Chapter 10
Kuhn and Philosophy

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Abstract In the present chapter, I discuss how Kuhn's work was received, not by philosophers of science, but by major analytic philosophers. I show, first, that it was ignored or snubbed and passed over in philosophical anthologies or textbooks. Secondly, I discuss how *The Structure of Scientific Revolutions*, nevertheless, impacted philosophical development. Finally, I offer an explanation of Kuhn's marginalization in analytic philosophy that goes beyond the prevalent view which says that he was marginalized because he was not a serious philosopher, and was advocating the dangerous doctrines of relativism, irrationalism and anti-realism. I argue that philosophers failed to seriously engage with Kuhn's work because they were not ready to appreciate its revolutionary character. This is a situation that Kuhn describes in relation to scientific revolutions. Initially, revolutionary ideas seem unintelligible, irrational or wrong and are resisted and cast aside. But, eventually, if they break new ground carrying a promise of success, they catch on. I argue that Kuhn's philosophy had a similar reception. It was initially resisted, but it eventually succeeded in impacting a great many areas of thought, including analytic philosophy.

Keywords Analytic philosophy · Incommensurability · Relativism · Thomas Nagel · Hilary Putnam · Bernard Williams · Richard Rorty

Thomas Kuhn's impact on a wide range of disciplines and areas of thought has been widely acknowledged and variously assessed. It has been particularly discussed in relation to philosophy of science (see, e.g., Wray 2012), the history of science (e.g., Daston 2016), and the social studies of science in all the forms they took (e.g., Sociology of Scientific Knowledge or Science and Technology Studies; see, for instance, Golinski 2005/1998), but it has also been taken up in relation to other

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fields, such as feminist studies (e.g., Longino 2003), history (e.g., Hollinger 1973), anthropology (e.g., Tresch 2001), psychology (e.g., O'Donohue 1993), or science education (e.g., Matthews 2004). Kuhn's impact on philosophy,[1] however, has attracted much less attention, mostly because Kuhn was not taken seriously as a philosopher by philosophers.[2] In his autobiographical interview, Kuhn attests to that:

> …people treated me as though I were a fool! I want to say how the hell could anybody ever have thought that I would believe anything like that! That was fairly destructive, and I fairly early simply stopped reading the things about me, from philosophers in particular. (RSS 315)

Philosophers of science criticized Kuhn's work harshly for relativism, irrationalism, or anti-realism, but even when they misinterpreted it, they engaged with it. Philosophers, however, did not care to attend to it.[3] In the present chapter, I will, first, show that Kuhn's work was indeed ignored or snubbed by major philosophers and passed over in philosophical anthologies or textbooks. Secondly, I will discuss how *The Structure of Scientific Revolutions*, nevertheless, impacted philosophical development. I will give particular examples of philosophical work that has been affected by Kuhn's philosophy, even as mostly a reaction to the threat that it presumably posed. In that sense, it may be said that there was a latent conversation between Kuhn and the philosophers. Finally, I will consider why Kuhn was not thought to be worth discussing in relation to philosophy. Obviously, for the dangers he was thought to be presenting. Philosophers assumed a caricature of his model that was carried over from philosophy of science, and pushed it to the margins as destructive and dangerous. Still, I would like to put forward another explanation as well. My main contention will be that philosophers failed to engage with the details of Kuhn's work because they were not ready to appreciate its revolutionary character. This is a situation that Kuhn described in relation to scientific revolutions. Initially, revolutionary ideas seem unintelligible, irrational or wrong and are, naturally, resisted and cast aside. But, eventually, if they are fruitful and carry a promise of success, they catch on. I will argue that Kuhn's philosophy had a similar reception. It was initially and overtly resisted and sidestepped, but it eventually prevailed as it succeeded in transforming our idea of science. Yet, he is still not getting the credit.

[1] Reference to philosophy in the present paper is confined to reference to English speaking philosophy or what is called analytic philosophy.

[2] "[T]here was a residual sense [among philosophers] that Kuhn wasn't a proper philosopher at all." (Shapin 2023). The same thought is expressed by Rorty. He said that his fellow philosophers withheld from Kuhn the honorific title "philosopher," preferring to refer to him as a historian of science. "Kuhn was constantly being treated … as at best a second-rate citizen of the philosophical community. Sometimes he was even treated as an intruder who had no business attempting to contribute to a discipline in which he was untrained" (Rorty 1999, 175).

[3] Philosophers of science are, of course, philosophers. Here I am contrasting specifically philosophers of science, who dealt directly with Kuhn's work and criticized it, with philosophers at large, i.e., philosophers that do not specialize in philosophy of science, who did not care to seriously consider it, but only to dismiss it.

10.1 Kuhn's Work Was Sidelined in Philosophy

In this section I will give examples that show Kuhn's work was neglected in philosophy.

Thomas Nagel, in the introduction to his book *Other Minds* (1995), which bears the title "The Philosophical Culture," discusses how quality in analytic philosophy does not depend on public support, but is evaluated technically by the academic community. In that context, he remarks:

> Analytic philosophy has escaped almost completely the facile relativism that seems to be so influential in the humanities, originally stirred up by Derrida, and now defended by references to Richard Rorty, Paul Feyerabend, and Thomas Kuhn. Philosophy seems to export its worst products, as happened earlier in the century with behaviorism and logical positivism and their influence on the social sciences and psychology. (ibid., 9)

Of the philosophers Nagel mentions in the above passage, Kuhn is the only one that denies endorsing relativism: "My critics respond to my views … with charges of irrationality, relativism and the defense of mob rule. These are all labels that I categorically reject, even when they are used in my defense by Feyerabend." (RSS 100; cf. RSS 307) And yet, despite Kuhn's protestations, and without any further argument, Nagel assimilates Kuhn's work to "facile relativism," and insinuates that it is one of philosophy's worst products which will have the fate of other failed philosophies that have been expelled to the social sciences which are eager to embrace loose and low-quality doctrines. He really does not mince his words. Nagel proceeds to say that the way he chooses to deal with "debased philosophy," which becomes influential in other fields, is to hope that "the current wave of confusion will subside if we just *ignore* it" (Nagel 1995, 9, my emphasis).

