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A STRUCTURE FOR HISTORY: REFLECTIONS FROM KUHN'S HISTORIOGRAPHIC STUDIES³

Abstract. In the present work, we aim to analyze Lorraine Daston's critiques of the historiographical value of Thomas Kuhn's *The Structure of Scientific Revolutions*: we will defend its relevance from the attacks of "the history of contingencies". Daston's proposal asserts that the Kuhnian historiographical programme of professionalizing the history of science (moving it towards history departments to the detriment of science departments) has been fulfilled but has resulted in the abandonment of the Hegelian spirit from Kuhn's historiography, *i.e.* the search for "a structure" of the history of science has been abandoned. We will analyze and incorporate the recent responses from K. Brad Wray and Pablo Melogno. Finally, through a thorough analysis of the relationships between philosophy and the history of science, particularly in Kuhn's work, we will propose a defense of the systematic and explicit use of metatheoretical structures for historiographical endeavors.

Keywords: historiography of science, historical philosophy of science, Thomas Kuhn, historicism

1. Introduction

The perennial importance of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) remains perplexing (Green 2016; Hacking 2012; Giri &

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³ This paper is dedicated to our great friend Dr. Pablo Melogno (1979-2023), a sharp Uruguayan philosopher and a profound connoisseur of Thomas Kuhn's thought.

Giri 2020). The topics Kuhn worked on in his classic are still contested, and this author remains a central interlocutor, despite the obvious advances that more than 60 years of development in the philosophy and history of science produced after *Structure* (Richards & Daston 2016; Giri, Melogno, & Miguel 2023). One of the recent debates (though clearly not entirely novel, since it was constitutive of the rise of the so-called “new philosophy of science” or, better still to abandon the anachronistic label, “historicist philosophy of science”) was triggered by Lorraine Daston (2016) in a volume in homage to the 50th anniversary of *Structure*. In the historian’s opinion, the term “structure” in reference to history (in general, and of science in particular) has become outdated as a guide for historiographical work.

Thus, proposals to conduct history of science from rational reconstructions (*i.e.* to search for “structures” that highlight certain dynamic patterns of theoretical change in order to develop relevant historical narratives) have, in Daston’s opinion, become obsolete. The history of science, as it moved from the majority aegis of scientists to history departments, took on a contextualist turn that distanced it from philosophy and sociology. The history of science becomes a history of contingencies, and each historical episode becomes a unique episode of its kind (see also Kindi 2023).

In this paper we will start from Daston’s analysis and the recent responses that her work received from two important exegetes of Thomas Kuhn’s work. On the one hand, the response of K. Brad Wray (2023), and on the other, the comments to Wray (and thus also to Daston) by Pablo Melogno (2023). We will then propose our interpretation of the controversy and our position in it, for which we will defend the relevance of a history of science based on systematic metatheoretical frameworks, and the dangers of not having an explicit philosophical framework to support the historiographic narratives. This way of working has a fundamental antecedent in Kuhn, although, to his regret, since he himself defended (as, we will see, not without serious contradictions) that philosophy and the history of science are separate enterprises. Finally, we will present our conclusions.

2. The controversy

2.1. *Unstructured history: Lorraine Daston*

The central idea of *History of Science without Structure* (Daston 2016) is perfectly consistent with its title, and is also a categorical diagnosis: historians of science have abandoned the search for structures in the history of science, not because the one proposed by Kuhn (1962) has necessarily been refuted or because a better one has replaced it: simply, historians do not consider useful for historical enquiry to take into account more or less constant patterns or models in the historical facts to be analyzed. Why?

“Most historians of science no longer believe that any kind of structure could possibly do justice to their subject matter. The very idea of looking for overarching regularities in the history of science seems bizarre, a kind of leftover Hegelianism seeking a hidden, inexorable logic in the apparent vagaries of history – in Kuhn's case, the last attempt to give Reason (now incarnate in science) a rational history.” (Daston 2016, 117)

Thus, according to Daston, there is a tendency, especially since the 1990s, towards a kind of academic baroque that favours works rich in complex information over those that seek simplifying patterns, which, in the history of science, translates into a contextualist history. There, each particular fact is unique, incapable of being fitted into any natural class.

Daston's diagnosis does not seem to be supported by any extensive bibliographical analysis of the works on the history of science of the last 30 years, but a brief analysis of the curricula of the history of science seminars offered in the history courses of some important Latin American universities seems to support the author, and it is very plausible that the same tendency is also registered in the American and European academy: the works of philosophers belonging to the “new philosophy of science” (nowadays better called “historicist philosophy of science”, *i.e.* those who defended the relevance of the use of philosophically grounded structures for a fertile history of science) are mostly conspicuous by their absence. The only work by Kuhn that seems to be read in most majors in introductory philosophy of science courses

is, of course, *Structure* (see Giri & Giri 2020), but the interest seems to be more in his model of the dynamics of science than in a heuristic for a history of science through rational reconstructions.

