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THE UNCOMFORTABLE KUHN. A REVOLUTIONARY READING OF DISBELONGING: TO WHAT DOMAIN SHOULD WE LEAVE THE KUHNIAN INHERITANCE?

K. BRADY WRAY, 2021, ***Kuhn's Intellectual Path. Charting "The Structure of Scientific Revolutions"***, Cambridge, Cambridge University Press, ISBN-10: 1316512177, ISBN-13: 978-1316512173

About Kuhn we have already read, in 60 years since the release of the *Structure of Scientific Revolutions*, critiques portraying him as a transgressor, a visionary, a reformist of the history and philosophy of science. But to achieve all this capital of notoriety and to raise a tradition by itself – for which many turned the partisanship for his convictions into a title of nobility, becoming "Kuhnians" – having a touch of genius is not enough: one's education is as important as one's innate talent. *Understanding Kuhn's Intellectual Path* is not only a curiosity, but also an exotic epistemic travel to different philosophical openings of his education, which influenced – contingently or decisively – his unique theory on the change of paradigms in the history of science. K. Brad Wray offers us intriguing insights on Kuhn's intellectual becoming in one of his recent volumes published by Cambridge University Press (2021).

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This is not a classical monography and has nothing to do with a biographical reconstruction. Wray's book fills in the gaps between the Harvard period of Kuhn (1947-1955), his influence on championing different movements from the sociology of scientific knowledge and his return to the history of science, marked by a committed interest in reconsidering historicism, and debates on the clash between realism and antirealism. These ages, gathered around the pre- and the post-*Structure* personal and intellectual history of Thomas Kuhn, reveal that the success of his bestseller was not a historical accident. Unlike Fuller, who strongly believes that Kuhnian audiences misunderstood Kuhn's notoriety and underestimated the way in which the *Structure* radically modelled our perspective on science and not always in the good way, Wray remains positive, preferring to tackle the reputation of Kuhn as a product of an intellectual pedigree in which different paradigms of thought educated his mind and tailored his beliefs on the nature of progress in science.

However, there are no reasons to feel envious of Kuhn: as Wray observed, he was late in accomplishing both his career and intellectual goals, having left his work unfinished, thus causing the public to continue to wonder about his last manuscript, which remains unpublished up to this day.

Unfortunately, Kuhn remained a dramatic figure, quite obsolete in the history of science, despite his efforts invested in raising departments of history of science. Rather the philosophy of science and the sociology of science recall his name and the impact of his thinking on such domains. Brad Wray is deeply seduced by the latter: Kuhn is portrayed in the pages of this book as responsible for challenging the knowledge produced by social sciences and recalled for realizing that "what the social sciences lacked, and what characterizes the natural sciences, are paradigms" (Wray 2021, 7). This key concept is invested in explaining both the epistemic consensus and the (im)predictability in the shift of mentalities, beliefs and values that shape and determine scientific revolutions. For my taste, Kuhn's originality has at its heart an invaluable contribution to what Kubler (1962) would call "the shape of time" or the progress of mankind: Kuhn persevered his whole life to understand if the nature of progress is cumulative or dialectical,

especially in sciences; if axiological commitments might influence the change of paradigms; if the exhaustion of a paradigm is part of the success or failure of a scientific revolution. Also, it is my conviction that the attempt to replicate the structure of scientific revolutions, by its Kuhnian recipe, in other domains, such as the history of arts, for example – explicitly, but not sufficiently tackled by Kuhn – might be a matter of ingenuity. In fact, the list for Kuhn's merits in the sociology and philosophy of science is inexhaustible, and so are his contributions in changing different practices in social sciences. But in this book, we have another Kuhn at stake: at first glimpse, an Aristotelian one.

The first part of Wray's book deals with the Aristotelian influence on Kuhn's thought, from his Harvard period, when he decided to write *The Structure of Scientific Revolutions*. Wray frames "an Aristotelian epiphany" (Wray 2021, 11), that influenced Kuhn's thought more than his readings on Galileo and Newton. Next to Aristotle, they were part of a historical branch of physicists, "or so Kuhn thought" (12). Aristotle is, in Kuhn's interpretation, an author of physics without authoring "a science of mechanics" (13). Ontological differences emerging from how Aristotle and Newton understood reality and material properties of bodies convinced Kuhn that "Aristotle had not been writing bad Newtonian physics but good Greek philosophy" (Heilbron 1998, 507; Wray 2021, 14), and this contrast might have produced his interest in *incommensurability*, one of the terms that *The Structure of Scientific Revolution* has at its heart.

