

## 14 Kuhn and the History of Science

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### 14.1 Introduction

The term “history of science” is at least ambiguous: it refers to both the historiography of science and the facts of the past (*res gestae*). In what follows, I will discuss, first, how Kuhn used the facts of the past in his historical philosophy of science and, second, how Kuhn’s work impacted developments in the historiography of science and the corresponding discipline. Before I proceed, though, I would like to highlight some oddities and puzzles pertaining to Kuhn’s legacy in the history of science.

### 14.2 Oddities and Puzzles Regarding Kuhn’s Relation to the History of Science

Kuhn became famous and influential in various fields including in the history of science, not so much through his historical work, but through his book *The Structure of Scientific Revolutions* (SSR), which, however, had been intended as philosophical in character. In his autobiographical interview, Kuhn said that his ambitions were always philosophical and that he thought of SSR as a book for philosophers (RSS 176; cf. ET 4). I will also argue below that Kuhn’s aim and arguments in SSR were philosophical. Yet the book itself was mostly received and interpreted as historiographical. Mary Jo Nye (2019, 10) says that SSR “was mainly intellectual history, or the history of scientific ideas and theories,” while Alexander Bird (2000b, viii) calls it “theoretical history.” As I will explain later, it is commonly, but wrongly, thought that Kuhn provided in SSR historical evidence for a non-cumulative model of scientific development. The model itself is again treated as a historical generalization or as the bare bones of a historical narrative. The first oddity, then, is that a philosophy book, which is mistakenly taken to be historiographical, influenced the developments in the history of science. Or, so I will argue.

This oddity gives rise to a second one. Kuhn’s work had a transformative effect on the historiography of science, but the historians in the field,

who acknowledge his impact, claim that their discipline was transformed by moving away from Kuhn. They also distance themselves from his legacy. Lorraine Daston, to take an example, says that most historians of science take Kuhn's account as an attempt to impose a rational order on the history of science and they reject it. They "no longer believe that *any* kind of structure could possibly do justice to their subject matter" (Daston 2016, 117, emphasis in the original).<sup>1</sup> This distancing attitude is strangely also present, if not more pronounced, among Kuhn's own students. Here, there is another, apposite oddity to be noted. Despite the fact that Kuhn, especially after the publication of SSR, increasingly focused his attention on philosophical matters, he had no philosophy students. All his doctoral students were historians of science, but they do not, at least expressly, subscribe to his way of doing history of science. Some of them were present at a symposium in Kuhn's honor, held at the Dibner Institute for the History of Science and Technology at MIT, on November 21–22, 1997, a little over a year after Kuhn's death. In their talks, several made a point of disassociating themselves from Kuhn, being rather critical of, and in some cases, even hostile to, his approach and results. The most critical was Paul Forman, but he was not the only one. Kenneth Caneva, who published a revised version of his talk in 2000, found tensions in the Kuhnian enterprise, incoherence, confusions, anomalies, ambiguities, inconsistencies, and incongruities. He also criticized Kuhn for never providing a "historical exemplar of what the Kuhnian history of science would look like" (Caneva 2000, 92). So, contrary to what Kuhn himself was saying, one might think, Caneva says, that Kuhn believed that he could teach by precept rather than by example. Andrew Pickering, at the same symposium, after stating that he was inspired by Kuhn's work, made a point of noting that his research departed not only from "the letter of the *Structure*," as he says (Pickering 2001, 503), but also from significant tenets of Kuhn's account. For instance, Pickering thought that Kuhn, laying emphasis on the scientists and the scientific community as agents of change, did not take seriously the material world and, so, offered ammunition to that party in the Science Wars who blamed Kuhn for constructivism. Thus, Pickering developed what he called a post-humanist, non-naïve realist approach to science and its history. The humanism of the SSR, Pickering said, "is

<sup>1</sup> The historian Michael Gordin (2020, 12) makes the same point more generally: "The revolt of the contributors to the journal [*Historical Studies in the Natural Sciences*] against the supremacy of philosophical structures grew with increasing assertiveness across the decade, as eclectic historians of science drew from other disciplines to find a macroscopic perch."

best regarded as a historical relic, and now is a good time to say goodbye to it, for political as well as scholarly reasons” (ibid., 508). He also said that “the claustrophobic image of the unitary paradigm and its magical self-renewal always felt wrong to [him]” (ibid.).

A third oddity stems from the fact that Kuhn was dissatisfied with the path the history of science had taken, although this path was largely paved by his own work. The contextualist turn in the history of science – a new version of what was formerly called external history – which directed attention to sociological and political factors, the cultural milieu, the institutions, and the rhetoric of science, was indeed inspired by Kuhn’s SSR, which did in fact produce a decisive transformation in the image of science that we at the time possessed. History of science was liberated from a certain constricting understanding of science and developed in various new ways. Although Kuhn praised the scholarship of this new history of science as exhibited in books such as *Leviathan and the Air Pump*, he strongly disapproved of it as not being history (RSS 316). What bothered him was that scholars and history of science students did not really care to delve into the science of each case, as he said, concentrating instead on negotiations to the neglect of the role of nature in the advancement of knowledge.

Finally, a puzzle in relation to the history of science emerges from Kuhn’s own work: Kuhn is credited with bringing about the so-called historical philosophy of science, but he wanted the two disciplines, history and philosophy, to be kept apart. He criticized attempts to amalgamate them and maintained that his model of science could be derived from first principles.