Thus, in the absence of refuting instances, we can take Daston's diagnosis as true: historians of science (to be cautious, let us say "the majority") who come from history departments do not use "structures" in their research, but have a contingentist conception of history. Daston then proposes a dichotomy: either one researches with "structures" or one is "historicist", on the understanding that one form of research implies the negation of the other:

"The historicist program in the history of science has fractured the once- monolithic 'science' into the sciences and raised serious questions as to whether the term can be applied at all to the premodern epoch. Yet the historicism Kuhn prophesied and welcomed has ultimately dismantled the structures he sought: an essential tension at the heart of his own still riveting vision for the history of science." (Daston 2016, 118)

In part, the break with structures has to do with the fact that this historicism (so called presumably because it is the method of historians coming from history departments) uses the same working strategies for the history of science as for the history of any other topic: basically an exhaustive treatment of contextualized sources, whereas structural methods, in seeking to isolate a structure for science, do not require much of the external (or non-immanent) other than as decorations in the narrative (in the manner of Lakatos 1970). The contingentist style dissolves the internalism-externalism discussion by intermingling them and denying that science has an "inside and outside". After all, the focus would be on practices, whose description will necessarily include components of both types of historiographies.

A corollary of the abandonment of structures is the severing of the links between the history of science and the philosophy and sociology (of science). However, it may be possible, according to Daston, to recover these links by changing what we understand by "structure". Structure in *Structure*, as Daston understands it, involves

"how disciplines became 'mature sciences' with the advent of their first paradigm; how paradigms 'provide scientists not only with a map but also with some of the directions for map-making'; how cumulative progress is only possible within a paradigm; why it was impossible for paradigms to peacefully coexist, for 'proponents of competing paradigms practice their trades in different worlds.' Only 'orthodox theology' could compete with the 'narrowness and rigidity' of scientific education into the reigning paradigm. The history of science was nothing more or less than the history of paradigms as they rose and fell like empires. According to Kuhn, paradigms could even mold perceptions, encoding knowledge into the very neural processes of scientists." (Daston 2016, 24)

However, if we take by "structure" not these elements of paradigms (understood as a "disciplinary matrix") but instead paradigms understood as "exemplars", then interdisciplinarity can still be achieved. An exemplar is for Daston (and for the Kuhn of the Postscript, see 1996) a much richer unit of analysis, as it points out, not by algorithmic rules, but by implicit characteristics, the correct ways of operating in scientific practice (in a given socio-historical and cultural context). The adoption of exemplars would allow "historicist" historians of science to work with a sufficiently concrete unit of analysis without committing themselves to the idea of a "special" science, esoteric and more rational than any other practice or mode of knowledge, and it is also a "context-sensitive" unit (Daston 2016, 128), and of interest also to sociologists, philosophers and even anthropologists of science. Thus, paradigmatic exemplars incarnate Daston's compromise solution for a history of science that is simultaneously contingentist but also interdisciplinary.

2.2. Structure as a strictly philosophical book: Brad Wray

Brad Wray's paper, "A defense of structure in *Structure of Scientific Revolutions*" (2023) constitutes Wray's vigorous response to Daston and can be read as a plea for the perennial relevance of *Structure* in the philosophy of science. The central core of Wray's proposition is the following: Daston's

critique of *Structure* is ungrounded because her argument rests on the claim that Kuhn attempted to provide therein a methodology for the practice of the history of science (namely, by making rational reconstructions of science using paradigmatic structures as the unit of analysis). Daston's argument is unsound, basically because Kuhn's intention in *Structure* would be for Wray entirely philosophical and not historiographical (in other words, the target reader is a philosopher and not a historian, and so, *Structure's* ideas are not meant to be applied in doing history of science). As additional premises, Wray adds that Kuhn's notion of "structure" is respectable and that the author's analysis of science is essentially correct.

Wray admits that it is not easy to classify *Structure* (or Kuhn himself, who has done both purely philosophical and purely historiographical work) disciplinarily. However, he emphatically defends the classification he chooses for *Structure* on the following grounds:

1. Kuhn (1962) claims that if we study the history of science (in an anti-whig way) it will transform our view of science, but "transforming our view of science" is a philosophical and not a historiographical goal.
2. Kuhn (1977a) recognizes that there are essential differences between history and philosophy of science (developing narratives about the particular and pursuing general theories, respectively).
3. Contemporary historians of science generally do not attach much importance to the impact of structure on their practice (in this respect there is clearly agreement with Daston).
4. Some epistemic authorities such as Peter Galison, Joel Isaac, Peter Dear and David Kaiser and even his disciple John Heilbron deny the value of *Structure* for the history of science.
5. Perhaps the strongest argument: Kuhn himself claimed in his famous interview in *The Road Since Structure* that his aims were philosophical, notably by stating: "...my ambitions were always philosophical. And I thought of *Structure*... as being a book for philosophers" (2000, 206).
6. He also states in the preface to *Structure*: "[had a shift in career plans] from physics to history of science and then, gradually,

from relatively straightforward historical problems back to the more philosophical concerns that had initially led to history" (1962, xxxix-xl).

