
Kuhn, Condorcet, and Comte: On the Justification of the “Old” Historiography of Science

J. C. Pinto de Oliveira

*Department of Philosophy, IFCH,
State University of Campinas, Brazil*

Despite the importance of the “historiographical revolution” in Kuhn’s work, he did not carry out a specific study about it. Without a systematic investigation into it, he even affirms that the “old” historiography of science (OHS) is unhistorical, suggesting its summary disqualification in the face of his “new historiography” of science (NHS). My wider project, of which this paper is a part, is to better discuss the issue of the justification of the NHS. In this paper, I discuss the justification (and the genesis) of the OHS, focusing on Condorcet and Comte and resorting especially to Koyré. This will allow us to understand that the relation between the OHS and the NHS is a new instance of inter-theoretical incommensurability. And, indeed, that the NHS is not strictly a new historiography. It is the same historiography used for other disciplines (art, philosophy), which in the twentieth century begins to be applied to science as well.

1. Introduction

Kuhn said that *The Structure of Scientific Revolutions* (thereafter *Structure*) depends on the “new historiography” of science (Kuhn, 1977, p. xv, 1970a, p. 3). Despite its importance in his work, the change from the “old” historiography of science to the new historiography of science did not

I am grateful to all those who read and commented on earlier drafts of this paper. I thank especially Amelia J. Oliveira and two anonymous reviewers for their suggestions, as well as the editor Alex Levine. I would like to thank also Baruana Calado for translations and revisions.

deserve a specific study from Kuhn.¹ Without a systematic investigation into it, he even affirms or insinuates in *Structure*, as well as in other texts, that the “old” historiography of science would be unhistorical (see Kuhn 1970a, p. 1, 1970b, p. 67, 1977, p. 150).

This idea is also present in other authors’ writings. Helge Kragh, for example, echoing the Kuhnian critique, writes that Sarton’s view “was, at least by modern standards, somewhat naive and surprisingly ahistorical.” He cites Rupert Hall who asks, “with all respect” regarding Sarton, “if he was ever an historian at all,” and also Herbert Butterfield, who speaks of Whig historiography as “unhistorical history writing” (Kragh 1987, pp. 18, 93, 198n43).²

The suggestion that the OHS would be unhistorical leaves another inaccurate idea “in the air”: that the change from the OHS to the NHS would have simply been a change from an unhistorical to a historical historiography. More than an explanation of what would have happened in the historiography of science, this idea seems to be an indicator that the topic of moving from the OHS to the NHS deserves more attention.

This is very clear if one takes into account another and very different Kuhn’s suggestion on the subject, which I will follow here. He refers to the shift from the OHS to the NHS as an “historiographical revolution” (Kuhn 1970a, p. 3, Kuhn 1970b, pp. 67, 69), which points to a more complex relationship between the two historiographies. Separated by a revolution, the OHS and the NHS would be incommensurable.

My wider project, of which this paper is a part, is to discuss better the issue of the change from the OHS to the NHS and the justification of the NHS. What this article seeks to do is try to make explicit a justification for the OHS, for the way to write the history of science that Kuhn himself identifies with a tradition, which he says goes from Condorcet and Comte to Dampier and Sarton.

The article then focuses on Condorcet and Comte, the earliest authors, as a reference for understanding the genesis and justification of this conception. OHS is sketched from direct authors’ references on how to write the history of science (with emphasis on progress) or through the way they write it (Section 2). With this in mind, I then try to identify more precisely Kuhn’s critiques of the OHS, which lead him to propose a “new historiography” (Section 3). In the last section, I draw some conclusions

1. Kuhn employs the expression “new historiography” of science (e.g., Kuhn 1970a, p. 3) as referring to the historiography opposed to the traditional historiography of science, which I name “old” historiography of science. Throughout the text I refer to them as NHS and OHS, respectively.

