study of historical "facts," did we learn to replace that static image with a dynamic one, an image that made science an ever-developing enterprise or practice. And it is taking longer still to realize that, with that perspective achieved, many of the most central conclusions we drew from the historical record can be derived instead from first principles. Approaching them in that way reduces their apparent contingency, making harder to dismiss as a product of muckracking investigation by those hostile to science (Kuhn 2000c, pp. 111–112).

In this passage Kuhn maintains that he and other philosophers/historians of his generation were, initially, under the impression that they were advancing a new philosophy of science based on the observation of actual scientific practice, past and present. Old philosophers, like the logical positivists, were dealing with an ideal image of science, completely detached from the experience of those who were involved in the scientific enterprise, whereas Kuhn and his contemporaries thought that they ushered in the actual life of science.[14] Later, Kuhn proceeds to state that the study of historical facts wasn't at all necessary. It may have helped genetically, but logically it wasn't needed at all. They could reach the same conclusions if they just considered science from a historical perspective, which requires us to look at things as developing over time. Given this perspective, the static image of science would be immediately replaced by a dynamic one. All the rest, i.e., the crucial parts of the model, would then follow: There would be no fixed Archimedean platform to judge the rationality of individual beliefs, but a moving, historically situated, reasoned, comparative evaluation of change of belief; no resort to facts that are prior to the beliefs they are supposed to supply evidence for and no convergence to an ultimate truth.

Kuhn's contention is that the consideration of any developmental process would yield these characteristics. They are not peculiar to science, but necessary features of any evolutionary practice (see Kuhn 2000c, pp. 116, 119). The only thing required in order to find them is to "approach science as a historian must," i.e., by "pick[ing] up a process already under way" (Kuhn 2000b, p. 95). In *that* sense, Kuhn says he derives his conclusions from first principles: "I've reached that position [i.e., that facts are not prior to conclusions drawn from them] from principles that must govern all developmental processes, without, that is, needing to call upon actual examples of scientific behavior" (Kuhn 2000c, p. 115). It is also from first principles that Kuhn derives "speciation" (the branching out of distinct scientific specialties after some revolutionary change) and incommensurability, both of which he sees as the necessary prerequisites for the advancement of knowledge and the authority of science.

> With much reluctance I have increasingly come to feel that this process of specialization, with its consequent limitation on communication and community, is inescapable, a consequence of first principles. Specialization and the narrowing of the range of expertise now look to me like the necessary price of increasingly powerful cognitive tools (Kuhn 2000b, p. 98).

Lexical diversity and the principled limit it imposes on communication may be the isolating mechanism required for the development of knowledge (Kuhn 2000b, pp. 98–99).

14. In (Kuhn 2000b. p. 95) Kuhn says: "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 (an evolutionary epistemology need not be a naturalized one)."

> I am increasingly persuaded that the limited range of possible partners for fruitful intercourse is the essential precondition for what is known as progress in both biological development and the development of knowledge. [. . .] [I]ncommensurability properly understood could reveal the source of the cognitive bite and authority of the sciences (Kuhn 2000b, p. 99).

In a later article, however, Kuhn allows for some, albeit minimal, contribution of historical observation regarding speciation. He says that, unlike the thesis that facts are not prior to the conclusions drawn from them, speciation "is not a necessary or an a priori characteristic [of a historical perspective], but must be suggested by observations. The observations, involved [. . .] require, in any case, no more than a glance" (Kuhn 2000c, p. 116).

Since recourse to history is limited to "no more than a glance," I do not think that there is significant change of position between the two articles. Kuhn needs history only to provide him with the historical perspective. Once this perspective is *suggested,* all the rest follow: "What has for me emerged as essential is not so much the details of historical cases as the perspective or the ideology that attention to historical cases brings with it" (Kuhn 2000b, p. 95).

VI. Assessment of Kuhn's later account

Kuhn clearly lays emphasis on first principles to avoid the problems he would face had he given prominence to the empirical aspect of his work. This move puts him "safely" on the philosophers' side. He avoids contingency and all the criticism mentioned above (underdetermination, self-refutation, limited empirical basis). He also manages to preserve important elements of the philosophers' standard image of science by emphasizing the dynamic appraisal of *change of belief* rather than belief *tout court:* some continuity across revolutions, communication among scientists, reasoned evaluation of incremental change of belief based on the ever-present values of science (Kuhn 2000c pp. 112-119).

The cost of this double move is that exclusive reliance on *a priori* principles (with history entering only by a glance), not only raises new concerns, mainly concern over the justification of such an account, but it does not explain the difference between science and other developmental processes. What is more, the alignment of Kuhn's model with that of the traditional philosophers deprives it of its notorious radical pronouncements (discontinuity in the development of science, incommensurability, conversion experience, etc). If the change of belief is incremental (even if adjustments are required), if the rationality of the comparative evaluation is accounted for by invoking typical standards (shared neutral observations, common, even if equivocal, values), then the development of science over time seems more continuous than the *SSR* had us believe. Kuhn, of course, anticipating objections like this one, does not fail to observe that emphasis on the appraisal of *change of belief,* rather than appraisal of belief tout court, has radical philosophical repercussions, namely, that the Archimedean platform with higher criteria of rationality "is gone beyond recall" (Kuhn 2000c, p. 115), that the evaluation of beliefs is not against an independent world but only comparative, that there is no ultimate truth to be reached, no unique method of science. I do not want to underestimate the revolutionary character of these suggestions. Yet, I believe that Kuhn, under the fierce and relentless criticism that he received from the philosophers, was too eager to play down some of his most radical ideas in order to accommodate in his model observations that seemed to his critics preposterous to have been overlooked and omitted. It was repeatedly pointed out to him, for instance, that scientists do communicate, even in periods of crises, that their communication is reasoned, that established scientific theories are not completely overthrown and replaced. Obviously, Kuhn was well aware of such facts and did not want to be seen as disregarding or going against them. So, by shifting emphasis on the appraisal of *change of belief,* he modified his model to accommodate them. In that shape the radical philosophical implications of his work are integrated in a more plausible account and can become more easily acceptable. The problem, however, is that, as I said before, Kuhn, in this retrospective reappraisal, does not really make room for history in his model and also takes away some of the bite that it had. What is more, he conflates two levels of discussion.