Nagel's dismissive arrogance which led to the injunction to ignore the facile relativists is characteristic of a more general attitude towards Kuhn. Larry Laudan bears witness to this attitude when he writes in his book *Science and Relativism* (1990, viii–ix) that his

> fellow theorists of science … are persuaded that epistemic relativism is just one of those episodic cultural sillinesses that will wither and die of its own accord. They seem to think that if one either *ignores* the Kuhns and Feyerabends, or dismisses them with a quick reductio, it will not be long before the situation puts itself right. (my emphasis)

Laudan disagrees that the proper attitude towards the corrupters of rationality, and of all that is valued in epistemology,—Kuhn being one of the most notable among them—, is to ignore them, and offers his book as "a purgative and a prophylactic" against the wiles of incommensurability and relativism (ibid., x). And in a magnanimous spirit, he tries to strengthen the relativist position in the dialogue that he sets up between a positivist, a realist, a relativist, and a pragmatist, so as not to win a cheap victory. The relativist literature, Laudan tells us, was in a "sorry state" and was not of much help in his effort to make the relativist that he painted "clever and argumentative adept" (ibid. xi).

Hilary Putnam, on his part, in his famous paper "The Meaning of Meaning (1975, 153), says that he wishes to contrast his view on meaning "with one which is popular, at least among students (it appears to arise spontaneously)". Which view is that?

It is the anti-realist view that takes truth to be an intra-theoretical notion, a view that is skeptical of convergence in science (i.e., of getting better descriptions of theory-independent entities), a view that challenges the idea that the extension of a term can be fixed extra-theoretically. Kuhn has endorsed or has been taken to have endorsed all these anti-realist claims, but his name is nowhere to be found in the text. Not only that; his views are disparagingly attributed to ignorant students who spontaneously, i.e., crudely and thoughtlessly, make statements like that.[4]

Bernard Williams, in his essay titled "The Truth in Relativism," (1981) which was published for the first time in 1974, discusses, among other issues, shifts in scientific paradigms, incommensurability and conversion, but again Kuhn is not mentioned by name. Theory-ladenness of observation is said to be part of "a fashionable line of argument" while it is alleged that the change of paradigm may be the result of boredom (ibid., 134–135). Philosophers of the caliber of Bernard Williams, Thomas Nagel, and Hilary Putnam, did not want to dignify Kuhn by engaging directly with him.[5]

In the philosophy of science literature, Kuhn cannot be ignored. As Alan Richardson puts it (2010, 363), *Structure* "became the most famous and influential work in philosophy of science in the second half of the twentieth century." Its approach and the problems it posed, "fundamentally altered the field's sense of its central problems and methods" (ibid.). Also, the use of Kuhn's concepts is now almost ubiquitous, his once radical ideas, now commonsensical. Yet, his work is still marginalized. It is mostly discussed as a matter of history. Excerpts from his work are anthologized in textbooks, in order that students get acquainted with the relevant terminology (paradigm, and paradigm shift, incommensurability, normal science, crisis, revolution) so that, then, they learn to dismiss him as a relativist and anti-realist. At best, one can come across papers that try to mend Kuhn's faulty account, most often by proposing ways to overcome lacunae of rationality.[6]

In philosophy of the analytic tradition, however, the picture is much worse. Kuhn is hardly ever mentioned in handbooks or anthologies. Most references to his work are incidental and, if he is ever briefly discussed, he is most often to be criticized. In *Analytic Philosophy. An Interpretive History* (Preston 2017), in *Philosophical*

[4] Later, in his book *Reason Truth and History* (1981), Putnam both acknowledges Kuhn's help in writing his book (ibid., xii), and criticizes him by name (ibid., 113–126). However, he still finds the more relativistic readings of Kuhn's work "sophomoric," (ibid., 113), recognizing at the same time that Kuhn became increasingly more moderate in his claims.

[5] Nagel, in his review of Alan Sokal and Jean Bricmont's book *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science*, mentions Kuhn and Feyerabend, since they are discussed in the book, and sternly states that they both "have a lot to answer for" (Nagel 2002, 170). Still, in the same review, Nagel has come to accept that certain Kuhnian ideas, such as, the theoretical shifts, are reasonable despite the fact that he still accuses Kuhn of radical relativism and bad philosophy (ibid., 169–170).

[6] Michael Friedman (2001) and Bas van Fraassen (2002), for instance, take Kuhn's work seriously and try to address what they take to be Kuhn's challenge to rationality. But they both think that Kuhn's account is somehow defective; so, they undertake the task to vindicate, in their different ways, the rationality of scientific development. For more on this see Kindi (2017).

Analysis in the Twentieth Century (Soames 2009/2003), and in *Analytic Philosophy. An Anthology* (Martinich and Sosa 2001a), there is no mention of Kuhn. In *The Oxford Handbook of the History of Analytic Philosophy* (Beaney 2013), Kuhn's name is only included in the chronological table of 150 philosophers, with his book, *The Structure of Scientific Revolutions,* and key events of his life (e.g., his move to Berkeley and Princeton, his birth and death) cited in another list with publications and events in the history of analytic philosophy. In the rest of the volume which is over 1000 pages long, Kuhn's name is dropped incidentally just twice. In *The Oxford Handbook of Contemporary Philosophy* (Jackson and Smith 2005), there are only 4 incidental mentions of Kuhn, less than the references to Steven Kuhn, a rather unknown figure in philosophy. In *The Oxford Handbook of American Philosophy* (Misak 2010), where there are chapters on the work of Sellars, Rorty, Quine and Davidson, Alan Richardson is the only one who briefly discusses Kuhn in a few sentences in his chapter on American philosophy of science in the 1950s and 1960s. *The Blackwell Guide to American Philosophy* (Marsoobian and Ryder 2004) devotes particular chapters to philosophers such as C.I. Lewis, W.v.O. Quine, Susanne Langer, Alain Locke, Justus Buchler, but not to Kuhn. Four of the five mentions of Kuhn's name in this volume are in notes, three in the essay on Quine and one on education. The only mention of Kuhn's name in the text is along with the names of Martin Heidegger, Ludwig Wittgenstein, and Donald Davidson in the chapter on the First Pragmatists, where it is said that the term 'pragmatist' is problematically applied to these figures. In *The Story of Analytic Philosophy* (Bilettzki and Matar 1998), Kuhn is considered one of the culprits who may have killed analytic philosophy. He is criticized in a less than a page long comment by Jaakko Hintikka who accuses Kuhn for "a singularly shallow level of awareness of what these notions [incommensurability and theory-ladenness] involve" and for reducing philosophy of science to history and sociology of science. In his view, this move "would not only put an end to the genuinely philosophical attempts to understand science but it would impoverish immeasurably the history and sociology of science. And it would impoverish our understanding of science itself." (Hintikka 1998, 267).