Once Wray leaves his thesis on the philosophical essence of *Structure*, he proceeds to elucidate the notion of "structure" presupposed therein:

"...Kuhn wants to show that scientific revolutions do not happen in some random, chaotic, or unpatterned way. Rather, they take on a particular form. In fact, according to Kuhn, it is not only scientific revolutions that have a structure... The development of a scientific field as whole has a pattern or form (...). Roughly, the structure is as follows. [After emerging out of the preparadigmatic state] A period of normal science, in which scientists take the fundamentals of a field for granted, leads to a period of crises, caused by persistent anomalies that resist solutions. The crisis causes a slackening of the disciplinary norms and standards, which leads to the generation of new theories (...). Finally, a new theory proves to meet the challenges the field faced and it becomes the dominant theory, which leads to a new normal scientific research tradition." (Wray 2023, 31-32)

This notion of structure may certainly not be of interest to historians, but it is still plausible, and for Wray essentially correct: theories are born, grow, go into crisis and then are replaced in a way that, while it may have exceptions, more or less represents actual processes of theoretical change.

Finally, Wray embarks on denying that *Structure* (in particular) or Kuhn (in general) has a "historicist" view of the history of science in Popper's (1991; 2011) sense, *i.e.* that it is believed to possess an identifiable trajectory comprehensible by the social sciences (and hence predictable at some point). The kind of pattern that Kuhn identifies in science and its progress is not teleological, it does not allow a forward-looking historical trajectory to be established more than vaguely. In other words, it is not the same thing to posit that a current paradigm will go into crisis as it is to make a specific prediction of the future. In fact, the most bitter enemy of Popperian historicism (Popper himself), although a fierce critic of Kuhnian metatheory, did not attack Kuhn for

imposing a teleology, but especially for defending the existence and progressivity of the stages of normal science (see especially Popper, 1970). Thus, Wray considers that Kuhn adheres to the so-called “contingency thesis” (Hacking 1999), which states that the conceptual development of science is by no means predetermined.

In short, for Wray, Daston is fundamentally wrong in her critique of Kuhn because *Structure* is a philosophical text, not a historiographical one, and because, although historians prefer to ignore it, this philosophically grounded structure is prolific for the analysis of science.

2.3. Pablo Melogno vindicates the historiographical role of Structure

The paper “A vindication of *Structure* in *Structure of Scientific Revolutions: A comment to K. Brad Wray*” (Melogno, 2023) is not only a response to Wray’s analysis, but also to Daston’s. The main reason for the disagreement between Melogno and Wray will be over the scope of *Structure*, which Melogno will understand not only as philosophical but also as historiographical. However, he will differ with Daston about the character of Kuhnian historiography being teleological, and hence, that it is “a kind of leftover Hegelianism” (Daston 2016, 117).

First, Melogno recognizes Wray’s analysis of the term “structure” as good, but defends its historiographical value by stating that “if we consider that Kuhn claims that scientific revolutions do not occur randomly but follow a pattern, and if this pattern is repeated throughout history, we are attributing to Kuhn a historiographical thesis about scientific revolutions” (2023, 45). This statement means that the admission of certain patterns in science sustained over time has an impact on the history of science, and since the search for these patterns has been a constant in the philosophy of science, it is clear that philosophical activity has a historiographical corollary.

Philosophical activity being inherently legitimate, Daston should accept that its corollary is legitimate too, regardless of whether historians (with history majors) like to use those patterns or not. The decoupling of historians and philosophers is neither new nor necessarily problematic

according to Melogno, given that both have been extremely prolific in their respective endeavours, and so was Kuhn himself:

“Kuhn's historiographical theses about the dynamics of scientific revolutions have turned out to be extremely fertile for shedding light on the historical nature of science. *Structure* enabled the development of a philosophy of science with a better historical sensibility and introduced game rules followed even by Kuhn's staunchest detractors. When considered in detail, the specific thesis of the kuhnian historiographical frame can be more or less acceptable. However, after Kuhn, it is no longer possible to address the problems of philosophy of science without considering the historical dimension of science.” (Melongo 2023, 49)