So, if we are to talk about Kuhn’s relation to the history of science, we ought to take these oddities and puzzles into account. In the present chapter, I will try to elucidate and perhaps ease the above tensions as I focus, first, on how Kuhn handled the history of science in SSR and, second, on how the history of science developed under his influence in new directions. I will concentrate on SSR since Kuhn, his historical work notwithstanding, influenced the historiography of science most notably through SSR.<sup>2</sup> Had it not been for SSR, Kuhn’s historiography would not have assumed a paradigmatic role. Kuhn’s historical work offered a revisionary history of science that gave measured consideration to the so-

<sup>2</sup> Stephen G. Brush (2000, 39) was wondering “why did Kuhn’s own publications in his primary field, history of science, have so little impact on that field?” Brad Wray (2021a, 131–132) discusses briefly the impact Kuhn’s historical work had on the history of science.

called external factors in science;<sup>3</sup> stressed the contingency in scientific development, the diversity of scientific practices, and the conceptual variability of scientific traditions; but did not clearly spell out epistemological and methodological observations as SSR did.<sup>4</sup> SSR, with its extensive impact, could not have been ignored, and it contributed to the expansion and transformation of the discipline.

In the next section, my main contention is that Kuhn did not use history as evidence for his philosophy. I compare his model to Wittgenstein's objects of comparison and argue that this model is used to highlight differences and diversity in the history of science. This feature of his model influenced the practice of the history of science as a discipline despite the fact that historians misinterpreted Kuhn's model. Then, in Section 14.4 I show how new developments in the history of science follow in Kuhn's steps.

### 14.3 How Did Kuhn Handle the History of Science in *Structure*?

Kuhn's use of historical examples in SSR has been variously interpreted. One interpretation takes Kuhn's account to be a kind of speculative philosophy of history, as an attempt to find a pattern of development in the past. Lorraine Daston, for instance, says that Kuhn's account of science is similar to those that aim to find "overarching regularities in the history of science." She calls this effort a bizarre idea, "a kind of leftover Hegelianism seeking a hidden, inexorable logic in the apparent vagaries of history" (Daston 2016, 117).<sup>5</sup> In her view, Kuhn's was "the

<sup>3</sup> Kuhn's consideration of the external factors in science was never as reductionist as the old Marxist approach of Hessen or Bernal or as decisive as the current Science Studies line of research. This is the reason that Kuhn is still interpreted as advancing an internalist history of science. But Kuhn's internalist account of science differed from the standard one at the time in that it made typical external factors, such as the subjective judgments and idiosyncrasies of scientists, vital parts of the hitherto internal scientific process. A better way to see this is to say that Kuhn challenged the distinction itself (cf. SSR-4 8–9).

<sup>4</sup> Kuhn's books on the Copernican Revolution and the black body theory, as well as his articles on specific historical issues and episodes, exemplify the kind of history that he favored: anti-Whiggish and not guided by a philosophical model, not even his own. His discussion of the atomic theory and the chemical revolution (QPT; 1952; 1976) illustrate the qualities that he valued in writing history of science.

<sup>5</sup> Gordon Graham (1997, 33) also compares Kuhn's account to a Hegelian philosophical history, but without Daston's disapproving overtones. It should be noted, however, that the comparison Graham attempts between Hegel and Kuhn on this particular point goes amiss. Hegel's reference to the Owl of Minerva that flies only at dusk does not correspond to Kuhn's use of history in SSR, as Graham surmises. First, Hegel did not mean to say that historical hindsight "gives history some control over philosophical speculation"

last attempt to give Reason (now incarnate in science) a rational history” (ibid.). A Hegelian-type philosophical history is too conjectural and metaphysical to fit Kuhn’s purpose. We do not see Kuhn looking for the meaning of history or some underlying logic that governs the unfolding of events. As David Hollinger has put it:

Connected to no total cosmic scheme, [Kuhn’s] theory stands somewhat apart from “speculative philosophy of history.” There is no determinate life cycle, no implication that certain changes are “natural” for a cultural unit at certain times, no insistence that traditions will or will not attain a certain degree of stability, no reason, in principle, why a given tradition might not live forever. The theory neither holds that change is always gradual, cataclysmic, or dialectical nor insists that change is generated by elements exclusively within or outside of “social systems.” The theory offers a thoroughly nonteleological view of change: no idea of progress is implied, nor one of decline. (Hollinger 1973, 375)

A second interpretation of how Kuhn handles the history of science in SSR says that he is offering us a grand historical narrative, a big picture. But Kuhn does not offer a rich historical developmental plot, involving detailed historical explanation and periodization. Neither does he practice integrated history and philosophy of science in SSR. He has said that he has “resisted attempts to amalgamate history and philosophy of science though simultaneously urging increased interaction between the two” (Kuhn 1980) and that he is never a philosopher and a historian at the same time (RSS 316). Do Kuhnian concepts function as Weber’s ideal types in an explanatory project of scientific development (Mladenović 2007)? No. Mladenović herself says that Weberian ideal types are “precisely and unambiguously defined” and are used as heuristic devices in order to form hypotheses that are to be adjusted by comparison to reality (p. 270). Kuhn’s concepts, however, are notoriously ambiguous (e.g., the concept of the paradigm) and Kuhn does not put forward or test hypotheses to then refine his views.

The most common interpretation of SSR, and the one I will dwell more upon, says that history of science in this book provides empirical evidence for Kuhn’s philosophical model. This interpretation, however, can be criticized in several ways. For one, Kuhn himself denied that he used history as evidence for a philosophical theory. He said to his student, the historian John L. Heilbron (1998, 511), “it is not a theory,

(ibid.). Hegel’s point was rather that philosophy waits for the things to unfold before it reflects on them. Second, Hegel’s thought is not similar to the view that Graham attributes to Kuhn, namely, that “good philosophy of science requires a good knowledge of the history of science” (ibid.). A Hegelian understanding of Kuhn’s account can also be found in Bird (2000a, 129–130) and Bird (2015, 27).

and I do not expect it to match the record.” Also, in his interview with Skúli Sigurdsson (2016, 27), Kuhn contended that one should not look at history to test the ideas of the book, to find out whether they are true or false. The model is “not meant to be applied this way.” Furthermore, Kuhn did not think that historical facts were essential to the development of his model. Long after SSR, he said that “many of the most central conclusions we [Kuhn and his fellow philosophers/historians] drew from the historical record can be derived instead from first principles” (RSS 112). In his view, what emerged as essential from his historical work was not evidential support, but the historical perspective that allowed him to concentrate on the change of beliefs in science rather than the individual beliefs as such. He could, thus, concentrate on a comparative evaluation of beliefs rather than on their individual assessment against the world, by confirmation or falsification (RSS 95).