Nonetheless, we shall not consider Kuhn an ideal Aristotelian, but rather an unsuccessful one, for at least two reasons expressed by Wray. One is that "Kuhn was not able to appreciate the integrity of Aristotelian worldview" (Wray 2021, 14) and the other one is that understanding the Aristotelian roots of the motion and the role played by it in different contexts was not an easy task for Kuhn (see Kuhn 1977, xii). But Aristotelian readings were an enlightening experience that influenced Kuhn's perspective on the nature of scientific revolutions.

On the one hand, Aristotle proved being capable of a very integrative worldview. On the other hand, reflecting on the Aristotelian *Physics* allowed Kuhn to draw some general insights on the nature of revolutionary changes, which, when applied in science, reveal how disruptive they can be, having arisen from the experiences encountered

during the research process. Consequently, "The Road since Structure" – an interview from 1995 – portrays Kuhn as being determined to write about such a topic immediately after he got contaminated by an Aristotelian perspective on progress and change. However, since his real engagement into ancient philosophy up until the moment when the manuscript of *SSR* became the book consecrating his thought, Thomas Kuhn was convinced that there were still things to learn, crucial to explain how anomalies tend to normalize through revolutionary changes. Wray highlights that there were many steps left from this ongoing process of crystalizing the theory on *SSR*, since the knowledge growing in natural sciences was not inscribed into a cyclical pattern of change. *Normal science* and *scientific revolutions* were blended into one theory only after Kuhn successfully managed to explain the link between the concept of *paradigm* and "the notion of mopping up", which was more or less "the bulk of scientific practice" and the key to understand "theoretical breakthroughs" (Wray 2021, 19-20). From this point on, Kuhn is influenced by an interdisciplinary approach on science, reading historians and sociologists that inspired him to critically undertake the genealogy and evolution of scientific communities. Fleck's writings concerned Kuhn since the concept of collective thought proved to be problematic in terms of the predictability embraced by scientific revolutions, but also in terms of raising and securing the authority of a paradigm. "A more complicated case" was, in Wray's opinion, Alexandre Koyré, with his *Études galiléennes*, which became a mandatory reference provided to his students and offered food for thoughts to conceive an internalist approach on the history of science. Diagnosticating Kuhn's originality is not an easy endeavour and yet Wray masters such analysis accurately, offering us a rare deconstruction of *SSR*.

To date, no one was interested, nor seduced, by the impact of Conant's writings on Kuhn. Wray devotes a full chapter to bridge the gaps between Conant's perspectives on natural science and Kuhn's theory. Nonetheless, despite the sincere admiration Kuhn carried for Conant, there are at least five contributions in *SSR* which were developed independently of this biographical link, with all the correspondent affinities: "the concept of paradigm", "the concept of normal science", "the problem of scientific revolutions", "the related concept of incommensurability",

"the emphasis on the social dimension of science" (Wray 2021, 26). On this topic I consider relevant the intersecting efforts of Fuller and Wray, developed by different means, to highlight the fact that without the Kuhnian interest on the social insights of scientific communities, the public would have remained immune to the dynamics of science and insensitive to the matter of progress. But Kuhn accommodated large audiences with the idea that as conservative and rigorous science might be, at the end of the day, science is shaped by social institutions, raising a capital of innovation which catalyses welfare and interdisciplinary knowledge. In fact, by adopting this perspective, Kuhn made Fleck notorious, recognizing in the opening of the *SSR* that without a glimpse on the sociology of the scientific community, the revolutionary practices in sciences would have been very difficult to observe. Wray adds that the influences of Toulmin and Polanyi on Kuhn's writings are equally remarkable and yet, one of the major outcomes of this book is represented by the argumentation in favour of separating the genealogy of major Kuhnian concepts and themes from *SSR* from these authors, rather grounding their origin and initial meaning in Conant's writings.