Thus, for Melogno, Daston's interpretation of the historiographical (but also philosophical) character of *Structure* is plausible, but not his accusation of historicism à la Hegel, as

“The historical structures posited by Kuhn are more formal, less ambitious, and clearly weaker than those predominant among historians at the beginning of the twentieth century—and among some historians of science during the nineteenth century [*i.e.* historians who advocated the existence of ineluctable historical laws, like Hegel]. Proposing structures and introducing historical regularities are elements of a project that can adopt different expressions, showing different degrees of formality and rigidity. (...). In other words, Daston's rejection of the historiographical use of the notion of structure requires a specific criticism of the historical patterns assumed by Kuhn, rather than a generic delegitimization of the notion as he used it.” (Melongo 2023, 48)

Melogno also agrees with Wray on the philosophical (and, he adds, also historiographical) fertility of the notion of “structure”.

3. Giri & Giri in the Daston–Wray–Melogno controversy: What is “historicist philosophy of science”?

The Daston-Wray-Melogno controversy is intrinsically interesting and also far from settled. It is not our intention to close it, but to enter into it with our own standpoint. Our position, in a nutshell, attempts to recover and vindicate the tradition of the so-called “historicist philosophy of science” (*i.e.* “new” philosophy of science), a tradition that for mainstream philosophy is inaugurated by the publication of *Structure* (although virtuous and necessary antecedents can be named, in particular Ludwik Fleck, see especially 1979; see also Hoyningen-Huene 1993; Hacking 2012). From this tradition we defend the following strong thesis: every history of science implies an idea about science. This thesis is a specialized version of a more general thesis that could be made explicit in this way: all metascience implies an idea about science. Other specializations of this thesis could then be on the anthropology of science, the sociology of science, the psychology of science, the rhetoric of science, and even the politics of science (this last point was emphasized by Mario Bunge, 1988). Our first step will then be to justify this thesis and to point out in what sense its support implies a collision with Daston’s position. We will then conclude by stating our position on the difference between philosophy and history of science, in order to contrast our position with that of Wray and Melogno.

3.1. *Metascience and science*

Daston’s statement, “The very idea of looking for overarching regularities in the history of science seems bizarre, a kind of leftover Hegelianism seeking a hidden, inexorable logic in the apparent vagaries of history” (2016, 117) is, in our view, the *locus* of the polemic. However, the accusation is not entirely novel. Feyerabend accused Kuhn in *Structure* of proposing a dialectical and rigid theory of science in the Hegelian sense (see Hoyningen-Huene, 1995), while Graham directly asserted that the Kuhnian proposal is a “philosophical history in the Hegelian style” (1997, 127), Reynolds (1999) asserted that Kuhn’s thesis of scientific revolutions

could be classified as a form of Popperian historicism (*i.e.*, the type Popper criticized, including the Hegelian variety), and Bird (2015) identified two "historicist strands" in Kuhn's historiography of science (a "conservative" and a "determinist"), both of which he claims are coincident with Hegel's historiography, and claimed that the determinist is the one attacked by Popper.⁴

Now, to establish that science has a dynamic structure with certain regularities does not necessarily imply a Hegelian remora. Everything that is not permanent, after all, is born, develops and disappears. Philosophers of science have analyzed and polemicized about the way in which this happens, and from reading their works it is more or less clear, implicitly or explicitly, that time is a variable to be taken into account. It is trivial but it seems necessary to state it: the birth, development and disappearance of scientific knowledge does not occur in a chronologically infinitesimal temporal instant, but during heterogeneous but never negligible periods of time. What is not trivial is undoubtedly the mechanism (or mechanisms) by which such changes occur, and it can be affirmed either that they always occur in the same way or that they never occur in the same way, or, maybe, some intermediate position (where, according to certain factors, change occurs in one way or another, but a taxonomy of types of change can be established). Which position is held depends on a particular idea of science, *i.e.* on a philosophy of science.

If a philosophically informed historian wishes to make a history of science, she may legitimately use whatever idea of science she sees fit as a hypothesis or model of how the events she cares to historicize happened when seeking sources and developing a narrative, and it can hardly be argued that this is a matter of debate. However, if the historian believes

⁴ Wray (2023) subscribes to Bird's idea of the two historicist strands in Kuhnian historiography, although Bird clearly states that, according to his thesis, Hegelian historiography also carries both strands, and that the determinist strand is the one attacked by Popper. Wray notes, "Significantly, Bird's characterization of the two dimensions of Kuhn's historicism are not the same as the historicism which Popper objected" (2023, 37), a thesis that, unfortunately, he does not justify beyond the fact that Popper himself did not accuse Kuhn of Hegelianism. In our opinion, as will be seen, the determinism implied by the Kuhnian model of theoretical change is too weak to warrant such an accusation.

that her theory of science implies a teleology such that her knowledge allows her to predict a concrete and objective goal or rational end of the history of science, she could be rightfully accused of Hegelianism. On the other hand, if metatheory points out that at some moment a paradigm will enter into crisis due to an accumulation of recalcitrant anomalies and will be replaced in a process called “scientific revolution”, or a research programme will become degenerative and will be gradually abandoned in order to focus resources on more progressive programmes, the accusation would be exaggerated and unfair. The charge does not fit either Kuhn or Lakatos, since none of them claim that their metatheory provides a general law of the course of history, they do not even have any pretensions to predictability other than at a very abstract level.