There is some limited discussion of Kuhn in the following books: In *The Bloomsbury Companion to Analytic Philosophy* (Dainton and Robinson 2014), James Ladyman discusses Kuhn briefly in the chapter "Philosophy of Science". Kuhn also appears in the timeline of individual philosophers as *Structure* also does in the Chronology. The book *A Brief History of Analytic Philosophy* (Schwartz 2012) lists Kuhn among the fifty-four "Leading Analytic Philosophers" and devotes a subchapter (of less than three pages), to his work where it is said that "the view of science that emerges from Kuhn's historical work is close to Quine's and supports his criticisms of the logical positivists." (ibid., 91) In *A History of Philosophy in America* (Kuklick 2001), Kuhn is discussed, along with Marcuse, Rawls, Putnam, and Rorty, in a chapter entitled "The Tribulations of Professional Philosophy 1962–1999". In *A Companion to Analytic Philosophy* (Martinich and Sosa 2001b), thirty-nine philosophers are discussed and one of them is Kuhn. The author of the entry on Kuhn is Richard Grady. Thomas Baldwin's *Contemporary Philosophy.*

Philosophy in English since 1945 (2001) devotes a chapter to "The Scientific Paradigm" in which the Kuhnian revolution and incommensurability are discussed. Giovanna Borradori includes Kuhn in the interviews she conducted with American philosophers (1994/1991), and John Rajchman and Cornel West anthologize Kuhn's paper "Mathematical versus Experimental Traditions in the Development of Physical Science" in their volume *Post-Analytic Philosophy* (1985), together with Ian Hacking's "Styles of Scientific Reasoning," and Donald Davidson's "On the Very Idea of a Conceptual Scheme" in the chapter entitled "Science".

Moving from collective works to particular philosophers, we find Rorty, who appreciates Kuhn's work and cites it repeatedly. He lists Kuhn with Kripke and Rawls as "three important contemporary philosophers" (1982, 217). John Searle (1993), on the other hand, thought that Kuhn, along with Rorty and Derrida, the postmodernists and the "Nietzscheanized Left," posed a threat to the Western Rationalist tradition, but noted that, unlike what happened in the Humanities, "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 (Searle 1993, 71).

Searle's negative attitude towards Kuhn's work did not prevent him, however, from acknowledging its significance. In his article on "Contemporary Philosophy in the United States" in *The Blackwell Companion to Philosophy*, published in 1996, he includes "the changes in the philosophy of science due to Kuhn and others" in "the important theoretical developments" that took place in the 1950s and 1960s, along with "Quine's rejection of the analytic–synthetic distinction, Austin's theory of speech acts, Wittgenstein's criticism of foundationalism, [and] Rawls's work in political philosophy" (Searle 1996, 6). A similar claim is made by Bruce Kuklick (2001, 270): "It [*Structure*] was the most influential philosophical volume of the last third of the century, and Kuhn himself became, as one historian put it,[7] 'one of the most widely discussed academic intellectuals of the century'." Kuklick (2006, 179) castigates also the philosophers at Berkeley who did not agree to give Kuhn tenure at the Philosophy Department there because they thought that "he [Kuhn] had few pretensions to being a philosopher, and that his history of science had little connection to philosophy." Kuklick calls this Berkeley decision "arguably the worst in the American academy in the twentieth century" (ibid. 180).

The above discussion aims to illustrate the marginalization of Kuhn and his work in Anglophone philosophy.[8] Important philosophers, such as Nagel, Williams, or

[7] Kuklick explains in his (2006, 184n30) that this judgment is made by the historian David Hollinger (1998, 43).

[8] A credible objection, which has been raised by my colleague Theodore Arabatzis and an anonymous reviewer, would say that several books and articles have been published on Kuhn's work since the early 1990s that haven't been published on the work of other important philosophers. And what is more, quite a few of those are positively inclined towards it. So, Kuhn's work is far from being ignored. It is true that things have somewhat changed in recent times. Kuhn's work attracts more serious attention. Monographs, edited volumes, and papers have been published; workshops and conferences have been held to celebrate *Structure's* 50th and 60th anniversaries and Kuhn's centenary. But, still, they have been written and organized by philosophers of science, not philoso-

Putnam, avoided to engage seriously with Kuhn's work. They preferred to dismiss it out of hand based on an impressionistic take that was shaped by the early reproving verdict issued in philosophy of science where Kuhn was accused of committing the major philosophical sins of relativism and irrationalism, but also of having made egregious mistakes, such as sloppy argumentation, inconsistencies, and equivocations. In short, Kuhn was not thought worth addressing as a *bona fide* philosopher. Similarly, most of the anthologies of Analytic or American philosophy either ignore him or discuss him cursorily and by using calcified clichés. Now, given that analytic philosophy modeled itself after science,[9] it is puzzling that a change in the conception of science brought about by Kuhn's work, does not register as having significant import in the literature of analytic philosophy. In what follows, I will try to explain why Kuhn's work has been largely ignored in philosophy. But first, I will argue that, despite the condescending arrogance by which it was treated, it exerted significant influence on philosophical developments. I will, first, discuss *Structure*'s influence on philosophy in general, and will, then, proceed to offer my explanation of why it has been largely ignored.

10.2 How Was *Structure* Influential in Philosophy?

Structure influenced developments and particular ideas in analytic philosophy through the reaction it provoked, given its stereotypical interpretation, but also, more positively, by providing inspiration for changes similar to those in philosophy of science. I am not claiming that Kuhn's was the only influence on these developments, nor do I suggest that it is the decisive one. I am claiming that there is a connection to Kuhn's philosophy that is usually overlooked. I will start with the developments that tried to respond to the presumed threats that Kuhn's work posed and then present the more positive reception of *Structure*.