Kuhn was also aware, already in SSR, of the descriptive/normative gap and other dichotomies current in philosophy at the time, such as the context of discovery and context of justification distinction: “I could scarcely be more aware of their import and force,” he said (SSR-4 9). He knew that his “sometimes normative” theses could not be derived from history, “a purely descriptive discipline” (SSR-4 8, 206). His way of dealing with the problem was to say that these distinctions were part of an approach that he would overcome. Equally mindful was Kuhn of the underdetermination of theories by historical data: “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” (RSS 313–314). Finally, if the model of science developed in SSR was an empirical generalization from historical evidence, it could be argued that it is very thinly and inadequately supported. Critics have actually challenged Kuhn’s interpretation of historical examples and pointed out the model’s slim evidential basis. Wes Sharrock and Rupert Read (2002, 107), for instance, found Kuhn’s account of science “largely unevidenced,” while Janet Kourany (1979) questioned the historical basis of Kuhn’s model altogether.<sup>6</sup> Kuhn himself admitted in the Preface of SSR (SSR-3 ix) that his presentation has a schematic character and that he has not exploited all the historical evidence he had. Also, several scholars disputed how Kuhn interpreted

<sup>6</sup> According to Caneva (2000, 110), “we find Kuhn looking for answers to his problems in one or another favored analogy, not in the historical record.” Caneva also says that “*Structure* provided only snippets of historical evidence in support of its major claims” (116). Brad Wray (2015, 141) responds as follows: “it seems that those historians who have seen *Structure* as an example of poor historical scholarship are mistaken. It is not poor historical scholarship, because it was not historical scholarship at all.” For more on the way critics assessed the use of history of science in SSR, see Kindi (2005b).

particular historical episodes to, purportedly, use them to base his model on. Kuhn's understanding of scientific revolutions was particularly contested. Steven Weinberg (1998) maintained that the transition from Newtonian to Einsteinian mechanics, or from classical to quantum physics, was not a revolution in the Kuhnian sense. Dan Garber (2009) makes the same point about "Galileo, Newton and all that": it wasn't a scientific revolution. Ernst Mayr (1972, 988) thinks that the Darwinian revolution that Kuhn appeals to does not conform to the Kuhnian idea of a revolution, while Ursula Klein (2015) thinks that the chemical revolution, which again features prominently in SSR as a paradigmatic example of a Kuhnian revolution, was "[a] Revolution that never happened."<sup>7</sup> So, if we do not want to dismiss Kuhn's model as unevicenced or poorly supported, we need to look for some other use of history in his work, that is, other than as empirical evidence for a philosophical model.<sup>8</sup>

In Kindi (2005b), which discusses the relations between history and philosophy of science in SSR, I advanced a philosophical reading of the text showing that Kuhn's arguments in favor of discontinuity in scientific development are philosophical, not empirical, that is, not based merely on historical evidence. I suggested that Kuhn uses historical examples as anti-essentialist Wittgensteinian "reminders" that expose a variegated

<sup>7</sup> To be sure, there are scholars who have argued that Kuhn's model is applicable to the history of science and who do not share the above views about particular historical episodes. See, for example, Hoyningen-Huene (2008) and Chang (2012b) about the Chemical Revolution, or Tanghe et al. (2021) about evolutionary biology. But they would not claim that Kuhn, as a matter of historical fact, has actually based his model on broad and solid historical evidence.

<sup>8</sup> It is true that Kuhn is not always consistent in what he says about his model. In the Postscript to SSR, for instance, responding to his critics, Kuhn speaks of his book as "a viewpoint or theory about the nature of science" and refers to his "descriptive generalizations that are evidence for [his] theory" (SSR-4 206). Taken out of context, these expressions may prove misleading. Kuhn does not mean to say that he formed a philosophical theory about science based on generalizations from experience. The use of the term "theory" for his model is just generic and it is interchanged with "viewpoint." He says of his viewpoint or theory that "it need not be right, anymore than any other theory" (ibid.) and notes that his descriptive generalizations are related circularly with his theory. They provide evidence, but also are derived from it (for other theories of science, they constitute, says Kuhn, anomalous behavior). Kuhn writes that he has used parts of the theory that is presented in SSR as "a useful tool for the exploration of scientific behavior and development" (SSR-4 207). And in the Introduction to SSR, Kuhn speaks of his generalizations that are about the sociology or the social psychology of scientists (SSR-4 8) and of *theories* about knowledge that, presumably, include his own (SSR-4 9). Again, in both cases he does not imply any theory building of the kind we have in science. His generalizations are simply general observations as opposed to singular statements; they have not been reached by any kind of systematic investigation, and they do not exhaust his complete "theory," which also includes "conclusions that belong to logic and epistemology" (SSR-4 8).

landscape in the development of science. My philosophical reading borrowed from the transcendental approach and amounted to the claim that Kuhn in SSR offers the historicized conditions of the possibility of science, that is, exposure to exemplars and dogmatic training. What I wanted to appropriate from a transcendental reading of SSR was *a priority*, so that the reading was in line with what Kuhn himself said about deriving his model from first principles, and *necessity*, so that Kuhn's particular model of science could be singled out from the rest that aspire to capture the nature of science.