A relevant analogy would be the following: we all know that people eventually die; a historian working on the biography of a figure from, say, the Roman Empire, knows, whatever the sources say, that her character died at some point, and that that point must be somewhere between his birth and hardly more than 100 years later (much more likely, less than 80). It is also possible to infer that he died either by disease, accident, or murder. These trivial patterns are not enough to accuse a historian of historicism à la Popper, and, we argue, neither are the patterns of historicist philosophers like Kuhn, but also Imre Lakatos, or Larry Laudan: their patterns are sufficiently abstract to prevent any form of general sense of history from being derived from them. Of course, one may suppose that the “structure” ascribed to science or its parts by some of these philosophers is more fertile or less fertile for historiographical work because of its suitability to the sources or the kind of narrative desired,⁵ but that would not constitute an attack on the idea that science possesses something like a structure, but on particular structures.

⁵ It is true that the data yielded by historical sources can be accommodated to some extent to match the constraints provided by metatheoretical structures. On the other hand, it is also true that structures can (and should) be made more flexible to better accommodate such data. However, if a philosophical structure must be twisted too much to accommodate the data, or if the data must be heavily altered to fit the structure, we have reason to doubt the fertility of the structure for historiographical work, and hence also for philosophical research (see Nickles 1986; Moulines 1986).

Let us look at another situation. Suppose a radical historicist historian à la Daston (*i.e.*, a historian of contingencies), who explicitly assumes that science has no structures or patterns, and who does not believe that the work of philosophers of science is anything more than an intellectual challenge on the same level of importance as solving crossword puzzles. This historian works in the manner indicated by Daston: she pays attention to the infinite contextual details of the facts of the selected time snippet and deliberately ignores the patterns that science may have. This type of historian, as Wray and Melogno acknowledge, not only exists but is even in the majority, especially within Kuhn's number one profile of the historian of science (see 1977a, b, c; e.g. historians coming from history majors, see also Giri & Giri 2020), as we will discuss later. Daston is absolutely right in her sociological analysis of the community of historians coming from history majors. However, denying that there are patterns in science that are useful for historiographical work implies affirming that science is a practice where contingencies are the most relevant thing, and that is a strong thesis about science.

In other words, according to this thesis, each historical event in the history of science is singular in such a way that it is not possible (or interesting) to group it with other events in order to obtain diachronic patterns. However, it turns out that scientists do things like propose hypotheses and test them experimentally, publish results and engage in controversy. It can be argued that the way these things happen is never the same, but to hold that they happen already establishes the existence of a kind of structure,⁶ which may be admittedly ephemeral and contingent, but by no means non-existent. To hold such a thesis does not, of course, amount to having no thesis about science, but rather the opposite: it amounts to holding a thesis about science (*i.e.* a philosophy of science) whose structure is ephemeral, variable and asystematic to such a degree that comparison is impossible or uninteresting because of

⁶ We are understanding here the notion of "structure of science" as it is understood by Wray and Melogno, *i.e.* as the form or sequence of the process in which the scientific dynamic occurs: in its Popperian form it would occur, for example, as a process of hypothesis formulation and bold attempts to falsify them (which at first the hypothesis stubbornly resist by showing their mettle, and finally fall down during the conduct of the crucial experiment).

its triviality. It is clear that such a historian is not a Hegelian, but neither is she exempt from a thesis about science, namely that science is a practice whose events are so unique that they cannot be fruitfully compared.

There is also another type of historian, much less sophisticated, who simply has no philosophical idea about science, and merely narrates it by more or less arbitrarily selecting facts to accommodate a certain narrative idea. Philosophical analysis of her work, however, would allow us to reconstruct an implicit idea of science, which might be asystematic and contradictory but by no means non-existent: this kind of hypothetical historian would presumably be dangerous in conveying asystematic and contradictory ideas about science, and we believe that education in philosophy of science should combat such approaches.