What we often overlook – even the most passionate Kuhnian readers – is that Kuhn rarely used "conceptual schemes", which are more likely to be found in *The Copernican Revolution*. However, in *SSR* the term is completely avoided, which makes Wray believe that although Conant influenced Kuhn methodologically, the analysis of science is mainly based on theoretical frameworks. Interesting is the following remark belonging to Wray: "like Conant, Kuhn claims that scientists spend much of their careers making nature fit into the conceptual boxes supplied by the accepted theory" (Wray 2021, 35). In my opinion, this aspect should raise questions on the risks of falsifying research or delaying progress and on coming up with revolutionary approaches, respectively. However, I am perfectly aware that conceptual schemes should also support the dominant holistic view embraced by Conant and Kuhn, and this might be the reason for which such a matter turns out to be less invasive or problematic. Wray also reflects on how Weber tailored Kuhn's taste for a holistic methodology inspired by social sciences, which made him consider regularities not as ends, but rather as means of knowledge (see Wray 2021, 36). But as we advance

further with Kuhn's attachment to sociology, we discover a historian of science who, like Conant, considers revolutionary practices to be determined by new theories: empiricism fails, in these terms, to provide a unique and exclusive origin for such progress.

Nonetheless, one of Wray's hypothesis drew my attention in particular: "In comparison to Conant, though, Kuhn was less insistent that revolutions are the sources of the greatest progress" (Wray 2021, 37). It seems that Wray places the narratives of progress at the heart of a more deconstructivist approach borrowed by Kuhn, to drop the canonized idea that scientific progress is *cumulative*. And yet it seems to me that the distinctions between the dialectical and the cumulative nature of progress are more relevant in post-*Structure* writings. For example, in "Comment on the Relations of Science and Art" where Kuhn (1977, 340-351) considers the hypothesis of applying the *SSR* in the field of the arts. On that occasion, he reflected on "the cumulative and disruptive character of art and science; the symptomatic, character of each discipline to structure its main topics and problems in the form of a puzzle; the rivalry between the following core-concepts paradigm, style, and theory" (Șerban 2022, 90-91). It might be true that progress was never a milestone for the Kuhnian thought, or that he never wrote challenged solely by the need to unveil the nature and dynamics of progress. But whenever he compares domains, science reveals itself following the cumulative path in the shift of paradigms, while arts retake and reshape contents of style by dialectical means. This is particularly why I believe that progress suddenly becomes one of the means that Kuhn had at his disposal to secure the particularities of science and to distinguish paradigms from Foucauldian episteme or Kublerian styles because the latter were behaving rather dialectical than cumulatively. Moreover, Wray denounces other misinterpretation of Kuhn's intentions: he never followed a Cartesian path, therefore, he never aimed to support a so-called *mathesis universalis*. A unified science was never a stake, neither for Conant, nor for Kuhn, but the explanation lies on the fact that the latter believed in the incommensurability "between theories in neighbouring specialities" (Wray 2021, 38; Kuhn 1991/2000a, 98). Implicitly, I consider that this position tailored a rather new age of modernity, in which *mathesis* was a dream left behind and where methodological holistic perspectives did not necessarily impose a Cartesian project of unified science.

In spite of everything, the obsession for method remains a dominant piece of this large puzzle of scientific revolutions, revisited by denunciating the collapse of "scientific method" and raising ambitions of privileging paradigms. Epistemic achievements rather than methods will guide the appetite of reflection that scientists invest in their research. This is the main reason for which Wray finds Kuhn responsible for "shifting emphasis away from scientific method" (Wray 2021, 40).

Three arguments provided in this book by Brad Wray seem to me truly relevant to understanding why *The Structure of Scientific Revolutions* was revolutionary by itself, all of them framed by the impact of Conant's writings on Kuhn's thought. First, *The Copernican Revolution* did not anticipate *SSR*. The former barrows Conant's vocabulary (41), whereas the latter deals with *paradigm shifts*, *normal science*, and *incommensurability*. In fact, the former implements Conant's conceptual scheme, the latter totally defies it. These differences equally stand for a change mirrored by the dynamics of Kuhn's mentality on the relevancy of psychological factors impacting matters of progress and revolutionary practices. *SSR* is more committed to embedding the role of values, behaviours and beliefs in tailoring scientific education and research. Secondly, there are separations between Conant and Kuhn, as the former advised the latter to restrict paradigms to the use of examples and models, not to theories. Wray states: "Conant was initially very uncomfortable with Kuhn's use of the concept when he read the draft manuscript of *Structure*" (43). Apparently, for Conant's taste, the concept of paradigm was too general, and it was no sooner than the 1970s when Kuhn restricted the term to determine something exemplary for a unified scientific discovery. At the same time, the concept of "paradigm" became canonical for normal science, and what was still on hold was the analysis of its capacity to embed the juxtaposition between a regular and a revolutionary form of progress. This contrast is relevant because it highlights Conant's orientation to track down paradigms as effects of radical conceptual innovations, while Kuhn would plea in favour of a more particular meaning, emerging from solving a puzzling problem by means of revolutionary science. Thirdly, if paradigms are delicate, scientific revolutions are even more subtle and difficult to approach. For Kuhn, they have at their heart epistemic problems stimulating progress. Wray recalls that "Kuhn's analysis