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phers *tout court,* and do not reflect some mainstream interest. A glance at the programs of philosophy conferences can reveal how marginal, if not completely absent, Kuhn's work still is. I also want to argue that Kuhn's neglect is not so much reflected in the number of publications that discuss his theses, but in the lack of serious engagement with his work. Critics and advocates alike, operate, in most cases, with a stereotype that does not do justice to what Kuhn did. The problem is not that Kuhn's work is criticized; criticism can signify serious engagement. The problem is that most of Kuhn's critics try to correct what they take to be Kuhn's thoughtless mistakes, to redress the damaging implications of his views, without making the effort to look past the stereotype that generates them. Even when they acknowledge that Kuhn is not as radical as he is usually portrayed (see Putnam and Nagel above, notes 1 and 2), they don't care to engage with his actual and more refined views, but prefer to concentrate on renouncing the "sophomoric" versions.

[9] Cf. Rorty (2006, 86): "Analytic philosophy was from the very beginning conceived as a project that would make philosophy into a science."

10.2.1 The Causal Theory of Reference

Both Saul Kripke and Putnam are credited with the causal theory of reference, i.e., the view that the reference of proper names and of natural kind terms, is not fixed by description, but by an initial baptism, an act of dubbing. Then, the name, together with its reference, is passed on to subsequent users of language by a chain of causal links. Kripke developed his account motivated by his interest in questions of modal logic. Putnam, on the other hand, was motivated by his desire, among others, to respond to views aired in philosophy of science in the 1960s which defended meaning change of scientific terms that also involved change of reference. Putnam wanted to provide a line of continuity and stability, i.e., stability of reference, despite meaning change even if radical. Helen Beebee and Nigel Sabbarton-Leary observe that "… some philosophers of science take a Kripke-Putnam-style causal theory of reference to defeat Kuhnian relativism" (2010,1), and write that "[Putnam's] prime concern was to ward off the threat of Kuhnian incommensurability" (2010. 13). An analogous comment is made by Ian Hacking:

> Although his [Putnam's] concern with analyticity is one of his starting points, he was also spurred on by philosophy of science current in the early 1960s. Against Paul Feyerabend, but also against Norman Malcolm's *Dreaming* of 1959, he urged meaning-constancy: we may learn radical new things about acids or dreams, but we are still talking about acids and dreams. The notion, that reference is what anchors meaning, provides a theory to make sense of meaning constancy. (Hacking 2007, 6)

Putnam himself, in the Preface to his *Reason, Truth, and History* (1981), names Kuhn ("in some of his moods at least") and Feyerabend as the philosophers against whom he would argue in his book.[10]

10.2.2 Scientific Realism

Philosophers of science, in the first half of the twentieth century, under the influence of the logical positivists who rejected metaphysics as nonsensical, at least initially,[11] concentrated on logical and epistemological questions and were reluctant to engage with metaphysical issues. But when Kuhn suggested that ontology may shift with paradigm change, that scientists after a change of paradigm may live in a different world, and that observation is theory-laden through and through, he was accused of idealism and both hailed and deplored for constructivism. Kuhn's account had also challenged the highly entrenched idea, among scientists and philosophers alike, that science progresses cumulatively, trying with its theories to approximate, as

[10] I and Theodore Arabatzis also thought that the causal theory of reference was responding to Kuhn's work (Arabatzis and Kindi 2013/2008).

[11] See Carnap's later work, for instance, his "Empiricism, Semantics, and Ontology" (1950), where he tackles questions of existence and reality, especially for abstract entities, through language.

faithfully as possible, a mind-independent world as it truly is.[12] The fear of shifting and constructed ontology prompted philosophers to defend realism in science. Richard Boyd, one of the earliest and most prominent scientific realists, in the entry he wrote on scientific realism in the *Stanford Encyclopedia of Philosophy* (2002), contends that it is

> conceptually useful (and approximately historically correct) to see the development of scientific realism as a response to… [among others]. The Neo-Kantian Challenge, First Version: This is the challenge raised by Hanson (1958) and Kuhn (1970) who argue from the theory dependence of methods (and, especially, of observation) to the conclusion that a realist conception of the growth of approximate scientific knowledge cannot be sustained, given the semantic and methodological incommensurability (Kuhn's term) occasioned by revolutionary changes in science. (Boyd 2002)[13]

A similar point is made by another notable scientific realist, C. A. Hooker, who said that his essay, "Systematic Realism," first published in 1974, and reprinted in Hooker's book *A Realistic Theory of Science* (1987), "is written with this new approach [Feyerabend and Kuhn's] explicitly in mind" (Hooker 1987, 10). The increased interest in realism in philosophy of science spread to other fields of philosophy as well.

10.2.3 The Absolute Conception

Williams, in his book *Descartes: The Project of Pure Inquiry* (1978, 66), describes Descartes' effort "to extract an absolute conception of reality from the process of Pure Inquiry" and embraces this aspiration himself. He expects that science will provide this ultimate and absolute description. Williams says that the description of that reality would be like Peirce's "final opinion" upon which scientific enquiry will converge. This final opinion would not be independent of thought in general but, in Peirce's words, "independent of all that is arbitrary and individual in thought" (Williams 1987, 244). Then, Williams turns to those who think that the notion of convergent scientific progress is a myth, and says that they have to explain the intercultural success of scientific theories (ibid., 248). He does not name them, but it is pretty obvious that he has philosophers of science, such as Kuhn, in mind. In Wililams' s view, the relativistic outlook, "must … acquire from somewhere a stable

[12] Cf. Chakravartty (2017): "On [Kuhn's] picture, empirical reality is structured by scientific paradigms, and this conflicts with the commitment of realism to knowledge of a mind-independent world."

[13] Boyd mentions three more challenges that realism had to meet: the empiricist challenge that relates to the underdetermination thesis, the second version of the Neo-Kantian challenge which was criticizing metaphysical realism (e.g., Putnam's "internal realism"), and the challenge posed by the science studies constructivist literature (Boyd 2002). Of these, the last two also respond, at least partly, to Kuhn's work. Putnam's internal realism, for example, owes much to Kuhn's challenge while the science studies constructivism was drawing on Kuhn's work.

conception of the world of nature, in relation to which it can understand cultural phenomena such as science and its own view of science." (ibid.)