I would now like to suggest another possible reading that I find illuminating: I would like to compare Kuhn's model of science, and its relation to history, to Wittgenstein's objects of comparison, an idea that I mention but do not expand on in earlier work (Kindi 2005b). There, I speak of historical examples in Kuhn's work as objects of comparison. Here, I would like to say that the whole model also functions as an object of comparison. Before I present this reading, however, some clarifications are in order: I do not claim that Kuhn deliberately adopted this particular Wittgensteinian idea. Although he read about it in the *Philosophical Investigations* and marked the particular remarks where objects of comparison appear,<sup>9</sup> there is no explicit discussion of this concept in Kuhn's work.<sup>10</sup> In addition, we know that the reference to Wittgenstein's work in SSR does not appear in drafts prior to the published text (Isaac 2012, 105–106; Hoyningen-Huene 2015a, 188). So, Kuhn could not have consciously intended his model as an object of comparison. Why, then, do I propose this interpretation? Because I think that it (1) is exegetically adequate since it offers a coherent account of Kuhn's project, (2) clarifies better than other readings what Kuhn tried to do with his book, (3) allows a better appreciation of Kuhn's contribution, and (4) provides a better evaluation of the critical reception of Kuhn's work. I do not discard the transcendental reading, given especially the fact that Kuhn calls himself a Kantian with moveable categories (RSS 104, 264), but I now think that attempting a transcendental reading may stretch too much, or trivialize, what we understand by transcendental arguments.

What are Wittgenstein's "objects of comparison"? The term appears in two remarks in *Philosophical Investigations* (PI):

<sup>9</sup> I have access to Kuhn's own copy of the *Philosophical Investigations*. It is the 1953 bilingual, first edition of Wittgenstein's book.

<sup>10</sup> There was not much discussion in the whole Wittgenstein literature either until rather recently. Oskari Kuusela (2008; 2019) is the one who has most highlighted and discussed this concept in Wittgenstein's work.



PI §130: Our clear and simple language-games are not preliminary studies for a future regimentation of language – as it were, first approximations, ignoring friction and air resistance. Rather, the language games stand there as *objects of comparison* which, through similarities and dissimilarities, are meant to throw light on features of our language.

PI §131: For we can avoid unfairness or vacuity in our assertions only presenting the model as what it is, as an object of comparison – as a sort of yardstick; not as a preconception to which reality *must* correspond. (The dogmatism into which we fall so easily in doing philosophy.) (Wittgenstein 1953/2009, emphases in the original)

In these remarks, Wittgenstein says that language games, that is, real and imaginary examples or models of language used in some particular practice, function as objects of comparison, that is, as measuring rods that we lay against the object that we want to measure. If the measuring rod is a meter, we measure length and we report centimeters; if it is a thermometer, we measure temperature and we report degrees. In the case of language games, we note similarities and dissimilarities between linguistic practices. Wittgenstein warns, however, that we should not proceed to claim that the object of the investigation really has the properties that we project on to it, that it really fits the measuring rod, the mode of representation that we have chosen to discuss it. The features that we project upon the object of the investigation are features of the mode of representation, not intrinsic features of what we measure. If we change the measuring rod and we have a yard instead of a meter, we will be measuring inches and feet. The object of comparison determines only *how* we will conceive the object of the investigation, how we will discuss it; it does not determine what it actually is. If we were to do that, we would be doing arbitrary, speculative metaphysics. And, according to Wittgenstein, we would be falling into dogmatism, thinking that reality must fit our preconceived idea.<sup>11</sup>

The point of the comparisons between a yardstick and a concrete particular is to achieve some end, for instance, precision in measuring or the testing of a hypothesis. In Wittgenstein's case, the point of using, or setting up, language games as objects of comparison is to clarify the function of language, especially by highlighting the different ways language is, and can be, used. He, thus, undermines the essentialist understanding of language, an understanding that lays emphasis on common features and similarities in order to determine necessary and sufficient conditions. The language games that Wittgenstein uses do not carry any

<sup>11</sup> Wittgenstein expands on this idea in a comment published in *Culture and Value* (1977/1994, 21–22).

ontological import. We are not supposed to ask questions such as: “How many language games are there?” or “Is X a language-game?” Developing and using language games is a technique, a method, to bring about philosophical elucidation.

I want to claim that Kuhn’s model of science functions as an object of comparison in order to highlight differences in various scientific traditions and to destroy the ideal image of science that governed both the history and the philosophy of science, an image that projected a seamless uniformity upon scientific practice.<sup>12</sup> Kuhn’s examples of paradigms and revolutions are not pointed at as actual instances of his model’s concepts, but are themselves used as objects of comparison and as reminders of the different ways science is practiced. Kuhn did not commit himself to there being particular revolutions in history. He discussed the more obvious ones, those that are commonly discussed as revolutions (SSR-4 6), but did not pronounce on which ones are to be recognized as such. He left it to historians to proclaim which of the conceptual changes that they study will be deemed revolutionary. He said: “I am repeatedly asked whether such-and-such a development was ‘normal or revolutionary,’ and I usually have to answer that I do not know.... Part of the difficulty in answering is that the discrimination of normal from revolutionary episodes demands close historical study” (Kuhn 1970a, 251).

So, the arguments that are supposed to show that certain historical episodes are, or are not really, Kuhnian revolutions, in order to support or discredit Kuhn’s model, misunderstand the nature of his proposal and do not affect its validity. One can certainly study a particular historical development and call it a revolution, but there is no point in trying to match it with the schema Kuhn provided. The Kuhnian schema is supposed to help you look at the historical material differently. This is the point of its use.

Kuhn’s concept of paradigm has also not been formed by systematic empirical observation. It is a concept that Kuhn used to account for agreement among scientists on a small scale.<sup>13</sup> If scientists were bound by the universal rules of the presumed scientific method, Kuhn would not be able to account for the variability that he had encountered as a historian (RSS 296). All disciplines that aspire to be called scientific

<sup>12</sup> I discuss these issues more extensively in Kindi (2015; 2017). Also, in Kindi (2012b), I consider how empirical material, from either history or sociology, features in Kuhn’s account of science and criticize attempts in the science studies literature to reduce philosophy to empirical research by appealing to Wittgenstein’s philosophy.