Returning to our sophisticated contingentist historian at the beginning of our argument, while we admit the legitimacy of such a position (without sharing it), we deny that it implies a denial of any structure in science. Indeed, we deny from our initial thesis that it is possible to do any kind of meta-scientific study without an idea of what science is and what its component parts are (which is itself a minimal notion of what a structure of science is). Thus, the contingentist should not accuse someone who uses some structure as a framework or model for her historiographical work of being a Hegelian, but reserve, like Popper (1991; 2011), such a label for those who think they can understand History's overall meaning.

Having said all this, unlike the criticisms of Wray and Melogno, we think very interesting to incorporate into our analysis Daston's proposal to reunite the history of science with philosophy and sociology through the "soft" (and spongy) core of *Structure*, namely, the paradigmatic exemplars (already present in the original edition but baptized in the postscript of '69 (Kuhn 1996) following Masterman's (1970) criticisms of the ambiguity of the term "paradigm"). As stated above, Daston argues that this unit of analysis is richer than the paradigm as a disciplinary matrix because it better reflects the nature of scientific practice and how aspiring scientists learn their craft. This is not the space to discuss this proposal, but we do not find it dismissible. In fact, in Giri & Giri (2020), it is argued that the most prolific version of Kuhnian historiography does not occur in *Structure* but in an previously unpublished work recently recovered and published only in Spanish, *Scientific Development and Lexical Change*

(Kuhn 2017),⁷ a work that draws from his Thalheimer Lectures of 1984. However, we do want to highlight the following point: if the “soft” structure of science (built on paradigmatic exemplars) does not imply for Daston a carrier of Hegelianism, her concern would not be with all structures *per se*, but with the particular structure upheld in the idea of paradigmatic cycles in *Structure*. It is a legitimate part of philosophical and historiographical work to consider which structures are most suitable for designing narratives in the history of science, but it is not legitimate to accuse any work explicitly supporting a specific structure as being Hegelian.

3.2. *Once again, the differences between philosophy and history of science*

As we saw in 2.2, Wray (2023) denies the presence of a presupposed historiography in *Structure*, providing a range of arguments to support the purely philosophical nature of the treatise.⁸ We won't deny that Kuhn's intention may have been purely philosophical. Nor will we refute Daston's claims regarding the limited influence of *Structure* on history majors. Our disagreement lies at a more conceptual level, regarding the possibility of making sharp distinctions between philosophy and the history of science. Certainly, the philosophy of science, as a professional discipline, differs in scope and method from the history of science, although they clearly have intimate and profound relationships.

⁷ There, Kuhn systematically considers a philosophy (and, we add, consequently a historiography) of science focused on the analysis of theoretical change through the alteration of taxonomies (which influences phenomena of local incommensurability). The novelty of the work lies in its systematic and comprehensive presentation, but Kuhn's semantic concerns do not emerge only in his Thalheimer Lectures; in fact, they go much further back (see Mayoral, 2023; Melogno and Giri, 2023).

⁸ Although this is not entirely evident, given that, according to the argument, Kuhn presented himself either as a philosopher or as a historian, the citations provided by Wray can be contrasted with others, such as his introduction to the Isenberg Conference in 1968, six years after the publication of *Structure* and a year before drafting his postscript: “I stand before you as a historian of science. My students, for the most part, wish to be historians, not philosophers. And I am a member of the American Association of History, not philosophy” (1977a, 3). For numerous citations and analyses of the intention of Kuhn's project, and a bold transcendental interpretation of it, see Kindi (2005).

The issue of the relationship between history and philosophy of science has been addressed in many works, some of which are already considered classic, especially by historicist philosophers of science. It is enough to recall Lakatos's (1970) famous Kantian paraphrase: "Philosophy of science without history of science is empty; history of science without philosophy of science is blind" (91). Perhaps Lakatos's work most clearly outlines the methodological relationship between the two, emphasizing the importance of historiographical work for the philosopher by providing the sources to be rationally reconstructed and the role of philosophical work for the historian by indicating units of analysis and dynamic patterns to guide the construction of historical narratives. It is clear that certain types of historians (numerically the majority) seem to doubt the fertility of such units of analysis and dynamic patterns for their work. Lakatos would accuse them of creating "blind" historical narratives, but in many cases, what seems to be happening is that such units and patterns are not explicit in the narratives and will only emerge with reconstructive philosophical effort.

Certainly, in discussions regarding the relationships between history and philosophy, as we said before, it is also asserted that, for these relationships to be virtuous, they should not be overly restrictive. In other words, it would be challenging for historical facts, as revealed by sources, to fit into a specific philosophical framework without some degree of flexibility concerning certain parameters (see Nickles 1986; Moulines 1986). However, according to our thesis, some philosophical premises will always be present in historiographical work. In fact, Paul Hoyningen-Huene (2012) terms a set of presuppositions that historians necessarily use to select material to guide the narrative as "philosophical elements of historiographical work," and these include:

"...the usually implicit assumptions about history itself, or about proper historical research and presentation which influence historical work. It is clear that, for example, decisions about the general aims of historiography of science (...), or convictions about the influence of social factors on the content of science, qualify both as criteria of historical relevance and as philosophical elements of the respective historiography." (Hoyningen-Huene 2012, 283).