On the historical, empirical level it is indeed true, as many of Kuhn's critics have pointed out, that there are several indications of continuity or reasoned debate across paradigmatic change.[15] It is often the same individuals that change allegiance and, certainly, one cannot say that these individuals do not understand their previous self (as the incommensurability thesis has been taken to imply). As Kuhn himself admits "communication

15. See, for instance, what Abner Shimony says in Klein, Shimony and Pinch (1979, p. 436): "On the whole, the intellectual processes of the few physicists immersed in black-body research seems to me to have been wonderfully rational." Toulmin (1972, pp. 103–105) makes a similar point in relation to both the Copernican revolution and the transition from Newtonian to Einsteinian physics. Daniel Garber (2001), on the other hand, while insisting that adherents of competing paradigms remain intelligible to each other, claims that rational argumentation breaks down when transition is under way. But, in disagreement with Kuhn, he does not attribute this to incommensurability and conceptual gaps between the rival paradigms but to more general cultural factors.

goes on, however imperfectly, metaphor serving as a partial bridge across the divide between an old literal usage and a new one. To speak, as I repeatedly have, of a community's undergoing a gestalt switch is to compress an extended process of change into an instant leaving no room for the microprocesses by which the change is achieved" (Kuhn 2000a, p. 88). Also, many beliefs stay intact and are not revised after a revolution. In fact, the closer one studies the historical circumstances of a period, the smaller the changes will seem, the more continuity will be found. Even conceptually, the very notion of change requires something that remains unchangeable, be that the scientists themselves, the scientific community, the scientific practice, the scientific enterprise at large. If there is no underlying identity, one would not speak of change, but of substitution of one alien entity by another. In that case we would not even speak of the history of some entity, e.g., of a discipline. However radical the change after a revolution, be it political or scientific, there need to be found some continuity, something that remains stable, in order to speak of change and revolution in the first place.

On the philosophical level, however, where the philosophers work at some distance from what empirically goes on, the issue of continuity in scientific development is raised differently. The continuity of which traditional philosophers spoke, and traditional historians of science recorded, was not an empirical finding.[16] It was a consequent of the ideal image of science, which presupposed a common atemporal method for the sciences in all times and places. It was also a demand of the particular theory of meaning incorporated in the ideal image. This image required that meaning seeps through from the level of experience up to the more complex theoretical abstractions. If, now, the only root of meaning is observation captured in intersubjectively avowed protocol sentences and carried forward by correspondence rules, then continuity in science is warranted by sameness of meaning. If any theoretical construct is latched on to the world by neutral observation statements in order to acquire meaning, then there is always a common core, whatever the change. Continuity in the minds of the philosophers concerned propositions and words and had nothing to do with shared elements of the actual scientific practice.[17]

16. Historians of science who took their cue from the philosophers did not *discover* continuity because they observed the facts from close proximity; rather the opposite was the case. Their research and its results were shaped by the philosophical tradition (see Kuhn 2000c, p. 111).

17. It shouldn't be forgotten that the term "science" for the philosophers who moulded the "received view" signified solely scientific theories, which were taken to be systems of propositions. The so-called "external factors" of science were not supposed to be of interest to philosophy.

When, now, Kuhn's critics and Kuhn himself indicate and acknowledge historical and sociological evidence of continuity drawn from the actual practice of scientists, they concede to the pressure still exercised by the traditional ideal image of science. They are trying to preserve or salvage some, non-Archimedean, but nonetheless shared, *terra firma.* In my view, though, the facts that are cited and the points made by both sides are not directly relevant to the philosophical issue addressed. Kuhn's target was the philosophers' image of science, which rested on a particular theory of meaning. Once this theory is challenged, the radical implications of the critique (discontinuity, incommensurability) follow no matter how much evidence of continuity the historians and sociologists accumulate.

I am not saying that the philosophers' account ought to be independent of or without regard to the facts. I am saying that the supposition of continuity and of a common scientific method was not derived from facts but was rather a postulate, a philosophical requirement imposed upon them. When historical research was freed from the streamlining that this monistic methodology imposed, it illustrated how diverse the practices of scientists have been. It has then provided philosophers like Kuhn with a reason to challenge the ubiquitous presence and validity of the standard image. It is not that Kuhn inferred from facts incommensurability and radical breaks. Rather, his historical research helped him see the philosophical character and presuppositions of the previous historical works.

Kuhn's claims about discontinuity are not theoretical constructions based on empirical evidence, but follow from seeing the scientific enterprise differently from the received view, i.e., as a developmental process and not as an axiomatically arranged set of sentences. When his critics cite all the evidence of continuity and communication, they fight, I believe, a straw man. As stated before, Kuhn could not have been unaware of the mundane facts of reasoned exchange between scientists advancing and supporting different theories and hypotheses. When he talked of incommensurability and resort to persuasion, when he used the metaphors of conversion and living in different worlds, he could not have implied that if historians looked at revolutionary periods they would find scientists indulging in typically "irrational" behavior like, for instance, preaching or engaging in idiosyncratic and irrelevant monologues; scientists would still be seen addressing the scientific community in the typical manner, i.e., by presenting arguments and articulating explanations. Nor would have he implied that the evidence of difficulties in communication would necessarily take the form of explicit remarks stating mutual incomprehension and unintelligibility. His point rather, I take it, was that historians and philosophers, taking for granted the standard image of science, tend to overlook, or do not attentively look for nuanced changes in use and meaning of terms that may indicate that divergent directions of development are under way.[18]

On the mundane, non-philosophical level then, scientists engage in the civil and reasoned behavior we normally associate with them. And they do communicate because they share the wider practice of science, which provides the conditions for making sense of each other. On the philosophical level, however, where the specific articulation of how terms mean and how concepts function matters, Kuhn's contention is that, once the standard image is challenged, there are no more common meanings secured by common observations. Then, continuity and communication are threatened. And once the changes become deep, lack of communication follows, if one remains within the philosophical perspective. Kuhn's pronouncements that pertain to incommensurability are provocative and radical only against the background of the received view. They do not question the rationality of ordinary affairs.