of progress through revolutions gave birth to the notion of Kuhn-loss, a key target of criticism raised by philosophers of science" (46). In short, Kuhn was more normatively oriented, while Conant remained skeptical or disinterested regarding this aspect. On top of everything, the contrast between Kuhn and Conant has been deepen by *incommensurability*, a concept coined by Kuhn, but totally absent from Conant's works. Either conceptual, methodological, or related to communitarian consensus, *incommensurability* became a key-term in Kuhn's model of structuring the scientific revolutions. The simple fact that social factors were tailoring *incommensurability* as much as scientific ones, turned Kuhn's theory into a very fashionable construct at that time. Science was, by this perspective, explained once again as a vector to propagate social phenomena, not only knowledge and epistemic traditions. Only for this, and *SSR* was worthy to be largely and suddenly consulted by outsiders of the scientific bubble, such as sociologists or historians of different domains.

The first part of Wray's volume ends with critical remarks on the impact of the Kuhnian legacy on the history of chemistry and the logical positivists. It seems to me that beyond particular reflections that Wray competently advances in regard to both domains, what they have in common is a sacrificed, marginal disciplinary history, along which Kuhn's influence on these domains was visible, as well as the other way-round. There is a discussion on how the Cold War culture influenced Kuhn's sensitivity to chemical innovations that could affect the quality of life, or to what extent the chemical implications of pigments and techniques of painting might have raised curiosities for him to explain the possibility to commute the model of scientific revolutions in the history of art, so as scholars are still discussing the role of logical positivism to strengthen Kuhn's caprice to write *SSR* as a book capable of synthesizing the image of science. But none of them seems to me more exotic than those passionate debates on what Wray presents in the second part of his book as "the unexpected uptake", meaning the impact of Kuhn on social-sciences.

Was Kuhn a trendsetter for the sociology of science? Definitely, he takes the credit for the popularity and authoritative rise of this domain in the 20th century. Before sociologists embrace Kuhn's writings, psychologists have enthusiastically declared their support for his authentic manner of discussing the structure of scientific revolutions. Not only because Kuhn

generously delved into the psychological analysis of Piaget and Gestalt scholars, but mostly because despite the lack of formal education in psychology, arbitrarily selecting relevant references to explain the playful psychological background of paradigm shifts and revolutionary practices, he managed to convince psychologists to pursue the *SSR* as "a contribution to their field" (85). On the side of sociologists, it seems that Kuhn's legacy was made responsible to fuel their domain with arguments to track down social sciences as capable to behave as scientifically as "natural sciences" (90). Many echoes revealed the revolutionary potential of Kuhn's bestseller: Wray recalls President Truman's speech from 1965 insisting on the need to operate political paradigms with precision, and President Almond's insistence to recognize the innate capacity of political sciences to operate paradigms. Both leaders of the American Political Sciences Association, their discourses are just two pieces of examples that synthesize the Kuhn-effect on domains to which no other historian of science reached, in his century.

Nonetheless, as Kuhn's arguments were more fashionable, the public reflection on the main differences between natural and social sciences became broader and more tensioned. One of the most relevant disputes on this topic was that between Charles Taylor and Thomas Kuhn, which at first glimpse was inspired by methodological differences between the two scholars. The former insisted on the hermeneutical capacity of social sciences, which is less tackled in natural sciences, while the latter was convinced that their rivalry – if such scenario is plausible – is that social sciences operate more unstable objects than natural sciences. It is the main reason for which "the heavens remained the same" (Kuhn 1991/2000b, 223) from Greek to Copernican astronomy, which we cannot say about political and social systems (Wray 2021, 91).