Having said all this, we find ourselves asking whether *Structure* is a book about philosophy or historiography, but the answer at this point seems trivial: it is both of them. It aims to reveal certain aspects of the nature of science (its structure, dynamics, etc.) and also aims to assert that such structure and dynamics fruitfully model real historical processes of science as revealed by sources⁹. In this regard, our disagreement with Wray (and agreement with Daston and Melogno) regarding the historiographical (but not exclusively historiographical) nature of Thomas Kuhn's classic is evident. We clarify that we are asserting that *Structure* is a book of philosophy and historiography, but not a history book. The historical cases, as interpreted by, among others, Sharrock & Read offer "precious little" (2002), serving as illustrative and persuasive examples without any claim to exhaustiveness. Thus, Kuhn's proposed model of how science progresses in his classic is simultaneously "philosophical" and "historiographical," as it can be used to describe and explain phenomena of theoretical change but also to support certain narratives about specific events in the history of science.

Having stated that, we would like to go beyond the question of the nature of *Structure* to further analyze the relationship between philosophy and the history of science. It is clear that it is possible to engage in philosophy of science without historical sensitivity. Much of the philosophy of the Received View possessed this nature, as it inquired about the abstract structures of certain scientific processes without concern for their adequacy to real historiographical sources, being more normative than descriptive in character (see Kuhn 1996). This philosophy may be accused (and has been) of being "empty," but in any case, it is evident that it can be done. On the other hand, doing the history of science without the philosophy of science seems, after our reflection, much more difficult. We insist, of course, that the scope and method of each discipline are different, but still, we, along with Hoyningen-Huene (2012), affirm that historiographical work requires criteria in its methodology of source selection and narrative construction that are of a philosophical nature

⁹ Without aiming for exhaustiveness, examples of the history of science carried out through a fairly systematic use of the Kuhnian metatheoretical tool can be seen in the approach to the chemical revolution by Chang (2012) or the history of the emergence of the theory of Jay Wright Forrester's *Theory of Dynamic Systems* by Giri (2021).

(although also involving others that are not, such as narrative and factual criteria). Again, it can be acknowledged that many historians may not be aware of the philosophical presuppositions they are considering when doing their work, and they may remain implicit, but that does not mean they do not exist.

Let's look at an example of a statement extracted from *Structure*:

"Just because the emergence of a new theory breaks with one tradition of scientific practice and introduces a new one conducted under different rules and within a different universe of discourse, it is likely to occur only when the first tradition is felt to have gone badly astray." (Kuhn 1996, 85-86)

Is it a philosophical or historiographical statement? It seems more like the former, as it looks like a generalization drawn from historical cases in which the resistance of scientists immersed in a theory prevented the adoption of a new one until the old theory entered a terminal crisis. Let's rewrite the previous quote to 'historicize' it:

Just because the emergence of Newtonian Mechanics broke with the Aristotelian tradition of scientific practice and introduced a new one conducted under different rules and within a different universe of discourse, it was likely to occur only when Aristotelian Mechanics was felt to have gone badly astray.

Our 'historicizing' method consisted solely of instantiating the previous generalization in a particular case, and indeed, it would not be complex to perform an inverse 'philosophizing' function to go from the second to the first, merely generalizing the particular. However, the fact that our method was successful should not be interpreted as a statement that the difference between the two disciplines is merely a difference between the general and the particular, or between the normative and the descriptive. What should be understood, in our view, is that the premises of the historicist philosophy of science, of which the previous statement is just a small example, although trivially philosophical, have clear and intentional historiographical implications. Therefore, it becomes an unfruitful task

to separate the philosophical from the historiographical in the works of authors like Kuhn, Lakatos, Laudan, Kitcher, and others. It only makes sense in obvious cases, but the most interesting aspects lie in the unclassifiable intermediate gray areas.

However, it is worth to note that the argument we have presented here not only sets us apart from Daston but also diverges from Kuhn himself. While he excelled as both a philosopher and a historian, Kuhn argued that these two activities should be sharply separated (see Kindi 2005, 496), even if there might be some kind of inter-fertilization. We believe that Kuhn, by fluidly switching roles, inadvertently pointed the way to undermine the strict separation between the two disciplines. An example of this is found in his work *Black-Body Theory and Quantum Discontinuity*, where the philosophical concepts of *Structure* are conspicuously absent. In fact, in his Afterword, he himself recommends avoiding philosophical terminology in historical work (Kuhn 1987), and scholars like Klein, Shimony & Pinch (1979) had acknowledged conceptual incongruities between the two works. However, Hacking asserts, "Note, however, that he often said in conversation that '*Black-Body and the Quantum Discontinuity*,' a study of the first quantum revolution launched by Max Planck at the end of the nineteenth century, is an exact example of what *Structure* is all about" (Hacking 2012, 6).