Admittedly, Kuhn is not always clear or consistent as to what exactly he is doing, whether, that is, he is advancing empirical or philosophical observations. But I am offering an interpretation that I believe explains how Kuhn's claims are radical on the one hand (on the philosophical level) and yet close to the scientists' experience on the other. Kuhn never wanted to be seen as advocating theses that would distance him from the scientific community and the view that science is a rational enterprise.

VI. In what sense is Kuhn's model necessary and a priori?

In the previous section I have presented Kuhn's later view that he can derive his model from first principles, with history entering only by a glance. I also contested the relevance of offering empirical evidence to counter Kuhn's claims about science and I argued that his project is philosophical. But I have not, still, explained what kind of a philosophical project that is. I believe that such an explanation is needed if Kuhn's account, especially the later one, is not to be dismissed as speculative armchair philosophy. It needs to be shown how Kuhn's model is not completely arbitrary and how, despite its derivation from first principles, it leaves some room for history.

I contend that his project is a transcendental one, offering the conditions of possibility of science. He maintains that science as a practice de-

18. "I don't think that the people who were doing history, by and large, saw everything in it, that I was seeing in it. They were not coming back asking 'What does this do to the notion of truth, what does it do to the notion of progress,' or if they did, they were finding it too easy to find answers that seemed to me superficial" (Kuhn 2000d, pp. 311–12).

pends logically on following rules, which in the case of science are set by concrete exemplars of scientific achievement. This practice has a dogmatic character and establishes the normalcy and normativity required. The rest of Kuhn's model is shaped accordingly. Science develops by solving the puzzles provided by the exemplars and paradigms. Emerging anomalies (i.e., deviations from normalcy), if they are to be made "lawful," may force a change of rules, in which case, crises and revolutions follow. If, now, meaning is given by the rules of normal science, then, when revolutions occur, there is change of meaning, which may be quite radical yielding incommensurable results. Revolution is not inferred from historical observation, but introduced as a concept to account for radical change.[19]

Admittedly, transcendental arguments, mainly because of Kant, have been associated with concepts more fundamental than science-like experience or knowledge- and they have been taken to serve a particular anti-skeptical purpose. They are construed as showing that skeptical doubts themselves, or a claim a skeptic would not challenge (for instance, that we have experience), presuppose the truths about the world that the skeptic takes to be questionable. I do not claim that Kuhn is engaged in a similar project. He is not mounting an anti-skeptical rebuttal. When I say that his analysis is transcendental I am trying to appropriate two features distinctive of transcendental arguments, which are also of interest to Kuhn: necessity and a priority.

Transcendental arguments bring out necessary connections between concepts. Typically, they have the form: "one thing (X) is a necessary condition for the possibility of something else (Y), so that (it is said) the latter cannot obtain without the former" (Stern 1999, p. 3). Stroud has argued that such a description "would make all valid deductions transcendental" (Stroud 1999, p. 158) and suggested that what is peculiar about transcendental arguments is both their beginnings and their end. They start from psychological facts (that we think and experience in certain ways) and they aim to prove the necessity of some non-psychological facts (how things in the world must be).[20] The latter are shown to be the conditions of possibility of the former. Stroud (1968) has questioned, however, whether this

19. That is why it is immaterial to wonder, as some critics do, how many revolutions there are in the history of science. Kuhn did not count paradigms or revolutions, he did not say how grand or how sweeping they might be.

20. Quassim Cassam (1999) spoke also of "self-directed transcendental arguments" as opposed to the so-called "world directed." The "world directed" argue from how thought is to how the world must be. The "self-directed," which were employed by Kant in the *Prolegomena to Any Future Metaphysics* in the context of his "analytic" method, do not address the skeptic and argue from certain cognitive achievements to how the cognitive faculties of the knowing subject are.

project can ever succeed without the baggage of idealism and in his later writings (Stroud 1999) sided with Strawson, who accepting Stroud's criticism, construed transcendental arguments as tracing connections *within* our conceptual scheme and not across the bridge from how we think to how the world must be.[21]

Kuhn can be seen as undertaking a similar analysis. Although he does not address the skeptic, although he does not start from indispensable psychological facts, we can say, by stretching the use of transcendental method, that Kuhn is interested in investigating the connections between the concepts that comprise the phenomenon we call science. These connections are not to be construed as simply causal but as logical and in that sense necessary and a priori.[22] His whole model can then be taken as an articulation of these kinds of connections.

But is Kuhn's model just a set of interdefined terms and the connections between them merely analytic necessities? This possibility cannot be ruled out even in the case of the Kantian arguments.[23] Nor can it be ruled out in the case of Kuhn despite the fact that it does not seem contradictory to say, for instance, that science does not rely on dogma. But even if the necessities that Kuhn's model indicates are analytic, it can be maintained that there is still something to be gained by employing the transcendental strategy. As Stroud observes (2000, p. 233), even in case the necessities are

21. Strawson (1985, p.ﬂ23), embracing the perspective of the naturalist philosopher, opts for a weakened version of transcendental arguments in the context of his "descriptive metaphysics." He claims that transcendental arguments could be used to investigate the connections between "the major structural elements of our conceptual scheme-to exhibit it, not as a rigidly deductive system, but as a coherent whole whose parts are mutually supportive and mutually dependent, interlocking in an intelligible way."