It seems that Williams is suggesting that, for the relativistic outlook to even make sense, the absolute conception of reality needs to be presupposed. And essential to the idea of absolute conception is the realist outlook (ibid., 301), which requires that there is an external world that is unaffected by our thinking and which causes our beliefs. This realist outlook, says Williams, "does not demand the simple positivist 'fact-copying' picture of the scientific process, nor a simply linear conception of scientific progress, against which contemporary anti-realist views are often an exaggerated reaction." (ibid., 249). So, Williams seems to accept the validity of some, at least, claims made by the so-called "antirealist" philosophers of science, namely, that scientific progress may not be steady and unencumbered, or that science does not aim to simply copy the world in its theories, but he still thinks that the anti-realists err when they do not see the necessity and the indispensability of the absolute conception and of the realist outlook that goes with it.[14]

Nagel, in his *The View from Nowhere* (1986) continues, in a sense, the project of the absolute conception and inquiry (cf. ibid., 70). He wants to provide an account of the impersonal standpoint that views the world objectively and, at the same time, include in it the subjective point of view. In his effort to explain that there is something irreducibly subjective in the world that cannot be dealt with by using the concepts of physical science, Nagel invokes revolutions in science where the vocabulary of the rejected theory cannot be used to express new phenomena. Analogously, the vocabulary of physics, he thought, cannot be used to analyze and explain mental phenomena. The universe of Maxwell, for example, says Nagel, cannot be put in the concepts of Newton's universe. He also mentions Einstein's revolution and notes that there would not be progress in science had everyone insisted to use the already available concepts to account for new phenomena (ibid., 52). This is very much the line of thought that Kuhn developed in *Structure* in order to talk of the *necessity* of scientific revolutions,[15] but Nagel does not cite him. Neither does he cite him for the

[14] Notably, Putnam, who devotes a chapter of his book *Renewing Philosophy* (1992) to Williams's absolute conception of the world, points out that Williams expresses reservations about the ultimate metaphysics that science is expected to provide (Putnam 1992, 35).

[15] Kuhn says in *Structure* that if we do not recognize radical changes in science, changes that require the rejection of a paradigm, if, that is, we preserve theories as special cases of subsequent ones preserving the same vocabulary, then science would stop.

> But to save theories in this way, their range of application must be
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> 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. (SSR 100)

For more on this see Kindi (2005).

incommensurability between successive theories or paradigms. Instead, Nagel cites P.M. Harman (1982) for developments in nineteenth-century physics (ibid.).

10.2.4 Conceptual Schemes

Conceptual schemes were discussed by William James in his *Some Problems of Philosophy* (1996) that he began to write in 1909, and by a number of scholars at Harvard, linked to the so-called 'Pareto Circle' in the first half of the twentieth century.[16] Peter Galison writes that "the conceptual scheme rapidly became such a pervasive idea in anglophone philosophy of science that it is hard to find a programmatic statement about science in the 1950s or 1960s without it." And yet, it was the threat of relativism that was thought to be posed by Kuhn's work which prompted Donald Davidson to write his paper "On the Very Idea of a Conceptual Scheme" (1984), which he delivered as a Presidential Address to the Eastern Meeting of the American Philosophical Association in 1973. In that paper Davidson distinguishes between two ways of talking about conceptual schemes, Strawson's, where a fixed set of concepts describes alternative conceptual schemes, and Kuhn's, where incommensurable schemes of concepts account for the same world. Davidson says that, in his article, he will be concerned with the Kuhnian version. Davidson's paper discusses the so-called "third dogma of empiricism," i.e., the scheme-content distinction, which is mostly associated with Quine in the twentieth century, but besides him, Davidson also targets Kuhn. Davidson says that "we are encouraged to imagine massive conceptual change", that "there may be no translating from one scheme to another" (1984, 183),—both Kuhnian theses—, and cites Kuhn's view that "scientists operating in different traditions (within different 'paradigms') 'work in different worlds'" (ibid., 186–187). Davidson calls Kuhn a historian of science,[17] and wrongly credits him with the idea that "we get a new out of an old scheme when the speakers of a language come to accept as true an important range of sentences they previously took to be false (and of course vice versa)" (ibid.,188). He also incorrectly attributes to Kuhn the idea that there is some neutral content that awaits to be organized by the different conceptual schemes. Kuhn, however, did not endorse the relativistic idea about truth attributed to him. He believed that that what varies with language is not truth but effability (RSS 99–104; 1993, 330, 1999, 35). He also believed that knowledge of words and knowledge of nature "are acquired together, not really two sorts of knowledge at all, but two faces of the single coinage that a

[16] These scholars included, among others, V. Pareto, L.H. Henderson, Talcott Parsons, W.V.O. Quine, James Bryant Conant and Thomas Kuhn. For more on this group of academics and conceptual schemes, see Heyl (1968); Quine (1981, 41); Galison (1997, 787–790); Turner (2008, 41); Isaac (2012).

[17] Davidson distinguishes Putnam and Feyerabend as philosophers of science, from "the historians of science, like Kuhn" (1984, 188).

language provides" (RSS 31). As for the scheme/content distinction, Kuhn denies it when he writes in a letter to Rorty (Sept. 19, 1986) that

> [i]f the world really were 'out there,' it couldn't be true to say that it changes. But if it's something produced by the interaction of people and noumena ["the ineffable causes of our sensations"],—if that is we participate in making it—then I don't know why you'd be expect it to be stable. (Kuhn MC 240, Box 22)

Davidson wanted to address the challenge of relativism that he thought the concept of conceptual scheme posed, and proceeded to show that the very idea of a conceptual scheme is incoherent.[18] After Davidson, the problems relating to conceptual schemes, some of which Davidson dealt with, have drawn the attention of scholars for decades. At the same time, the emphasis laid on *conceptual* schemes played down the practical aspect of Kuhn's paradigms and contributed to its neglect. Kuhn had used the expression 'conceptual scheme' in his *The Copernican Revolution* (1957), most probably influence by James B. Conant who used the term repeatedly, but in *Structure* he gave prominence to seeing science as an activity rather than as a set of statement involving concepts.