On the other hand, Kuhn himself also suggested that, "Often I do not know for some time after my historical work is completed the respects in which it does and does not fit *Structure*. Nevertheless, when I do look back, I have generally been well satisfied by the extent to which my narrative fits the developmental schema that *Structure* provides" (Kuhn 1987, 363).¹⁰ Considering these quotes, although *Black-Body* may not have been explicitly done using *a priori* the *Structure* model as a

¹⁰ We do not quote this passage extensively, crucial as it is, for synthesis, but here Kuhn summarizes how the concepts of *Structure* apply to the narrative of *Black-Body*, and after that states "These illustrations of the substantive applicability of *Structure* can be extended, but, for this paper, it is the book's historiographic applications that are relevant" (1987, 364). Kuhn's statements in the Afterword of *Black-Body* not only support Hacking's claim that Kuhn regarded it as a narrative strongly compatible with the philosophical model of theoretical change in *Structure* but, as we assert here, Kuhn also considers his model applicable to historiography.

historiographical tool, the fit was sufficient for Kuhn to be satisfied, to the point that it is deemed “an exact example of what *Structure* is all about”, at least *a posteriori*. We won’t attempt to assert how Kuhn was able to unintentionally achieve an exemplary instantiation of the Kuhnian model but will limit ourselves to affirming that this integration between history and philosophy exemplified by *Black-Body* is what we defended in these pages.

This issue has an interesting interpretation by appealing to Kuhn’s taxonomy of profiles of historians of science, which we have already referenced (Kuhn 1977a, b, c). Kuhn distinguished two profiles of historians of science: type I had been trained as historians and dedicated themselves to science as a subdiscipline. It is clear that this is the type of historian, a historicist, described in detail by Daston (2016) in her anti-structure proclamation. Additionally, Kuhn identified a type II profile: those trained as scientists who later worked on the history of the disciplines in which they were experts (these concerned Kuhn due to their Whig tendency, although he also recognized the utility of this profile for the training of scientists, see 1963).

In Giri & Giri (2020), a third profile of historian is described, one of “individuals trained in the philosophy of science who, based on some epistemological profile preference, have begun to delve into the past, generating fruitful works in the history of science” (2020, 79). This philosopher/historian is undoubtedly chameleonic, making it not worth classifying as one or the other. What is worth emphasizing, however, is the legitimacy and fruitfulness of their work. Kuhn is undoubtedly one of the most emblematic scholars belonging to this profile, even though he denied its fertility; we assert that he embodied it in an exemplary manner.

4. Final remarks

This work has aimed to recover the most relevant notes from the fascinating Daston-Wray-Melogno controversy, particularly regarding the relevance of abstracting a structure for science in historiographical work. We have argued that it is not possible to conduct the history of science without an idea of what science is, and any conceptualization of science involves a

description of its parts, relationships, and dynamics, ultimately constituting a minimal notion of "structure." As a corollary, every historian presupposes a structure of science in their historiographical work, even if not necessarily made explicit.

We find Daston's accusation of a Hegelian remnant in *Structure*, as well as Wray's disdain for the historiographical value assigned to Kuhn's classic work (in agreement with Melogno on both criticisms), unfair. At the same time, we celebrate the controversy as an opportunity to re-explore classic themes that emerged with the popularization of historicist philosophy, especially regarding the methodological value of explicating structures during historiographical analyses and the relationship between history and philosophy of science.

We have defended the relevance of using philosophical rational reconstructions for the history of science as a typical method of a third profile of historian coming from the disciplinary area of the philosophy of science. We have also asserted that, while history and philosophy of science are different disciplines, their relationship is so close, especially in historicist philosophy of science, that certain statements can be read as belonging to either discipline or easily lead to equivalent statements in the opposing discipline.

As a corollary to all of this, it is cause for celebration to revive these classic discussions and seek dialogue, albeit critical, among different profiles of historians, which will ultimately contribute to better historiographical work. At the centenary of his birth, it is also valuable to reclaim the legacy of one of the great authors of our time through the critical discussion of his perpetually relevant contributions.

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All links were verified by the editors and found to be functioning before the publication of this text in 2024.

DECLARATION OF CONFLICTING INTERESTS

The authors declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

FUNDING

The authors received no financial support for the research, authorship, and/or publication of this article.

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