22. Kuhn does not seem to distinguish between "necessary" (a metaphysical concept) and "a priori" (an epistemological concept). He does not expand on them, but it seems that he is following Kant in assuming that "if we have a proposition which in being thought is thought as necessary, it is an a priori judgment" (Kant 1933, B3). Kripke thought the two terms are not coextensive and claimed that there exist *necessary a posteriori* and *contingent a priori* truths (1980, p. 38).

23. Strawson, according to Stroud, advanced in *The Bounds of Sense* a particular interpretation of the Kantian transcendental strategy in order to avoid the threat "that what Kant establishes are at most 'analytic' or 'definitional' necessities" (Stroud 2000, p. 234). Strawson maintained that the necessities sought by Kant were not those between concepts or meanings but between conceptual and experiential capacities (Stroud 2000, p. 235). But subsequently Stroud notes "it is just possible, I suppose, to see even that enterprise as yielding at best only 'analytic' or 'definitional' necessities. They would express what is 'covertly contained', not in the concept of, say, 'experience' or the concept of 'subject of experiences', but in the concept 'possesses the concept of experience', or the concept 'possesses the concept of a subject of experience'. 'Analysis' might reveal that the quite different concept 'thinks of the world as containing objective particulars' is contained in one or both of these concepts" (Stroud 2000, p. 239).

construed as analytic, it does not follow that the connections can be immediately seen by simply "gazing inside" the concepts. It is an operation much more complex and rewarding than that.

Still, I would say that Kuhn aimed for different necessities, not analytic but synthetic and without the Kantian idealist baggage. By giving the conditions of the possibility of science Kuhn was not trafficking simply among concepts nor was he engaged in linguistic analysis. He was saying something about the world, something that he had picked up from his experience as a scientist and a historian, namely, that we cannot have science as we know it unless we have dogmatic training, paradigms and rules. Science would not be possible if any of these things did not obtain. Now, if this is how we construe the Kuhnian project, we preserve some, at least, of its transcendental character, but then, Kuhn has to face the early Stroudian challenge to transcendental arguments, i.e., that their validity hangs on verification. So, if the necessities are intended as synthetic a priori, the proof is in verification, if the necessities are analytic, one is confined within a particular conceptual scheme.[24]

The first option, that of construing the Kuhnian necessities regarding science as synthetic a priori, would bring back the problems related to empirical confirmation that Kuhn was moving away from. As for the other horn of the dilemma, I believe that Kuhn would have welcomed the historization of what science presupposes and requires.[25] As he had wel-

24. Strawson (1985, p. 26) talking about the Kantian project feared that if the transcendental investigation is confined within a particular conceptual scheme, it would be historicized and, consequently, weakened and relativized. It would mean that metaphysics would become an essentially historical study, much in the spirit of Collingwood who sought the "absolute presuppositions" of each historical epoch. He also stressed that "if we stick to the actual behavior of words, then what we will discover will not be sufficiently general, or sufficiently far reaching to satisfy our urge for full metaphysical understanding" (Strawson 1970, p. 319). Stroud also observed that "the 'historical' conception of metaphysics could endorse what Kant would call the 'analytic' character of the necessities it discovers" (Stroud 2000, p. 233).

25. Fuller is of a different view. He claims (2000, pp. 73, 195, 215) that Kuhn has offered a description of a mythical image of science, which he created by disregarding the great differences in scientific practice in the course of history. Kuhn did that, according to Fuller, in his effort to carry out a Platonic mission, which called for the legitimation of the contemporary scientific-industrial-military status quo. Fuller thinks that a transcendental strategy lends support to an ahistorical understanding of science: "the transcendental argument [. . .] attempts to convert an impoverished imagination—specifically, our inability to envisage what the world would be like if progress and rationality turned out to be complete myths—into a guarantee that our faith in these myths is well-placed" (Fuller 2000, p. 29). Strawson had already made the point: "The transcendental arguer is always exposed to the charge that even if *he* cannot conceive of alternative ways in which conditions of the possibility of a certain kind of experience or exercise of conceptual capacity might be fulfilled, this inability may simply be due to lack of imagination on his part—a lack which makes him prone to mistake sufficient for necessary conditions" (Strawson 1985, p. 23).

comed the historization of scientific knowledge, claiming that he is a Kantian with moveable categories (Kuhn 2000d, p. 264, also Kuhn 2000b, p. 104), he would not, I think, object to the possibility of imagining a differently shaped practice of science. And if he is credited with such a view, his model would give us, then, the historicized conditions of the possibility of science. But, then again, the problem that his model carries only analytic necessities re-emerges. The worry is that the sentences stating these connections would be true solely in virtue of the meanings of the terms involved, which would imply that they are simply a matter of stipulation.

This fear, however, can be alleviated. It can be maintained that analytic necessities are not detached completely from the world. Quassim Cassam (2000, p. 60) in a paper where he reconsiders the distinction between empiricism and rationalism as regards a priori knowledge, refers to Quine, Boghossian and Peacocke, and defends the view that "no sentence is true but reality makes it so. [. . .] It makes no sense to suppose that linguistic meaning alone can generate truth." Even analytically necessary propositions, Cassam says, have factual content and they are true by virtue of how the world is.[26] Then, following a reading of Locke's *Essay* by James Tully,[27] Cassam proceeds to distinguish between *natural* and *conventional reality* in order to explain how analytic necessities can be both conventional and "about the world." They respond to reality, but a reality that has been set up. They are answerable to social facts (Cassam 2000, p. 59).[28] This does not make them merely arbitrary. In a similar vein, Wittgenstein has this to say regarding the "arbitrariness" of our color system, a system that precludes as a matter of conceptual truth the existence of a color intermediate between red and green:

> We have a color system as we have a number system.
>
> Do the systems reside in *our* nature or in the nature of things? How are we to put it?—*Not* in the nature of numbers or colors (Z 357).