10.2.5 Novelty and Revolutions in the Arts

Kuhn's work, both explicitly and implicitly, has influenced discussions in aesthetics and, more particularly, discussions pertaining to Modernism in a positive way. Michael North, in his book on novelty, writes (2013, 116): "Because of its direct and indirect influence over certain art critics in particular, *The Structure of Scientific Revolutions* had a very powerful role in shaping midcentury theories of modernism in the arts." The art critics that North is referring to, who are also philosophers, are Michael Fried, Arthur Danto, and Stanley Cavell.[19] Fried mentioned Kuhn in a paper of his on Frank Stella, published first in 1966 (Fried 1998, 99n11). Danto's celebrated paper "The Artworld" (1964), does not mention Kuhn, but later Danto himself has said to Caroline Jones (2000, 501): "I think I must have been deeply influenced by Kuhn's idea. [Although] we were very uneasy about his relativism."[20] In fact, even though there is no explicit mention of Kuhn in the article, there is a

[18] Davidson did not confine his criticism to Kuhn's work. His targets, besides Kuhn and Quine, also included Feyerabend, Putnam, Whorf and Sapir. There are numerous papers that discuss and criticize Davidson's essay on conceptual schemes. See for instance, Henderson (2013). Both Quine and Putnam have responded to Davidson (Quine 1981; Putnam 1987).

[19] North also mentions the art critic Clement Greenberg whose work has great affinities to Kuhn's (see Kindi 2010). Yet, I do not include him here since his work precedes *Structure*.

[20] Cf. Jones's judgment on the influence of Kuhn on Danto: "Danto's "The Artworld" thus paralleled the sense of science Kuhn aimed to produce (without then citing Kuhn or acknowledging his importance)." (Jones 2000, 501) Jones also reports that Danto acknowledges both Kuhn and Ernst Gombrich as "two great influences on [his] work" in the introduction to a translation of "The Artworld" (ibid.).

comparison of the history of art to the history of science in relation to conceptual revolutions (Danto 1964, 573), and talk of incompatibility between artistic identifications of objects or worlds (ibid., 577–578). Actually, in discussing this latter point, Danto gives the example of two rectangles, A and B, divided in two by a line, which are supposed to be two paintings to be respectively called Newton's First Law and Newton's Third Law (ibid., 577). They are exactly the same, indiscernible, but they are identified differently, that is, they are explained differently by the artists that created them. For example, one may take the middle line to be an edge, some other may take the entire area as a single space with no edge (ibid., 578). The discussion of this example is reminiscent of Kuhn's discussion of the duck-rabbit figure that does not change and yet, it is identified differently either as duck or as rabbit. North (2013, 145) also notes that "Danto calls on world to do much the same duty done in Kuhn's work by worldview or, more famously, by paradigm."

Kuhn's influence on Cavell as regards aesthetics is more profound and explicit. Cavell and Kuhn were colleagues and close friends, at Berkeley in the late 50 s and early 60 s. Both acknowledge the significant influence of each other in their books that were the product of that time, namely in Kuhn's *Structure* (SSR xlv–xlvi) and Cavell's *Must We Mean What We Say?* (1969, xii) and *The Claim of Reason* (1979, xix). Cavell has said that it was all [him] learning from [Kuhn], about the history of science, about history as such, especially in relation to ideas" (Conant 1989, 41). What interested Cavell most, were revolutions and reconceptualizations in art and he made the comparison to how Kuhn viewed revolutions in science. In *The Claim of Reason,* he writes:

> … only a slave of [convention] can know how it may be changed for the better, or know why it should be eradicated. Only masters of a game, perfect slaves to that project, are in a position to establish conventions which better serve its essence. This is why deep revolutionary changes can result from attempts to conserve a project, to take it back to its idea, keep it in touch with its history. […] It is because certain human beings crave the conservation of their art that they seek to discover how, under altered circumstances, paintings and pieces of music can still be made, and hence revolutionize their art beyond the recognition of many. This is how in my illiteracy, I read Thomas Kuhn's *The Structure of Scientific Revolutions*: that only a master of the science can accept a revolutionary change as a natural extension of that science; and that he accepts it, or proposes it, in order to maintain touch with the idea of that science, with its internal canons of comprehensibility and comprehensiveness, as if against the vision that, under altered circumstance, the normal progress of explanation and exception no longer seem to him to be science. And then what he does may not seem scientific to the old master. (Cavell 1979, 120–121)

Cavell's, Fried's, and Danto's works have had a lasting significance in the appraisal of modernism and of art in general. Kuhn's revolutions have had a continuous influence on aesthetics, not just through these philosophers' contributions, but also in works such as Remi Clignet's *The Structure of Artistic Revolutions* (1985), Gianni Vattimo's "The Structure of Artistic Revolutions" (2014/1985), Natalie Heinich's *Le paradigme de l'art contemporain: Structures d'une revolution artistique* (2014), or Oana Șerban's *After Thomas Kuhn: The Structure of Aesthetic Revolutions* (2023).

10.2.6 History of Philosophy

The impact of Kuhn's work on the history of philosophy is another case of positive influence. Several historians of philosophy have noted how Kuhn's work has induced changes in the history of philosophy. Dan Garber, in his article on the relevance of an antiquarian history of philosophy to analytic philosophers, defends his preference for "a genuinely historical history of philosophy (2005, 129). He ends this article by paraphrasing Kuhn's opening line in *Structure*: "History of philosophy, if viewed as a repository for more than assorted arguments and errors, could produce a decisive transformation in the image of philosophy by which we are now possessed." (ibid., 146). In an earlier text, Garber (2004) recounts how developments in the history of science in the 1980s and 1990s, had influenced the way history of philosophy was practiced. The focus had shifted from the rational reconstruction of views to the views themselves and their social background while anachronism emerged as a major problem in both history of science and history of philosophy.

Catherine Wilson expresses a similar view about Kuhn's impact on the history of philosophy. She says that "Kuhn's book probably accomplished nearly as much for the history of philosophy as it did for the history of science in the anglophone world" (Wilson 2005, 72). Michael Beaney, in his article on "The Historiography of Analytic Philosophy" (2013), also notes that debates in the history and philosophy of science, "inspired, in particular, by Kuhn's paradigm-shifting work of 1962, *The Structure of Scientific Revolutions*" encouraged "deeper reflection about methodology" in the history of philosophy. According to Beaney, analytic philosophers had "an impoverished understanding … of their own history and of historiographical issues" (ibid., 53) which, Beaney implies, was enriched by developments in the history and philosophy of science.