<sup>13</sup> Cf. SSR-3 viii. On Kuhn’s concept of paradigm, see Kindi (2012a).

would have to follow the same rules. But with paradigms, Kuhn could limit the consensus to particular scientific communities, bound together and shaped by taking particular scientific achievements as models. Kuhn was not committed to the existence of one or more paradigms in each scientific community and did not say whether these remained unchanged or were further articulated during the course of normal science.<sup>14</sup> The use of the concept of paradigm served Kuhn's philosophical purpose, which was to transform the image of science by which we were then possessed (SSR-4 1). He did not derive the concept of paradigm, nor any other concept of his model, for that matter, from experience, even though his coming to use it was informed by his experience as a scientist and historian. Hence, from this perspective, complaints such as Kenneth Caneva's that Kuhn does not tell us "how one actually identifies a paradigm" (Caneva 2000, 116) are off the mark as criticisms of Kuhn's model.<sup>15</sup>

Kuhn's aim in SSR was philosophical, not historiographical. He was interested in "the discovery of essentials" in science (RSS 129) and spoke of his view as "an altered view of the nature of science" (RSS 129, cf. SSR 206–208). The term "structure" in the book's title does not point, as Lorraine Daston seems to think, to overarching regularities in the development of science, but instead points to the logic of science, its skeleton and frame. It is a reference to what makes science what it is (cf. SSR 129, 203). This seems like an essentialist undertaking, but it is not of the kind that seeks once and for all definitional characteristics. It is a logical investigation that explores the essence or the structure of science, similar to Wittgenstein's logical investigation of the essence or structure of language and propositions (Wittgenstein 1953/2009, §§89–92). But instead of trying, with the typical metaphysicians, to penetrate phenomena to find a hidden essence, Kuhn's and Wittgenstein's investigations stay on the surface and survey the different ways science is practiced and language is used, respectively. It can be said that they both look for the essence as expressed in grammar, that is, in the surveyability of different cases (Wittgenstein 1953/2009, §371, 122).<sup>16</sup>

<sup>14</sup> Nickles (2012), for instance, criticizes Kuhn for presenting paradigms/exemplars as rigid, fixed, and permanent. Dudley Shapere also raises the criticism that we do not know where "we draw the line between different paradigms and different articulations of the same paradigm" (Shapere 1964, 387).

<sup>15</sup> Kuhn said the following to Skúli Sigurdsson (2016, 27) about his model: "Can you always locate the paradigm, can you always tell the difference between a revolution and a normal development? It's not meant to be applied that way."

<sup>16</sup> Wray (2021a, 142–148) advances the view that Kuhn's use of "structure" originates in sociology. Kuhn may have come across the term "structure" in sociological studies with which he was familiar, but he did not try to find the structure of science by conducting

If Kuhn's model of science is an object of comparison that is not assessed empirically, why should we prefer it to other possible objects of comparison? We should prefer it because, first, it fulfils an important objective, set by Kuhn, but also more widely shared, namely, to free the study of science from the ideal image. Second, it gives a more inclusive and coherent picture of science than other models. For instance, what in other models is aberrant behavior of scientists – for example, resorting to values that are subjectively interpreted and assessed – is an essential part of science's practice and success (RSS 129). Finally, Kuhn's model is more fruitful in comparison to other models, say, the logical positivist model, in that it opens up more possibilities of research. This is a consideration that features prominently in Kuhn's discussion of scientific revolutions. A new paradigm needs to carry the promise of fruitful research. As we will see in the next section, where we will discuss the impact of Kuhn's model on the history of science, Kuhn's understanding of science has proven very fruitful indeed.

#### **14.4 Kuhn's Impact on the History of Science and Further Developments**

Kuhn reflected at some length on the state of the history of science in the 1980s on two occasions.<sup>17</sup> First, in 1984, in his contribution to the centennial of George Sarton's birth, which was included in a *Recollections and Reflections* section of *Isis* together with the pieces of other "distinguished statesmen of the history of science," as Arnold Thackray put it in his editorial (Thackray 1984).<sup>18</sup> The second occasion was in 1985 when he addressed the Seventeenth International Congress of History of Science at Berkeley. In the first piece, Kuhn comments on the professionalization and shaping of the field that he relates to the expansion of higher education but, most importantly, to the interest and concern about the power of science as it emerged after the role it had in the Second World War victory of the Allied forces. History of science was thought to be more suitable, in comparison to the more technical philosophy of science, for educating the wider public about

empirical investigations of the type sociologists undertake. Wray rightly criticizes the understanding of "structure" as a Popperian historicist law that underlies scientific development allowing predictions (pp. 148–151).

<sup>17</sup> Kuhn reflected on the history of science before (ET 3–20, 105–126, 127–161) and commented on developments in the discipline in his autobiographical interview (RSS 316–317).

<sup>18</sup> The other distinguished historians were John T. Edsall, I. Bernard Cohen, A. Rupert Hall, and A. C. Crombie.

science and for bridging C. P. Snow's two-culture gap. Yet historians of science, according to Kuhn, concentrated on training future professionals and neglected to create wider interest for the subject.

In the 1985 piece, Kuhn goes over the programs of three international history of science congresses: the sixth at Amsterdam in 1950, the tenth at Ithaca in 1962, and the seventeenth at Berkeley in 1984. He notes (1) the very rapid growth in the volume of papers presented, a growth that led to the transformation of the discipline; (2) a rapid shift of attention from older science (in Antiquity, the Middle Ages, and the Scientific Revolution) to newer and more contemporary topics; (3) a move away from history of ideas to more social and institutional history; and, finally, (4) a greater interest in fields of study and geographical areas that were not sufficiently researched before. He attributes these developments to the same reasons that he discussed in his 1984 comments, educational expansion and the need for scientific literacy. But he also adds that the placement of historians of science in history departments and the task of addressing a lay audience that knew very little science may have led to the turn to social history, away from technical scientific matters. Kuhn says that he welcomes these developments, but he cannot conceal his dissatisfaction with the sidelining of intellectual history and the neglect of the role of reason and experiment in scientific development in favor of socio-economic interests.<sup>19</sup> He recognizes that the issue is mostly philosophical, but thinks historians of science should also be concerned as they "played a primary role in the destruction of the traditional viewpoint," that is, the viewpoint that takes science to be progressively approaching the real world (Kuhn 1986, 33). Kuhn envisions a greater interaction between intellectual and social-institutional history and finds the prospects for such an enterprise encouraging (*ibid.*).