26. According to Cassam (2000, p. 60) who cites Boghossian, the difference with synthetic statements lies in that, in the case of analytic statements, grasping the meanings of the terms involved, suffices for justified belief in their truth.

27. According to Tully's (1980) reading of Locke, our ideas of substances are inadequate because they do not represent natural substances accurately whereas our ideas of modes are adequate since they (our ideas of modes) do not copy but define their objects (Cassam 2000, pp. 58–9). It follows that the necessity of analytic propositions involving adequate ideas of modes becomes conventional.

28. John Searle (1995) distinguishes between institutional or social facts on the one hand and "brute" facts on the other and argues that the former are no less real than the latter.

> Then is there something arbitrary about this system? Yes and no. It is akin both to what is arbitrary and to what is non-arbitrary (Z 358).

According to Wittgenstein, our systems, with their conceptual and logical truths, may owe a lot to what we contribute but they are not whimsical constructions of the mind.[29] They are constituted and constrained by a network of practices and by the concrete and very real implications that these practices have. Cassam commenting also on the necessity of the proposition "Nothing can be red and green all over at the same time," notes that "there does not seem to be a straightforward answer to the question whether it is nature or convention to which [this proposition] owes its necessary truth" (Cassam 2000, p. 59). He then approvingly quotes D. F. Pears: "perhaps the emphasis on either side is a mistake; perhaps the culprit is neither convention alone nor nature alone" (ibid.).

The point of the above digression into Wittgenstein and Cassam is to indicate that even if the necessities one deals with are analytic, it can be maintained that they are accountable to reality and not merely arbitrary. Returning to Kuhn I would claim that he undertakes a transcendental analysis of science and establishes a priori necessities, which, even if regarded as analytic, are not devoid of factual content and are not capriciously stipulated. An obvious objection, however, to this line of thought would be the following: if the only necessities Kuhn establishes are analytic, irrespectively of how they are interpreted, why not call what he does conceptual rather than transcendental analysis? I prefer transcendental to conceptual analysis for the following reasons: (a) Conceptual analysis is usually taken to be a merely conventional linguistic analysis. Michael Devitt and Kim Sterelny (1999, pp. 282-3), for instance, write: "Since all the concepts that the analysts are interested in are ones for which we have words (on the language-of-thought hypothesis, the concepts are mental words synonymous with the public words that express them), their method is hardly distinguishable from the ordinary language philosophers' investigation of the use and misuse of words." Devitt and Sterelny call the method of conceptual analysis "armchair thought experiment" and they quote G. J. Warnock who says that philosophy as conceptual analysis is "the study of the concepts that we employ, and not of the facts, phenomena, cases, or events to which those concepts might be or are applied" (Devitt and Sterelny 1999, p. 282). I do not think that Kuhn would endorse this kind of philosophy. He was very much interested in the facts and cases of science and could not settle for armchair reflection. As I will

29. For the sense in which grammar is arbitrary and non-arbitrary see Forster (2004).

argue, his historical studies were very significant in shaping his philosophical position. (b) Conceptual analysis is all too often understood as a definitional inquiry into the essence of things, very much like the activity of the ancient Greeks who sought to define beauty, justice, knowledge, virtue, etc. Kuhn, however, was not an essentialist philosopher. He was very much opposed to trying to define science independently of time and space. (c) I believe that transcendental analysis, unlike conceptual analysis, which oscillates between essentialism and conventionalism, fits better Kuhn's project.[30] It gives the conditions of possibility of science, (i.e., dogmatic training and exposure to exemplars),[31] it accounts for the necessity and a priority that Kuhn bestows upon his model and, finally, it is in line with Kuhn's own understanding of himself as a Kantian with moveable categories.

VII. A role for history

If, now, Kuhn's project is taken to be transcendental, what role does history of science play in it? How can something empirical be accommodated in an a priori investigation? One thing to consider is the following point made by Kripke: "Something may belong in the realm of such statements that *can* be known *a priori* but still may be known by particular people on the basis of experience. [. . .] So, '*can* be known *a priori*' doesn't mean '*must* be known a priori'" (Kripke 1980, p. 35, emphasis in the original). Kuhn himself is a case in point. As he said, he was led, as a matter of historical fact, to the formulation of his model on the basis of his studies in the history of science, but he came to realize that he could have derived it from first principles (Kuhn 2000c, pp. 111-2). But is, then, the contribution of history dispensable? Was it only an accident that historical research led to the articulation of Kuhn's account of science?

Kuhn has said that what emerged as essential for him was the perspective or the ideology that attention to historical cases brings with it (Kuhn 2000b, p. 95). This perspective offered Kuhn a dynamic conception of science-as opposed to the static one that dominated the so-called received view-which yielded in turn the rest of his model. So, what Kuhn says is

30. Robert Hanna in the entry "Conceptual analysis" in the *The Routledge Encyclopedia of Philosophy* (1998), comparing transcendental arguments to conceptual analysis, captures nicely what is distinctive in Kuhn's work: "Transcendental arguments extend the scope of conceptual analysis from the mere definitional or logical exploration of conceptual contents (something also called 'philosophical grammar') towards insights into first principles expressing the 'conceptual geography' of the common sense world."

31. Science is Y and dogmatic training and exemplars are X in Stern's description of transcendental arguments ("one thing (X) is a necessary condition for the possibility of something else (Y), so that (it is said) the latter cannot obtain without the former").

that his a priori investigation begins after the historical perspective is assumed. In that sense, the contribution of history seems only to be preparatory. It gets us acquainted with the concepts and facts involved in the analysis that will follow. But what concept of science does Kuhn's historical research bequeath to us, what kind of facts does it report?