2. Sarton may be considered one of the main representatives of OHS or of Whig history of science. See note 3 below.

and raise some questions, from what has been considered, which, I believe, will pave the way and contribute to the future discussion of the NHS justification. Also, an explanation for Kuhn's own attitude of disqualifying the OHS as unhistorical is outlined.

2. Condorcet, Comte, and the Justification of the OHS

Curiously, although the entire work is permeated by the issue, none of the OHS adepts' names are mentioned in *Structure*. But one can find Kuhn's direct reference in *The Essential Tension*. He speaks of "an almost continuous tradition from Condorcet and Comte to Dampier and Sarton," which "viewed scientific advance as the triumph of reason over primitive superstition, the unique example of humanity operating in its highest mode" (Kuhn 1977, p. 148; see also p. 106).³

Thus, Kuhn signals the eighteenth century with Condorcet as the moment of emergence of the OHS and at such point I will initially concentrate my attention. Condorcet's main work in this regard, *Outlines of an Historical View of the Progress of the Human Mind*, was published posthumously in French in 1795. Condorcet divides his historical frame into ten periods, covering the remote past, the present, and even the future. At the same time a protagonist and a victim of the French Revolution, he announces his optimistic message in these terms:

This picture, therefore, is historical; since subjected as it will be to perpetual variations, it is formed by the successive observation of human societies at the different eras through which they have passed. It will accordingly exhibit the order in which the changes have taken place, explain the influence of every past period upon that which follows it, and thus show, by the modifications which the human species has experienced, in its incessant renovation through the immensity of ages, the course which it has pursued, and the steps which it has advanced towards knowledge and happiness. From these observations on what man has heretofore been, and what he is at present, we shall be led to the means of securing and of accelerating the still further progress, of which, from his nature, we may indulge the hope. Such is the object of the work I have undertaken (...)
(Condorcet 2011, pp. 9–10)

Kuhn merely mentions Condorcet's and Comte's names, which leads us to resort to Koyré's work in order to summarize the perspective of the *Outlines*

3. Here I investigate Condorcet and Comte. On Sarton, at the other end of the spectrum, see Pinto de Oliveira and Oliveira 2018.

and, as a matter of fact, as a reference throughout this paper. His work on the Scientific Revolution is comprehensive and very important to the twentieth century's historiography of science, directly influencing Kuhn's thought. Besides, Koyré is a critic of Comte and has a very suggestive paper on Condorcet, which might have been Kuhn's source given that it was published in English in 1948. Koyré properly contextualizes Condorcet's work and the very Enlightenment historiography, and it is through him that we can begin to outline a justification for the OHS.⁴

According to Koyré, history, in the conception of the eighteenth-century Enlightenment,

is not something which makes us, but something which we make, which is the entirety of things which man has made, which he is making, and which he is going to—or can—make. Therefore, and this follows from this activist attitude, the historian does not look to the past but to the future; and what he has to relate, what he finds most precious in history, is nothing else but the history of progress, that is to say, the story of the progressive liberation of the human spirit, the story of its fight against the forces—ignorance, prejudice, etc., etc.—which oppress and which have oppressed it, the story of the gradual conquest by man of his Enlightenment—of his liberty in the truth. (Koyré 1948, p. 134; compare Kuhn 1970a, pp. 1–2, quoted below.)

And Koyré continues, directly referring to Condorcet:

The philosophy of the eighteenth century—a meritorious feature—not only wanted to explain the world; it wanted also to transform it. It even believed that it could transform the world by explaining it, in other words, that it was necessary only to show men what was true and what was false—they would invariably tend toward the truth. But it felt that history supported this faith in the power of truth and of reason: isn't it true, as Condorcet writes, that in spite of all the obstacles which have blocked its advance, humanity, in its sum total, has achieved an almost constant ascent? (...) Thus the optimism of Condorcet is a reasoned optimism, and, as a matter of fact, an empirical one. Progress is by no means inevitable and fatal. But the history of humanity shows that it is real. (Koyré 1948, pp. 134–135)

4. On Kuhn and Koyré see Pinto de Oliveira 2012. Also see the chapter on Kuhn and Sarton in the book on Koyré (Pinto de Oliveira and Oliveira 2018). It can be said that Koyré regarding Sarton has the same concerns with tolerance and contextualization he demonstrates with respect to the Enlightenment historiography.