However, beyond local and global disputes, the seed of the Kuhnian thought in social sciences flourished by upgrading the social scientific paradigms. Wray observes that Kuhn becomes fashionable in social sciences without taking any explicit credit for his renewed concept of paradigm: "there is no mention of the specifics of Kuhn view" (93), although Marcionis, for example, splits sociology into three main historical paradigms: the structural-functional paradigm, of Durkheimian origin; the social-conflict paradigm, which was entirely Marxist; and the symbolic-interaction paradigm, decisively Weberian (Marcionis 1997, 16-22).

What seems relevant to me is how we can link in these dishonest conditions Kuhn's name with social sciences and to what extent the natural use of paradigms in social sciences, without giving credit to their author, might affect the accurate analysis of Kuhn's successful and unfinished project, to commute the structure of scientific revolution in other domains, such as the history of art or sociology. Wray offers a precious perspective, arguing that "when sociologists discuss paradigms it is now quite common for Kuhn not to be cited at all" (94), a phenomenon targeted as "obliteration by incorporation", using Merton's formula (Merton 1988, 621). What we find out after surfing generous examples grasped from anthropology, political science, and economy, is that Kuhn's interpretation on paradigm became normalized. Or, to express it more aesthetically, he became the victim of his own way of interpreting normalizing practices of sciences. A paradigm is part of the regular discourse of a normal science. Is there any exception to this pattern? Apparently, there is one, but even more dissatisfying, and from one point onward, quite toxic, given the social impact. Wray critically undertakes Walker's argument that following paradigms, political scientist will "engage in hostile zero-sum turf war" (2010, 434) and will focus on explaining those occurrences when a revolutionary paradigm does not substitute a dominant one, but rather develops an alternative theory or a subfield of research. Exotic examples arise from Walker's analysis: "hyper-specialized tribalism within subfields and furthers the Balkanization of political science as discipline" (Walker 2010, 434). Wray explains that this standpoint is too virulent and a little bit anti-Kuhnian. In fact, Kuhn foresaw the fact that new scientific specialties will emerge not from revolutionary interpretations, but from reframing of different problems correspondent to a valid, normal and dominant paradigm. On the contrary, it was a proof of success, not a piece of tribalism and balkanization and, to be more precise, Kuhn would say it is quite desirable to assist to such forms of transition and innovation. For Wray (96-97) the eccentric point of view of Walker is worthy to be considered just to answer to how many paradigms can coexist in a social scientific field, how many specialties are valid at once and how are these new specialties or fields of expertise arbitrate the competitivity and incompatibility of paradigms.

For my taste, this problem should be also framed as a possibility to understand new fields of expertise embed the notion of predictability of changes, based on their receptivity on paradigms and puzzling problems. But Wray takes this framework as an excellent opportunity to discuss the undiscussable, "the elephant in the room": the limits and authority of the sociology of scientific knowledge. A cocktail of events and perspectives present Kuhn as deeply engaged into the sociology of science. First, his interest into Merton's theories on priorities in scientific research and discovery set up a powerful background for analysing the culture of science. The simple fact that Merton recognized Kuhn's particular reading of sociological processes as impacting historical development make us wonder not only what his influence was on reshaping the destiny of research communities, but also to reflect on how sociological the concept of structure was emphasized at that time. Kuhn has the merit of having deconstructed the multiple phases that a research community undergoes by social changes, transgressing cultural challenges, (un)popular mentalities and contractual forms of agreement, consensus, and quantified progress. Whenever we deal with a scientific crisis, there is always a social explanation as well that stands for that impasse. If scientific education stimulates progress, then socialization is part of it. Moreover, Kuhn is deeply seduced by effects of this professional cohesion: the raising and strengthening of consensus paves the way to normal science and its unproblematic uses. The rise of anomalies might be, from my standpoint, a matter of sociological deviance, but Kuhn remains loyal to matters of intellectual commitments and interests and economic negotiations that scholars tend to practice in order to predict and control a paradigm. Wray adds to these elements some other ingenious insights that justify the sociological turn of Kuhn: his criticised, and yet, intriguing *Strong Programme in the Sociology of Scientific Knowledge*; his perspectives on the moral responsibility of scientists for progress; as well as his concerns for the methods invested in measuring the level of satisfaction that scientists reach in achieving epistemic goals.