I believe that Kuhn's historical studies offer much more than just a dynamic conception of science. One would get such a conception by just considering that science is not merely a set of atemporal propositions comprising scientific theories, but a social activity that develops over time. One would not even need to conduct historical research to get such an idea. Once science was perceived as undergoing change in time, the dynamic conception would immediately become available. But this is not enough to yield the Kuhnian model. The dynamic conception by itself can account for several of the concerns that Kuhn had in his later writings (evaluating change of belief instead of belief tout court, preserving elements of continuity despite change), but not for all. It does not entail radical differences across revolutions; it is compatible with cumulative growth of scientific knowledge.

Kuhn (2000b, p. 98) drew parallels between biological and cognitive processes to account for revolutionary change. He said that speciation in biological evolution is the analogue of incommensurability in the evolution of knowledge. But this move is simply analogical. It comes *post festum* to illustrate a point already assumed. Where, then, does Kuhn get the idea of discontinuity in the development of science? I believe that I have shown in section VI above that Kuhn has mainly philosophical arguments to defend this thesis. But, how does history fit in?

Kuhn described a role for history in the very first sentence of his book. He summarized his project succinctly:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation of the image of science by which we are now possessed (Kuhn 1970, p. 1).

In this sentence Kuhn contends that we are possessed by an image of science-which he also calls ideal-and suggests a use of history to transform it. He rejects both anecdotal history, i.e., history that narrates the striking achievements of science, and the compilation of chronicles that assign credit for inventions and discoveries. Both these types of history are in the service of the ideal image. They take for granted the view of science that this image provides and they exemplify and enhance it. Kuhn's alternative use of history aims to combat the ideal image. But how does it do it if it does not provide evidence to refute a hypothesis?

I believe that one can get a better idea of Kuhn's use of the history of science if one compares it to what Wittgenstein did in relation to language. Wittgenstein, in his *Philosophical Investigations* ([1951] 2000) attacked an essentialist idea of meaning by bringing forward the multiplicity of ways language is and can be used. He cited real and fictitious examples of application in order to show that meaning is not an entity attached to words but a matter of practice that may vary widely from language game to language game. He said that he assembled reminders to bring into relief the great diversity that characterizes the employment of language. Wittgenstein's reference to particular examples of language use is not a recourse to empirical facts in an effort to ground or refute philosophical pronouncements. Wittgenstein does not say: "This is how we ordinarily use language. So, philosophical usage is wrong." Had this argument been successful, it would have discredited not only philosophical usage, but any non-ordinary use of language (e.g., scientific or literary), and what is more, any new application of words.[32] Wittgenstein attacks a philosophical idea of meaning by questioning its presuppositions, which concentrate on our "craving for generality," on our abstracting from the concrete case. He unmasks this strategy of assimilation by considering the multiplicity of language use. Yet, he is not interested in the actuality of the cases he gives as examples nor in how probable they are. His investigation "is directed [. . .] towards the *possibility* of phenomena" (PI 90). He says: "I'm just citing what is possible and am therefore giving grammatical examples" (Wittgenstein 1993, p. 187).

Similarly, I would say, Kuhn attacks an essentialist idea of science. He does it philosophically, but he also summons concrete examples from the history of science to illustrate the different routes science has taken. He even constructs "fictive" examples, as R. Westman has pointed out (see Section IV above). His aim is to loosen the grip of the ideal image, which insisted on an assumed uniformity of scientific method and possessed us, just like Wittgenstein was aiming to loosen the grip of the picture of language, which insisted on an assumed general form of proposition and held us captive (PI 115). Wittgenstein reminds us of facts of our natural history (PI 415), Kuhn reminds us of facts of "scientific history." The former are open to view to whoever succeeds in overcoming their familiarity. In Kuhn's case facts had to be dug out, they had to be freed from the straightjacket that had put them in the service of the official image of science. Once they were out, they functioned as objects of comparison. Just like language games "are meant to throw light on the facts of our language by way not only of similarities, but also of dissimilarities" (PI 130), Kuhn's historical cases are supposed to show (not in the sense of prove)

32. I have discussed this more extensively in Kindi (1998).

how varied things have been and can be in the future. The different scientific traditions that Kuhn describes are not presented as instances of an alternative a priori idea of what science is, nor are they collected to be used as (feeble) evidence in support of a new theory about science. Put side by side, they map out a mosaic of possibilities. Instead of the monolithic uniformity of the ideal image, Kuhn draws our attention to the diversity of practices set up by different exemplars and rules. The result of this process is an "open concept" of science, characterized not by delimiting necessary and sufficient conditions, but by a complicated network of similarities and dissimilarities.[33]

VIII. Is Kuhn's historiographical work consistent with the present reading?

Certainly, an overall assessment of Kuhn's historiographical work in relation to the present reading falls outside the scope of the present paper which is concerned to account for the relation between history and philosophy of science in the *SSR* and Kuhn's later philosophical writings. Yet, I would like to consider briefly if there is any prima-facie evidence in Kuhn's historical projects that would go against the reading advanced. It would, indeed, be quite strange—let alone unaccommodating for the present interpretation—if Kuhn did, and claimed he did, one thing when using history in his philosophy and went in the opposite direction when engaged in historiography.