Still according to Koyré, Condorcet, as a *philosophe* actively involved with the French Revolution, does not separate intellectual progress from moral progress. More than that, “he believes that intellectual progress implies and conditions moral progress” (Koyré 1948, p. 140; see also Condorcet 2011, p. 112). And the whole eighteenth century is delighted with Newton’s progress in knowledge. Not only the philosophers of the French Enlightenment, such as Condorcet himself, Diderot, D’Alembert, and Voltaire (who wrote a successful work of disseminating Newton’s *Principia*), but also Locke, Hume, and Kant. In this regard, Koyré writes elsewhere:

No wonder that (in a curious mingling with Locke’s philosophy) Newtonianism became the scientific creed of the eighteenth century, and that already for his younger contemporaries, but especially for posterity, Newton appeared as a superhuman being who, once and for ever, solved the riddle of the universe. (...) Indeed, as Lagrange somewhat wistfully put it, there being only one universe to be explained, nobody could repeat the act of Newton, the luckiest of mortals.

Small wonder that, at the end of the eighteenth century, the century that witnessed the unfettered progress of Newtonian science, Pope could exclaim: “Nature and nature’s laws lay hid in night: God said, Let Newton be! and all was light.” (Koyré 1965, pp. 18–19)⁵

It is in this historical context, the context of its genesis, that the OHS would be justified. Given the way science was then understood, the OHS would be justified as the general historiography of the eighteenth century is justified to Koyré, and even more since the “history of progress” precisely focuses on science (See Koyré 1948, p. 135, quoted above).

In other words: If, as Koyré says, the eighteenth century’s historiography is justified as a way of writing history that takes into account the progress observed in society, the historiography of science would be justified in the same way, and *a fortiori* given the fact that it is particularly in science that the most expressive progress, cumulative progress, occurs. This seems to be the justification core of the OHS. Faced with the cumulative progress which was then observed in science, and upon which there was broad consensus, the historiography of science becomes the way of writing history of science that takes this cumulative progress as its leitmotif, its guiding principle.

5. Of course, Koyré does not fail to observe Newton’s difficulties with his theory, such as with action at a distance (he does this even in the pages immediately preceding the passage quoted). The same is true regarding Kuhn (see, e.g., Kuhn 1970a, pp. 104–5).

The justification core would comprise the emergence and consolidation of the Newtonian theory, a period of about 100 years in the Enlightenment perspective. As Condorcet himself writes about Newton:

Thus we see man has at last become acquainted, for the first time, with one of the physical laws of the universe. Hitherto it stands unparalleled, as does the glory of him who discovered it. A *hundred years* of labour and investigation have confirmed this law, to which all the celestial phenomena are subjected, with an accuracy which may be said to be miraculous. Every time in which an apparent deviation has presented itself, the transient uncertainty has soon become a subject of new triumph to the science. (Condorcet 2011, p. 104, my emphasis)⁶

This process (this “incontestable and even spectacular” progress) is traditionally associated with the autonomy of science and, consequently, of its own history and historiography, as Koyré points out. According to him, it is under the influence of eighteenth-century philosophy that history becomes a history of progress and so must be told:

In his beautiful account of the development of history—I refer again to the history of historians—Mr. Guerlac draws our attention to the widening scope of this subject in modern times, especially since the eighteenth century. (...) Under the influence of the philosophy of the enlightenment, history becomes that of “the progress of the human mind”: let us remind ourselves of Condorcet whom, curiously, Mr. Guerlac has omitted to mention. Thus it is natural that it should have been during the eighteenth century that history of science, a field in which this progress was incontestable and even spectacular, constitutes itself as an independent discipline (Koyré 1963, p. 849; see also Koyré 1965, p. 19)⁷

And could one not think of an entirely natural expansion of the justification core, accompanying the very development of Newtonian science? Condorcet, as we have seen, writing at the end of the eighteenth century,

6. And D’Alembert writes that “le vrai système du monde a été connu, développé et perfectionné” (“the true system of the world has been known, developed and perfected”) (D’Alembert 1986, p. 10. See the entire passage).