Kuhn may not have commented explicitly on the role of his own work in the developments in the history of science, but historians of science have generally acknowledged the great significance of his contribution. To take a few examples: Michael Gordin (2020, 12) has said that the discipline of the history of science emerged "from under Kuhn's overcoat." Jan Golinski was very confident that

there is no way back to the time before Kuhn's intervention. His sonorous words at the opening of *Structure* still hold good: taking history seriously challenges all preconceived ideas of the nature of science. It calls into question any assertions that presuppose the unity of science across time and space or its singularity as a

<sup>19</sup> Ken Adler (2013, 94) uses the word "dyspeptic" to characterize Kuhn's reaction to the fact that general historians did not appreciate the technical part of the history of science.

cultural phenomenon. The move cannot be undone, even if the arguments for it do not have quite the force of logical compulsion. (Golinski 2012, 25)

Alex Soojung-Kim Pang (1997, 167) contends that post-constructivist history of science, a quieter approach in comparison to the more pronounced constructivism of science studies and the Edinburgh School, “takes as a starting-point the work of Kuhn, the Edinburgh School, and science studies, but modulates their tone and claims.” Finally, the historian Nathan Reingold (1980, 484) calls Kuhn a liberator for freeing history of science “from both practitioners’ history and dominant factions in the philosophy of science.” Practitioners’ history is the old fashioned, anachronistic, triumphant, cumulative history of science, written by scientists and propagated in textbooks. The dominant factions in philosophy of science that Reingold refers to are the various versions of logical positivism, which again see science as achieving a teleological goal (p. 485).

Still, despite the acknowledgment of Kuhn’s overall impact on the history of science, many historians of science see his influence as a burden that has been, and needs to be, lifted. What they find particularly bothersome is a certain understanding of Kuhn’s model of scientific development. They take it as imposing on the various practices of science universal structures, overarching regularities, or theoretical canons, or as cramming the vast variety and heterogeneity of practices into a few empirical generalizations that are assembled in patterns that repeat themselves in cycles. Peter Galison (1988, 394), for instance, has said that for Kuhn “there was a universal structure for the supplanting of one theory with another in his timeless cycle of epochs: normal science, crisis science, revolutionary science, and the return of normal science.”<sup>20</sup> Stephen Brush (2000, 39–40) notes the “hostility and indifference to Kuhn’s work displayed by many historians of science,” while Mario Biagioli (2012, 480) refers to “evidence of the increasing irrelevance of *Structure* to contemporary history of science (and even doubts about its historical impact on the field).” Biagioli acknowledges that *Structure* helped create the field, but says that it “no longer frames the research agenda” (p. 479).<sup>21</sup>

Historians of science distance themselves from Kuhn’s work because they see it as bringing schematic and universalistic *philosophical*

<sup>20</sup> Similarly, Bird (2015, 27) thinks that Kuhn finds a “fundamentally cyclical pattern with its alternating phases of normal and extraordinary (revolutionary), science” in the history of science.

<sup>21</sup> A similar view is stated by Wray (2021a, 139): “historians of science generally have a bleak view of the relevance and impact of SSR on the history of science.”

considerations to bear on the complicated and heterogeneous manifold of historical reality. Philosophy is seen as the villain from whom they should protect themselves and the discipline. For better or worse, says Michael Gordin (2020, 12), “by the mid- to late 1980s, the historiography of science was emancipated from philosophy.” Historians and sociologists, contended the sociologist Trevor Pinch (2017, 128), “fought the common enemy (the philosophers of science who beat up on relativism and felt the sociological approach to be impossible).”

History of science, liberated from philosophy, presumed to be despotic and censorious, turned to other fields and disciplines for inspiration, collaboration, and guidance. Sociology, anthropology, literary theory, political science, political theory, and politics have all provided historians of science with the intellectual and practical resources that helped the discipline grow and develop into a multidisciplinary, expansive field. The “enormous camaraderie” (Pinch 2017, 128) between historians of science and sociologists formed a whole tradition of historical/sociological research – the Strong Programme, SSK, and Science and Technology Studies (STS) – and produced works such as Shapin and Schaffer’s *Leviathan and the Air Pump* (1985). Anthropology, with its attention to the local and the particular, and the foregrounding of cultures in their dynamic development (see Dear 1995; Galison 2011), offered a trove of concepts, such as Geertz’s notion of thick description, and tools and methods, such as ethnomethodology, that were of lasting significance for the history of science, and led to works such as Latour and Woolgar’s *Laboratory Life: The Social Construction of Scientific Facts* (1979). Focus on narrative and texts has invited considerations from rhetoric and literary theory with works such as Mario Biagioli’s *Galileo, Courtier* (1993), Jean Dietz Moss’ *Novelties in the Heavens: Rhetoric and Science in the Copernican Controversy* (1993), and Peter Dear’s anthology *The Literary Structure of Scientific Argument* (1991). Politics in general raised issues that pertain to interests and power relations. A characteristic book in this regard is Naomi Oreskes and Eric Conway’s *Merchants of Doubt* (2010).