What would this opposite direction be? If Kuhn—independently of his repeated explicit denials—did, *in fact,* when doing history use it to derive, ground or illustrate his philosophical model, then, the reading outlined above would have very limited and disputable value. It would apply, possibly, only to Kuhn's philosophical writings while Kuhn himself would appear torn between the two fields. But the proposed reading would be enhanced if Kuhn, in his historical work, did not engage in the effort of using history to construct and justify his philosophical model. In that respect, I offer the following considerations: First, in Kuhn's major historiographical writings, one early, before the *Structure—The Copernican Revolution*—and one late, after the *Structure—Black-Body Theory and Quantum Discontinuity—Structure's* concepts are conspicuously absent which is an indication that Kuhn's historical accounts were carried out independently of his philosophical concerns. He says so himself: "I do my best, for

33. Kuhn says that the aim of the *Structure* is "a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself" (Kuhn 1970, p. 1). Compare what Wittgenstein says about "family resemblance" terms like "games" (PI 65–71).

urgent reasons, not to think in these [philosophical] terms when I do history, and I avoid the corresponding vocabulary when presenting my results" (Kuhn 1987, p. 363). Commentators also point to this fact. Abner Shimony writes about the *Black-Body* book: "Kuhn deserves credit for analyzing the discovery of quantum discontinuity without any apparent control by the conceptions of scientific change expressed in [the SSR]. The interpretations which he offers in his new history, whatever their strengths and weaknesses, seem to be the product of immersion in the scientific texts and of reflection upon scientific problems" (Klein, Shimony and Pinch 1979, p. 437). The same claim is made by Trevor Pinch:

> It is as though *The Structure of Scientific Revolutions* had never been written. The familiar notions of paradigm, normal science, exemplars, problem solving, anomalies, crises, extraordinary science, and incommensurability are nowhere to be found. In short, virtually all the terminology associated with Kuhn's earlier work has been purged from the present one. Most of the concepts have vanished as well or else remain in such a heavily veiled form that their significance is unclear (Klein, Shimony and Pinch 1979, p. 438).

In *The Copernican Revolution,* Kuhn uses terms such as *conceptual scheme, conceptual disparities* (Kuhn 1957, p. 229), *alternative cosmologies* which do not appear in the *Structure* but which, admittedly, can be compared to *Structure*'s *paradigm, incommensurability of concepts* and talk of *different worlds* respectively. However, despite these analogies,[34] one can find claims in the same book that do not conform to Kuhn's philosophical schema. Kuhn writes, for instance, in *The Copernican Revolution:* "Each new theory *preserves a hard core* of the knowledge provided by its predecessor and *adds* to it" (Kuhn 1957, p. 3, emphasis added). This is very different from what is said in the *Structure.* Robert Westman also notes "Kuhn produces no conversion stories" in *The Copernican Revolution* (1994, p. 95), and he adds: "Kuhn had shifted to a more radical notion of revolution [by the time he published *SSR*]. [. . .] *CR*'s overall image of science, by contrast, resembles the positivist and pragmatist conventionalism of Henri Bergson, Henri Poincaré, and Ernst Mach and, especially, some of the more conventionalist passages in Pierre Duhem's *The Aim and Structure of Physical Theory*" (Westman 1994, p. 84). Trevor Pinch makes a similar claim about incongruities between the *Structure* and *Black-Body Theory* (Klein,

34. Another similarity between the *Structure* and *The Copernican Revolution* lies in the consideration of the wider social and intellectual context and, of course, in the use of the term "revolution." As regards the latter, however, Toulmin insists that Kuhn used the term differently in the two texts (1972, pp. 107–12).

Shimony and Pinch 1979, p. 439): "There is also a discernible shift of emphasis in Kuhn's current work [*Black-Body Theory*]. Science is portrayed as a process much less susceptible to human or even social influence: nature is firmly in the driver's seat." Finally, Martin Klein, in his essay on the same book, again notes that Kuhn's historical work cannot be used to clarify his philosophical categories:

> Since the creation of quantum physics would be called a scientific revolution on any account of that term, we might well anticipate that Kuhn's new book [*Black-Body Theory and Quantum Discontinuity*] would be a kind of scholarly descendant of Karl Marx's *Eighteenth Brumaire of Louis Bonaparte.* This would be a book in which the theorist of revolutionary change analyzes a particular revolution in terms of his general categories, using them to improve our historical understanding of the events in question and at the same time clarifying their meanings. But Kuhn has not written another *Eighteenth Brumaire* (Klein, Shimony and Pinch 1979, p. 430).

The second point to consider, then, is that Kuhn's historical work, far from being the ground or the illustration of his philosophical contentions, includes claims that can and have been interpreted as even going against claims in the *Structure.*

Kuhn himself did not find the two accounts (historical and philosophical) as divergent as the above critics have taken them to be. "Often I do not know for some time after my historical work is completed the respects in which it does and does not fit *Structure.* Nevertheless, when I do look back, I have generally been well satisfied by the extent to which my narrative fit the developmental schema that *Structure* provides" (Kuhn 1987, p. 363). In this remark Kuhn restates that he conducts his historical and philosophical research independently of each other, but recognizes that, *retrospectively,* his historical narrative turns out not to have undermined or—more positively—even to be in general accord with the developmental schema he proposed. He further claims that specific episodes in the history he recounted may be taken, again retrospectively, to illustrate concepts of the *Structure.*[35] If we bracket the thought that historical data can be made to conform to any philosophical model, including, obviously, Kuhn's own, how can we explain the fit that Kuhn sees? Is it a happy coincidence or did he fiddle with the facts? This is an important issue to consider because if Kuhn induced the harmony between the two accounts, he

35. According to Kuhn, Boltzmann's probabilistic derivation of the entropy of a gas is an illustration of the concept of paradigm in the sense of concrete model (Kuhn 1987, p. 363).

did not really work independently which means that he may have used his historical research to produce and support his philosophical model. And if this is correct, then the reading I have proposed is not borne out, at least, by Kuhn's historical work.