The third part of Wray's volume is consecrated to the relationship between the Kuhnian philosophy and the history of science, which is complex and inexhaustible, but brings to the spotlight the fact that surprisingly, "contemporary historians have a rather dismal assessment of *Structure* as a contribution to the history of science", an assessment

quite irrelevant, as it Kuhn's book "was not intended to be a contribution in the history of science" (Wray 2021, 119). This chapter represents a valuable contribution to the attempts of mapping what we might call *a Kuhnian ethos*. Wray is a master of linking shifts in Kuhnian scientific interests and levels of notoriety with the mental geography that emplaces his ideas. Copenhagen, Harvard, Princeton, Berkeley are tackled as places for making Kuhn's ideas either revolutionary or unpopular. Wray portraits a Kuhn who navigates through multiple academic traditions, which provided him different senses of belonging, and becomes convinced that "there was something that happened to people who'd spent too much time around Harvard" (Kuhn 1997/2000a, 28). And here is how we reach the Kuhnian paradox: being "never at home in any discipline", although he impacted all disciplines at once through his *Structure*. In short, Wray highlights the lack of narrow philosophical education that Kuhn resented from one point onward, but what strikes me is this arguable dichotomy, that I have never given attention previously: "Kuhn did not think one could work as a historian and a philosopher at the same time" (Wray 2021, 134). Should we tackle Kuhn's legacy enlightened by this exclusive disjunction that attests that one could be either a historian of philosophy or a philosopher, but never both at the same time? How many philosophers, reading this statement, nowadays, feel their careers shadowed by such discretionary approach?

Wray prefers to remain silent on this topic – there is no explicit sign that he would have been interested to dismantle different answers to this pivotal question; however, implicitly, it seems to me that the major, untouched topic here is: to what extent choosing both would alter the meaning of normal science for a historian of philosophy, respectively for a philosopher? Until we ever get a reaction on this, we find Wray's arguments that the *Structure* was not decisive for the history of science, regardless of its popularity in this field, that "insofar the book is a contribution to the philosophy of science, the sort of thing Kuhn means by structure is perfectly respectable, and is often presupposed in many philosophical studies", everything in order "to defend Kuhn against the charge of historicism" (136). By reflecting on matters of *Structure* and structures in philosophy of science, what it seems to me is that Wray succeeds in reconstructing genealogically the Kuhnian paradigm by tackling limits of its archaeological application. If we look to Kuhn's legacy

through the lenses of scientific change, then I agree, there is nothing new. I equally find more than honourable and accurate Wray's intention to save Kuhn from a Popperian form of *historicism*, that he has never authored. Nonetheless, I still question myself if this is not a very restrictive manner of devised to puzzle historicism, one which very conveniently isolate Kuhn's work from Popperian amendments. What if we leave aside Bird's deconstructivist approach on historicism, which made him consider Kuhn as a practitioner of a conservative historicism (see Bird 2015, Wray 2021, 150) and we implement a more Foucauldian approach?

To fully understand the role of Kuhn on the sociology of knowledge, that Wray seems to defend, I think we should take the risk of considering the structure of a scientific revolution influenced by the power-knowledge relationship that a discourse embeds, and through which is capable to react to standards of "normality" and "truth". It would be more useful, I guess, to arrest this Foucauldian perspective, since on the one hand is generous in setting affinities and discontinuities between *paradigms* and *epistemes*, which have been unrightfully overlapped, and on the other hand is more attentive to shed light on an anti-historicism that both Kuhn and Foucault might share, but which is tolerant with the so-called "historical *a priori*", that blends categories of time, place and culture, to alter paradigms and impose the urgency of a scientific revolution.

This link would also stimulate the reflections on Kuhn's philosophical legacy, that Wray discusses in the fourth part of his book. For a former non-philosopher, Kuhn succeeded in "setting the agenda with the problem of theory change, a consequence of reflecting on the nature of scientific revolutions" (153). In my opinion, Wray's chapter equips philosophers with all the necessary concepts and methods to answer to questions still fashionable for scholars working in this field: "do Kuhnians have to be anti-realists?" (Dimitrakos 2023) Is there any anti-realism that we might rebuild as an autonomous tradition, "from Kuhn to Foucault"? (Gordon 2012) How can we resist scientific realism (Wray 2018) and to what extent this is a worthy discussion to be carried out nowadays?