Views similar to the above are also expressed in Michael Frede's book *The Historiography of Philosophy* (2022) that was published posthumously and was based on Frede's Nellie Wallace Lectures at Oxford in 1990. Frede does not mention Kuhn by name, nor does he mention incommensurability, but many of the themes he addresses respond to problems related to this concept, namely, lack of understanding and untranslatability of past views.[21] Just like Garber, Frede is defending a historical history of philosophy (as opposed to a doxographical and philosophical history of philosophy), i.e., a non- anachronistic history of philosophy that is supposed "to give us a philosophically disinterested reconstruction of the actual history of philosophy" (ibid., 43). He notes how reflection on the history of science has effected "enormous changes" in its historiography (ibid., 5), unlike what was happening up to that time in philosophy and its own historiography.

[21] For an extensive presentation of Frede's discussion of issues pertaining to incommensurability in this book as well as the affinities between Kuhn's take and Frede's, see Kindi (2022).

10.2.7 Value Incommensurability

Also positive was the reception of Kuhn's work in ethics and philosophy of law in relation to the subject of value incommensurability. Although the term 'incommensurability' had been used before Kuhn and Feyerabend—and not just in relation to ancient Greek mathematics—, these two philosophers are the ones who brought it to the fore in philosophy of science. Since then, it has become a matter of debate because of its presumed undesirable consequences, e.g., incomparability, between incommensurable magnitudes. Kuhn's notion of incommensurability, which was less restrictive than Feyerabend's, did not apply only to concepts, but extended to other elements of the tradition that is governed by a paradigm. Values feature prominently in the constellation of commitments that members of a scientific community share and one expects that, not only concepts, but also values may change radically when paradigms shift. Value incommensurability is not so much discussed in relation to science, since it has been assumed, even by Kuhn, that scientists share transparadigmatic epistemic values which, nonetheless, may be applied differently by individual scientists. However, in ethics and philosophy of law, value incommensurability has become a key issue that is broadly debated because it concerns comparing goods and balancing rights. Important scholars, such as Joseph Raz, Cass Sunstein, Jeremy Waldron, Frederick Shauer, Bernard Williams, Ruth Chang, and many others, have discussed different accounts and aspects of incommensurability as well as its ramifications in different areas. The literature is massive and cannot be reproduced here but one can get a picture of the debates from Chang (1997) and Hsieh and Andersson (2021).

10.2.8 Rorty

Rorty is perhaps the only philosopher who explicitly advocated Kuhn's work and seriously engaged with it in his own. He calls Kuhn a hero in his influential book *Philosophy and the Mirror of Nature* (1979, 288, 382), devotes a subchapter in it to "Kuhn and incommensurability" (ibid., 322–333), but also cites Kuhn abundantly throughout this and other books of his. Rorty mentions Kuhn, along with Wittgenstein, Heidegger, Dewey, Sellars, Quine, Davidson, Ryle, Malcolm, Feyerabend, and Putnam, as one of those whose work he borrowed from in order to develop his own anti-foundationalist and anti-representationalist philosophy (ibid., 7, 288), but he criticized him for "incidental idealism" (ibid.325). He did not like Kuhn's talk of "different worlds" (ibid., 324–325, 345). In a letter to Kuhn (October 20, 1986, Kuhn MC 240, Box 22), Rorty acknowledged that they "never seem to meet head-on in discussion," and asked Kuhn why he does not take him [Rorty] and Foucault "as good Kuhnians". Kuhn, who was reluctant to side with Rorty, had told him that "we share a problem that we approach from opposite sides" (Letter to Rorty Sept. 19, 1986, Kuhn MC 240, Box 22). The problem Kuhn was referring to

is the relation of the new to the old language game or paradigm, whether there is continuity or discontinuity with change. Rorty favored a "seamless redescription", as Kuhn put it, whereas Kuhn insisted on ruptures and radical differences.

10.3 Why Is Kuhn, Despite His Influence, Ignored in Philosophy?

Alexander Bird, in his article titled "Kuhn's Wrong Turning" posed a similar question: "why, despite Kuhn's enormous impact, his legacy seems so thin as regards philosophers and so beset by misunderstanding as regards historians and sociologists.?" (2002, 444). His answer was that Kuhn abandoned the naturalistic approach that he had adopted in *Structure* in favor of an a priori philosophical approach focusing on language. In *Structure* he made use of research in gestalt and experimental psychology while in his later writings, according to Bird, he resorted to "quasi-Wittgensteinian considerations from the philosophy of language, while he characterized that view in terms of Kantianism." (ibid.)[22] Bird argued that if connectionist research in cognitive science was available to Kuhn at the time, and if Kuhn was willing to take advantage of it, he would have been able to draw attention to and "exploit his central achievement, viz. the identification of paradigms, in the form of exemplars, as the motor of the development of a scientific field." (ibid.). Finally, Bird noted that Kuhn lacked philosophical training which meant that he did not realize that his account did not really reject logical positivism, the philosophical school that he aimed at bringing down. His revolution, Bird said, just like Copernicus's, was left incomplete. Because Kuhn's knowledge of logical positivism was rudimentary, he did not really break with philosophical tradition.[23] What is more, Kuhn, Bird tells us, aimed to be accepted as a philosopher and sought the appreciation of professional philosophers while he was a philosopher manqué and lacked the argumentative skills to win the case against the defenders of the causal theory of reference (ibid., 457, 459, 460–461).

One may well agree with several of the points made by Alexander Bird. For instance, that Kuhn sought indeed the appreciation of professional philosophers, that he lacked the typical philosophical training,[24] or that, under the pressure of criticism that focused on linguistic matters, Kuhn, in his later writings, forgot about paradigms and exemplars, and concentrated more on lexicons, concepts, meaning

[22] George Reisch, in his Introduction to Kuhn's Lowell Lectures, says that the so-called linguistic turn in Kuhn's later philosophy should be more appropriately called the 'linguistic return' since, Kuhn, already in 1951, when he delivered these lectures, had things to say about language and science (Reisch 2021, xx).