I think Kuhn was right (and sincere) when he said that he did not amalgamate the two tasks. I have offered evidence above (that the vocabulary of the *Structure* is missing in Kuhn's historical work and that there are incongruities between the two domains), which gives us reason to believe that Kuhn worked independently in the two fields. The same evidence indicates that, if there is a match, it is not a perfect one. Kuhn himself was cautious enough to write that he has only "*generally* been well satisfied by *the extent to which* [his] narrative fit the developmental schema that *Structure* provides" (emphasis added). Still, how did this general alignment come about?

The reading I have proposed offers a perspective in which the overall agreement between Kuhn's historical and philosophical accounts can be explained, but not as a matter of happy coincidence or as a match between empirical data and theory. Kuhn's accounts run, more or less, parallel to each other because in both Kuhn entertained similar concerns. Even when doing history his aim was to tear apart the seamless image of science, which, when cast on facts, muffled all diversity and difference. He aimed at ridding the minds of historians from this distorting mould in order to advance a different understanding of science. "[H]istorical study could yield a new sort of understanding of the structure and function of scientific research," he wrote in *The Copernican Revolution* (1957, p. ix),[36] while in his ""Afterword: Revisiting Planck" appended to the *Black-Body Theory,* he claims that history "can influence views about the nature of knowledge and about the procedures to be employed in its pursuit" (1987, p. 370). His objective in writing the *Black-Body* book was, he explicitly said later, "a fundamental reinterpretation of Planck's thought and of the stages in its gradual transformation" (1987, p. 349). He reinterpreted the standard account of this period which "[matched closely] a still cherished view of the nature of science and its development" (1987, p. 370). Even when Kuhn writes that this cherished view ought to be recognized as a myth (ibid.), I do not think that his remark ought to be taken in the strict sense that he has proven this view false in the manner of disconfirming a theory by data. Rather, given his other, just cited, expressed comments

36. Compare also Kuhn (1957, p. 4): "Historical analysis may not answer questions like these [What is a scientific theory, On what should it be based on to command our respect? What is its function, its use? What is its staying power] but it can illuminate them and give them meaning."

(e.g., that history can *influence* our views), I take it that the alternative narrative that he offered showed how facts are when freed from the burden of the ideal image. "[M]y primary object was just to get the facts straight" (ibid.). This is an aim internal to history—it is constitutive of the discipline itself—which can simultaneously serve Kuhn's philosophical purpose[37] of pointing to the diversity of scientific development. Getting the facts straight calls attention to the specifics of the cases under examination and requires sensitivity for detail. It, thus, highlights complexity and difference instead of assimilation and conformity to philosophical schemes.

"I was enough of a historian to know that the agreement did not exist among the people who were [concerned]" (Kuhn 2000d, p. 296). Kuhn makes this remark in his autobiographical interview trying to explain how the concept of paradigm as model emerged when he was writing *Structure* and, in particular, the chapter on normal science. The received view approach to scientific theories—with which he says he was working—required agreement among scientists, which would appear in the axiomatization of the theories in the form of axioms or definitions (ibid.). Knowing as a historian that this vast agreement wasn't there, he came up with the concept of paradigm as model. Here, one might take it that Kuhn is relying on historical data to draft his philosophical categories. But that would be, I think, a mistake. In the same interview he mentions another source that might have influenced him to get to the idea of paradigm, namely Polanyi's tacit knowledge (ibid.). It is again, I think, the historian's sensitivity for difference that helped him, together with other considerations, to shrug off a particular philosophical image that squeezed diversity into rigid preconceived moulds.

So, is Kuhn's historiographical work consistent with the reading advanced in the present paper? I think it is. What are this reading's relevant contentions? First, that Kuhn's philosophical model does not rely on history. I have shown this in the bigger part of the paper by concentrating on Kuhn's philosophical writings and mainly on the *Structure.* In the present section I have offered evidence to show that Kuhn's history is not used to ground or illustrate his model. This does not mean that Kuhn's historical work bears no relevance to his philosophy; it shares a goal with it, namely, that of undermining the standard image of scientific development. This is achieved by adherence to history's constitutive principle, i.e., "Getting the facts straight" which, by itself, contributes to the proliferation of diversity and difference. So, the second relevant contention of the reading I put for-

37. Compare Kuhn's remark that he was "[a] physicist turned historian for philosophical purposes" (Kuhn 2000d, pp. 320–1).

ward, namely, that the role of history in Kuhn's philosophical model is that of illustrating diversity, finds implicit support here. I am saying "implicit support" because there is no explicit description and statement of such a task. My reading offers a way to incorporate historical research in an otherwise a priori model and Kuhn's historical work not only does not preclude such a reading but, by being autonomous and concentrating on the details of facts, contributes to the achievement of Kuhn's philosophical goal.

Conclusion

Kuhn's was a philosophical project aiming to undermine a particular, essentialist image of science, a project that he carried out with the help of the history of science. His attack comes on two fronts: on the logical and a priori and on the historical. On the logical, he gives an account of science that focuses on the practice that sustains it (giving its conditions of possibility), and on the historical, he lays emphasis on the differences rather than the similarities that historical record illustrates. This is where the Kantian and the Wittgensteinian aspects of Kuhn's work merge. A transcendental investigation into the conditions of possibility of science is met with the observation of historical facts, which, once the mould cast by the standard image is broken, disclose a variegated landscape rather than a strictly and rigorously bounded lot. Kuhn's historical research is not simply preparatory on the way to an a priori analysis, but really vital in showing concretely how distorting the ideal image was. It is true that Kuhn could have defended the thesis of diversity based on the claim that meaning is not given by some invariant extra-linguistic entity but by rules that give rise to concrete practice. Since these rules may change, the differences between the corresponding practices may turn out to be radical. Yet, had Kuhn confined his investigation to this general and abstract level, he would not have succeeded in shaking our deep commitment to the view that science is an exception, that it is not subject to radical change. Even if his presentation of historical facts turns out to be wrong, he has managed to show concretely that things could have been otherwise. And that was a liberating move for both history and philosophy of science.

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