At the end of this book, we discover another Kuhn: one on which scholars remained silenced, because it is not a comfortable endeavour to bridge biographies and ideas behind well-reputed figures of the intellectual history of our last century. In the case of Kuhn, this difficulty is doubled by his own affinities and curiosities for topics that go beyond and

behind the history of science, which render this reconstruction as transgressed not only by an epistemic altruism, at different ages, but also by forms of scientific rigor, scepticism or creativity. If one is not a Kuhnian, after reading Wray's book will consider becoming one. Wray has a particular manner of overcoming a hermeneutical prudence to leave the biography of an intellectual immune or unlinked to the layer of intellectual positions; reading this book makes you wonder if this stubbornness of keeping the ideas as "clean" as possible, away from biographical occurrences, is not counterproductive; if what we miss, in our most competitive educational paradigms, regardless the academic traditions behind them, is particularly this ability to get engaged into the history of ideas and to use the intellectual history as a path to reach the heart of philosophical debates. "Charting *The Structure of Scientific Revolutions*" means to map cultural mentalities and beliefs of scientific communities that welcomed or rejected the Kuhnian inheritance with equal plausibility and rigor. I wholeheartedly recommend this book to those who would like to understand the impact of Kuhnian works in the field of the sociology of knowledge and social sciences in general, for at least two reasons. One is quite selfish: it supports my own reading on Kuhn's role in shaping the notion of progress in these domains but delivers new insights and arguments on Kuhn's preferences for certain standpoints and beliefs. The other one is quite altruistic: because it is time to raise the awareness of researchers and specialists from different fields that interdisciplinarity, which sets the trends in academic research and founding nowadays, depends on understanding paradigm shifts, the incommensurability of scientific discourses, or the relationship between scientific problems shared by different domains and the history behind them. No one can really be a master on that, lacking a particular knowledge of Kuhn's works and the intellectual paths that guided them.

References

Bird, A. (2015). "Kuhn and the Historiography of Science." In W.J. Devlin and A. Bokulich (eds.), *Kuhn's Structure of Scientific Revolutions – 50 Years On*. Dordrecht: Springer, 23-38.

Dimitrakos, T. (2023). "Do Kuhnians have to be anti-realists? Towards a realist reconception of Kuhn's historiography." *Synthese* 202, 21, Doi: 10.1007/s11229-023-04225-z

Fuller, S. (2000). *Thomas Kuhn. A Philosophical History for Our Time*. Chicago: The University of Chicago Press.

Gordon, P.E. (2012). "Agonies of The Real: Anti-Realism from Kuhn to Foucault." *Modern Intellectual History* 9(1): 127–147, Doi: 10.1017/S1479244311000515.

Heilbron, J.L. (1998). "Thomas Samuel Kuhn, 18 July 1922 – 17 June 1996." ISIS 89(3): 505-515.

Kubler, G. (1962). *The Shape of Time. Remarks on the History of Things*. New Heaven, London: Yale University Press.

Kuhn, Thomas S. (1970). *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.

Kuhn, Thomas S. (1977): "Comment on the Relations of Science and Art." In *The Essential Tension: Selected Studies in Scientific Traditions and Change*. Chicago: The University of Chicago Press, 340-351.

Kuhn, Thomas S. (1991/2000a). "The Road Since Structure." In Thomas Kuhn, *The Road since Structure: Philosophical Essays, 1970-1993*, with an autobiographical interview, edited by James Conant and John Haugeland. Chicago: University of Chicago Press, 90-104.

Kuhn, Thomas S. (1991/2000b). "The Natural and the Human Sciences." In Thomas Kuhn, *The Road since Structure: Philosophical Essays, 1970-1993*, with an autobiographical interview, edited by James Conant and John Haugeland. Chicago: University of Chicago Press, 216-223.

Marcionis J.J. (1997). *Sociology*, 6th edition. Upper Saddle River, N.J.: Prentice Hall.

Merton, R.K. (1988). "The Matthew Effect in Science, II: Cumulative Advantage and the Symbolism of Intellectual Property." ISIS 79(4): 606-623.

Șerban, O. (2022). *After Thomas Kuhn: The Structure of Aesthetic Revolutions*. Berlin, Boston: De Gruyter.

Walker, T.C. (2010). "The Perils of Paradigm Mentalities: Revisiting Kuhn, Lakatos, and Popper." *Perspectives on Politics* 8(2): 433-451.

Wray, Brad K. (2018). *Resisting Scientific Realism*. Cambridge: Cambridge University Press.

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