[23] Bird makes a similar point in his (2000, 278–280).

[24] Kuhn says so himself in his autobiographical interview (RSS 273) and Stanley Cavell reports that he felt that he was assuring Kuhn "that philosophy did not have standing answers to the questions he was asking" (Conant 1989).

and reference. But the transition, in my view, was not from naturalism to philosophy of language, but from science as practice to general philosophical matters of meaning, reference, and natural kinds.[25] One can also raise objections to Bird's claims: The Wittgensteinian considerations were present in *Structure*, but are sidelined in Kuhn's later published work.[26] What is more, Kuhn's account bears only superficial resemblance to what the logical positivists were doing. For instance, Carnap's frameworks which are often compared to Kuhn's paradigms in order to point out affinities between Kuhn and the positivists, are propositional, unlike paradigms which are concrete particulars that, by being followed, give rise to practices and traditions.[27]

Bird wants to say that if Kuhn were a better philosopher and argued successfully against his critics, if, additionally, he had pursued the more fruitful naturalistic research program of cognitive science, then his legacy would not be as thin as it is today. In my view, these explanations do not capture what has happened with Kuhn's work in the field of philosophy. The marginalization of Kuhn's work was not his fault. Kuhn's work was marginalized because it was revolutionary, and as revolutionary, it was misunderstood, just like Kuhn suggested in relation to scientific revolutions. In *Structure* he has said that, after a revolution, scientists misunderstand each other and talk past each other, that there are important scientists who hold out against revolutionary developments. In a similar manner, philosophers held out against Kuhn's approach and misunderstood his revolutionary model. They forced it into the old mold and found it wanting. They stereotyped it and looked at it from the point of view of the received view.[28] Through these glasses, Kuhn's model appeared threatening because it was challenging well entrenched and valued ideas about science, knowledge, rationality, progress, and truth. It had to be resisted and fought against. In that battle, all kinds of ammunition were used: Kuhn's work was either ignored, or ridiculed, or criticized by being reduced to a strawman. Generations of scholars did not feel the need to carefully read and engage with his actual work, so that they then proceed to criticize what he actually had said, but preferred to

[25] Actually, in his unfinished manuscript that was recently published (Kuhn 2022), it can arguably be said that Kuhn does take a different wrong turn from the one Alexander Bird identified. In his last writings, Kuhn underplays the Wittgensteinian dimension of his early work that gave prominence to practice, and casts more than a glance to research in cognitive and developmental psychology in order to provide some underpinnings to his views about kind terms. Bird saw a transition from naturalism to philosophy of language but, it can be argued, that Kuhn became more favorably inclined towards naturalism in his later writings. This could be a wrong turn if it sacrifices what was truly philosophically revolutionary in the original model.

[26] Kuhn discussed Wittgenstein's *On Certainty* in his Foerster lecture (Kuhn 2023 forthcoming) which deals with knowledge and belief.

[27] Jonathan Tsou (2015) makes a solid case against the view that there are significant similarities between Carnap's and Kuhn's work. For Kuhn's understanding of paradigms see Kindi (2012).

[28] Cf. Marx (1978, 595): "…the beginner who has learnt a new language always translates it back into his mother tongue, but he has assimilated the spirit of the new language and can produce freely in it only when he moves in it without remembering the old and forgets in it his ancestral tongue."

engage with what they were taking for granted that he had said.[29] It has been argued, for instance, that in the history of science there are no Kuhnian revolutions, which are commonly understood, on account of Kuhn's use of the gestalt switch analogy, as rare, dramatic, and abrupt events.[30] But, Kuhn in *Structure* had said that he had an "extended conception of the nature of scientific revolutions" covering different kinds (SSR 7), and spoke of revolutions as extended historical episodes (SSR 15). He also thought that they occur frequently, affecting even fewer than twenty-five people (SSR 181). None of these qualifications, however, made a difference in how scientific revolutions à la Kuhn were perceived.

Yet, despite the resistance that *Structure* encountered, the revolution that Kuhn intended, caught on and largely succeeded. His aim, as he expressed it in the first sentence of *Structure*, was "a decisive transformation of the image of science by which we [were at the time] possessed" (SSR 1). That image has indeed been transformed. We no longer think of science simply as a set of statements that are tested by experience, as accumulating truths, and as triumphantly progressing to approach a "full, objective, and true account of nature (SSR 170). As widespread and persistent these convictions are, they no longer dominate the fields of history and philosophy of science, of philosophy and of the humanities in general."[31] Even the general perception of science has changed. Kuhn's understanding of science as practice, with the implications that this understanding has, has now become a platitude.

Kuhn's intended revolution succeeded, not because its merits were openly acknowledged, but because it opened new possibilities of research in areas that were previously thought to be irrelevant to the philosophical study of science. Aspects of the practice of science (experiment, analysis of concrete scientific concepts, scientific education, institutions, scientific controversies and rhetoric, scientific communities, etc), became legitimate topics to attract philosophers, historians, and sociologists of science who forged new disciplinary divisions and collaborations. Kuhn had already said it in *Structure* in relation to science: a new paradigm wins over advocates, not by being proven more empirically adequate or closer to the truth, but mostly because it holds the promise of fruitful research (SSR 158, 202).

Kuhn did not have any philosophy students to follow in his steps, defend his work, and develop it further. He corresponded with several important philosophers, but most of them did not directly engage with his work. Many philosophers were disturbed by *The Structure of Scientific Revolutions,* as Allan Megill (1994, 4) put it,[32] and he himself felt that he "was not really getting through to philosophers, although some of them were very interested" (RSS 309). Still, despite the

[29] For the stereotyping of Kuhn see Kitcher 2018; Kindi 2017.

[30] For examples of this criticism see Kindi (2005, 2023).

[31] For Kuhn's impact on the history of science see Kindi (2023).

[32] Rorty spoke of the "fierce indignation with which Kuhn's work was greeted" (1979, 333), mainly by professional philosophers who rightly saw, according to Rorty, that "Kuhn's criticism of the tradition went deep, and that the ideology which had protected the rise of modern science was in danger." (ibid., n.16).

stereotyping, the neglect, the resistance, the often unfair criticism, Kuhn's work has left its mark on philosophy, just like it did in other areas of thought and culture.

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