How did history of science further develop, and did Kuhn have anything to do with it? History of science went, on the one hand, local: it concentrated on case studies, micro-histories, and highly specialized and detailed research topics, focusing considerably on archival material. Daston (2009, 809) spoke of “miniaturizing tendencies.” On the other hand, history of science also went global. Studying the circulation of knowledge, historians of science transcended national borders, studied non-Western practices in an inclusive and cosmopolitan spirit, and abandoned metaphors of center and periphery or of unidirectional

dissemination.<sup>22</sup> Now these developments interweave with not only negotiations, controversies, and rhetoric that have engaged historians of science since they left behind the purely intellectual history of science, but also “trading zones” and “contact zones,” migration and appropriation, translation studies, postcolonial studies, and social, political, and economic relations (Secord 2004). The circulation of knowledge involves diverse groups that need to communicate as they participate in various transactions that are not limited to purely epistemic ones. An opposite development is the demotion of science, from its distinguished and privileged status to its assimilation to other cultural practices, such as art and craft, that yield different, and no less important, types of knowledge (Renn 2015, 242).<sup>23</sup> History of science becomes more history of knowledge. In a parallel line, Ken Adler (2013, 88) also advocates the jettisoning of “moralistic assertions of scientific exceptionalism,” enlarging thereby the scope of history of science. Our historical investigations, he says, should be not just science but a set of activities that constitute a certain cultural milieu. Science is not something separate from its environment, but mutually interlocked with and mutually formed by its surround. In his view, historians of science should treat science as any other historical topic. It should be studied

alongside contemporary knowledge-making practices not canonically scientific. And in fact historians of science have repeatedly charted the affinities of emergent scientific practices with cognate ways of knowing and manipulating the world – Renaissance craftwork, early modern natural philosophy, Enlightenment engineering, Victorian quantification, or the twentieth century’s discovery of “traditional” healing – the better to identify those aspects of knowledge making that came to be considered scientific. (Adler 2013, 95)<sup>24</sup>

The scope of history of science as a discipline is also broadened through another route: science is no longer understood narrowly and anachronistically. Science as an object of study is variously understood, depending on what the term “science” and the related words, for example, “natural

<sup>22</sup> For more on this global trend, see Secord (2004), Fan (2012), and Renn (2015).

<sup>23</sup> SSK’s impartiality with respect to “truth and falsity, rationality or irrationality, success or failure,” and its symmetry principle, that is, that the same kinds of causes will explain both sides of the above dichotomies (Bloor 1976/1991, 7), have also contributed to the assimilation of science to other practices.

<sup>24</sup> Adler suggests substituting “episcience” for “science” in the “history of science” so that the name of the discipline reflects more accurately what professionals in that discipline do. Episcience, according to Adler, is “the imbrication of science with its ‘surround,’ or what historians of science generally invoke with terms such as ‘environment,’ ‘milieu,’ or ‘*Umgebung*’” (2013, 96). One might say that episcience may come close to another science studies concept, “situated knowledge,” which requires embedding knowledge in context (Haraway 1988).



philosophy” or “history” signified in different times and places. Exploring affinities and differences between types of knowledge and different genres has led to the transgression of disciplinary boundaries as we understand them today. Art, craft, technology, engineering, data science, theology, law, literature, and humanities are all legitimate objects of research in history of science.

Contemporary history of science not only has transgressed boundaries between disciplines, but also has transgressed and overcome long valued dichotomies, such as the internal/external distinction (e.g., Shapin 1992), the context of discovery/context of justification distinction (e.g., Arabatzis 2006; Schickore and Steinle 2006), and the content/context distinction (e.g., Shapin 1980). Another set of developments relates to the understanding of science as practice. This approach has brought into prominence institutions, experiments, and material culture, while considerations of gender, race, and class have enriched both the perspective and the range of research topics of the history of science. History of science has also given prominence to the study of contingency in scientific development, that is, that science may have developed in an alternative way. Finally, from the focus on ideas, history of science has shifted attention to communities and individual scientists (interest in new, non-idolizing types of biography has surged),<sup>25</sup> reaching, in the contemporary scene, post-humanistic studies.

Now, what is the relation of all these developments in the history of science to Kuhn’s work and, more specifically, to SSR? Wray (2021a, 7) says that the impact Kuhn had on the history of science “was comparatively insignificant.” Mario Biagioli (2012, 480) believes, as we have seen, that SSR is irrelevant to contemporary history of science and that there are doubts about its historical impact on the field. He also claims that “the ‘turn to practice’ only sealed ... the increasing drift of *Structure* away from the research agenda of the field.”<sup>26</sup> Biagioli notes that “*Structure* was an attractive ‘forward-looking statement’ about what the history and philosophy of science could be,” offering “examples of the exciting work the discipline might produce” (pp. 282–283). Jouni-Matti Kuukkanen (2013, 96) writes that the local turn in the history of science

<sup>25</sup> See Nye (2019, 11): “Kuhn shifted many historians’ interest away from a heroic history of ideas and individuals toward the history of groups and communities, that is, from histories of eureka moments of discovery to histories of communication, organization, and social context.”

<sup>26</sup> In Biagioli’s view, Kuhn’s work gave the field visibility, but now can function only as a “recruitment tool,” “seducing” students into the field of history and philosophy of science by providing a model “that appears to explain everything, or almost everything,” leaving at the same time “a lot of the answers open” (Biagioli 2012, 483).

“is incompatible with Kuhn’s attempt to draw general conclusions on the nature of science on the basis of historical sources,” since it lays emphasis on particular practices and cultures and presents a fragmented image of science. He observes, however, that a localist historiography implies a non-localist theory of historical interpretation (i.e., that “a historical interpretation could be made without any higher-level constructions”) and the general idea that science is inherently something local. In that sense, Kuukkanen argues, contemporary localist historiography is not incompatible with Kuhn’s approach.

Kuukkanen is right that the localist turn in the history of science is not incompatible with Kuhn’s approach, but not for the reasons he cites. We do not need to assume that the localist perspective presupposes some general idea or construction so that it becomes compatible with what Kuhn did. As I have explained in the first part of this chapter, Kuhn’s model was not formed by drawing general conclusions from historical evidence. Kuhn did not present a schema that was supposed to fit every developmental case in science.<sup>27</sup> It would take a very insensitive historian to prescribe such a one size-fits-all pattern to science. Far from being incompatible with the localist turn, Kuhn’s work made it possible by combating anachronism, the projection of the contemporary conception of science to the past as if it is the only one, and by laying emphasis on the particular practices that develop around a paradigm. The priority of paradigms over rules that Kuhn speaks of in SSR is exactly supposed to highlight that scientists do not follow general rules that allegedly comprise the scientific method. Scientists bond together by using specific achievements as prototypical ways of doing research. This emphasis on diversity paved the way for turning the attention of historians of science to the peculiarities of each historical case.

Biagioli’s statement that the turn to practice in the historiography of science increased the drift away from SSR is also questionable. SSR, more than any other book in late 1950s and early 1960s, contributed to the understanding of science as practice rather than as a set of statements. It brought to the fore the elements that build a practice, namely, education and initiation based on paradigms, textbooks, institutions, values and norms, training, research procedures, and the building and communication of communities. History of science took up these newly

<sup>27</sup> Kuhn distances himself from Gaston Bachelard’s work. He characterized it as “too much of a constraining [system].” Bachelard, Kuhn said in his autobiographical interview, “had categories, and methodological categories, and moved the thing up the escalator too systematically for me” (RSS 285). Kuhn did not want to form that kind of a model for science, which he regarded as constraining, overly systematic, and too general.

opened fields of study and explored them further. The current research agenda in the history of science may not be framed by SSR (it is already sixty years since its publication), but it owes a lot to Kuhn's pioneering work. SSR's emphasis on practice held the promise of fruitful research and steered the course of development in a new direction, just like Kuhn has said it happens in revolutionary changes in science. Scientists decide between alternative pathways based on the future promise the emerging paradigm holds (SSR 156). This is exactly what Mario Biagioli himself acknowledges when he says that SSR was an attractive "forward-looking statement" and offered "examples of the exciting work the discipline might produce" (2012, 482 and 483)

Kuhn's work, and more particularly his discussion of paradigm shifts and incommensurability, paved the way for many other topics in the historiography of science. Studies of scientific controversies and negotiations relate to Kuhn's showing that the choice of paradigm or theory does not depend on logic and experiment alone, but involves the judgment of individual scientists who may disagree among themselves. Change of allegiance requires persuasion, and rhetoric becomes a legitimate area of study in relation to science. Similarly, Kuhn's rejection of a mechanical understanding of rationality opened the possibility of attributing rational decisions to individuals and groups that differ on how they comparatively assess the merits of the competing paradigms. This means that alternative ways of scientific development should not be excluded or disqualified as irrational, a thought that foreshadows more recent developments in science studies literature. Kuhn has addressed the contingency issue already in his 1951 Lowell Lectures when he discussed the rejection of the phlogiston theory (QPT 57–76). Finally, conceptual incommensurability and problems of communication between scientists defending incommensurable paradigms invite discussions of translation. Translation studies and knowledge transfer research may be seen as involving considerations that were first taken up in SSR in relation to the implications of incommensurability.

Anti-Whiggism and sensitivity to the varying meaning of words, including that of "science," was highlighted in many ways in SSR and, in general, in Kuhn's work (see SSR-4 148; ET 154; RSS 290), as was the blurring of the age-old distinctions, such as the internal/external, the is/ought, and the context of discovery/context of justification distinction.<sup>28</sup> Even the blurring of the language/fact distinction, so crucial for constructivism in the history of science, can be traced back to

<sup>28</sup> See, for example, Alexandre Koyré's comment about SSR to Kuhn: "you have brought the internal and external histories of science, which in the past have been very much

Kuhn, who maintained that “the so-called facts proved never to be mere facts, independent of belief and theory. Producing them required apparatus which itself depended on theory, often on the theory the experimenters were supposed to test” (RSS 108). Last, one should not ignore the project “Sources for History of Quantum Physics” that Kuhn conducted between 1962 and 1964, together with his students John Heilbron and Paul Forman and with the assistance of Lini Allen, a project that involved interviews with the leading figures of twentieth-century physics. This project is now considered a predecessor of oral history and new historiography (see Heesen 2020). Similarly, Kuhn’s *Black Body Theory and the Quantum Discontinuity, 1894–1912*, despite the criticism that it has received, has been a point of reference in the historiography of quantum theory.

#### 14.5 Conclusion

Kuhn’s work decisively influenced, and even transformed, the historiography of science, but historians want nothing to do with it. They keep Kuhn’s philosophy at bay, and philosophers treat him as a historian. He was an outcast. At the heart of this state of affairs is a series of misunderstandings that proved, however, as is often the case, very fruitful, at least in the field of the history of science (in philosophy Kuhn was simply ostracized).<sup>29</sup> Historians of science have largely misunderstood Kuhn. Kuhn did not try to impose a philosophical ideal on the history of science; he did not look for laws underlying the scientific development; he did not try to generalize from a few cases; he did not use historical evidence to ground his model. Kuhn’s work breached established distinctions and boundaries, and, in consequence, it was difficult to appreciate its revolutionary import. SSR, in particular, did not fit the traditional molds in which it was cast, which meant that tensions and incongruities would, not infrequently, surface. Still, despite the misreadings or the disregard, Kuhn’s SSR played a groundbreaking role in how historiography and the discipline of the history of science developed.

apart, together.” Kuhn said that he had not thought of this as what he was doing, but he felt that Koyré was right in his judgment (RSS 286).

<sup>29</sup> John Searle (1993, 71) says that “a solid and self-confident professorial establishment, committed to traditional intellectual values,” blocked the works of authors such as Jacques Derrida, Thomas Kuhn, and Richard Rorty from affecting analytic philosophy, something that did not happen in the rest of the humanities.

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