7. In an added note to this passage, Koyré writes: “Contrary to widespread opinion which regards it as anti-historical, it is the eighteenth century that gave birth to our historiography.” On the idea of autonomy of science in the Enlightenment see Condorcet 2011, pp. 94, 111–112; Voltaire 1967, p. 194, 1977, pp. 1246–47 (letter 1028); D’Alembert 1986, pp. 38, 48–49. See also Casini 1983, chaps. II, IV.

speaks of 100 years. In the same sense, Koyré speaks about the more than 200 years of success in the *Newtonian Studies*:

Besides, all of us, or if not all still most of us, have been born and bred (...) in the Newtonian or, at least, a semi-Newtonian world, and we have all, or nearly all, accepted the idea of the Newtonian world machine as the expression of the true picture of the universe and the embodiment of scientific truth—this because for more than two hundred years such has been the common creed, the *communis opinio*, of modern science and of enlightened mankind. (Koyré 1965, p. 4)

Indeed, in the mid-nineteenth century, Auguste Comte refers to Newton with the same fervor as the Enlightenment thinkers, as, for example:

...the ultimate perfection of the positive system would be (if such perfection could be hoped for) to represent all phenomena as particular aspects of a single general fact—such as gravitation, for instance. (...) We say that the general phenomena of the universe are explained by it, because it connects under one head the whole immense variety of astronomical facts; exhibiting the constant tendency of atoms towards each other in direct proportion to their masses, and in inverse proportion to the squares of their distances. (Comte 1989, pp. 36 and 39)

Thus, the justification core of the OHS would comprise about 200 years: the 100 years of success—spoken of by Condorcet and other Enlightenment thinkers at the end of the eighteenth century—plus another 100 years of the nineteenth century. Nevertheless, the OHS does not present itself exclusively as the historiography of these 200 years of success. But if one can admit the core justification of the OHS based on the extraordinary success of Newton's theory in 200 years, how then could one assess the adequacy of the OHS beyond that period, both for the future and for the past?

Kuhn's reference to Comte, alongside Condorcet, can be considered here as a clue to understanding the expansion of the OHS and its intended justification. Comte, less historian than philosopher⁸, presents

8. As Cohen writes on Sarton and Comte: "Although he expressed an enormous admiration for Auguste Comte, founder of the positivist philosophy, and the man who—in Sarton's opinion—first conceived the subject of the history of science, he was also careful to point out that Comte did not really know science well and that his knowledge of the history of science was poor" (Cohen 1957, p. 298; see also Sarton 1948, p. 31).

in the nineteenth century his well-known law of the three stages. Enchanted with Newton, as pointed out, Comte seeks an emulation of the universal gravitation theory, presenting his philosophical conception as a general law of the historical development of knowledge (and of society). As Pietro Redondi writes:

Condorcet placed his philosophy of progress in a historical decor, but he did not elaborate the laws of progress (...). Comte had constructed a philosophical system which aspired to offer at the same time a philosophy of history and a logical coordination of the progress of scientific knowledge. (...) With Auguste Comte the history of science thus appeared as logical reconstruction, a rationale of scientific development based on a philosophy of history of an anachronistic and finalist kind. (Redondi 1989, pp. 16–17)

Comte summarizes his own “law of the three stages” in these terms:

From the study of the development of human intelligence, in all directions, and through all times, the discovery arises of a great fundamental law, to which it is necessarily subject, and which has a solid foundation of proof, both in the facts of our organisation and in our historical experience. The law is this: that each of our leading conceptions—each branch of our knowledge—passes successively through three different theoretical conditions; the theological, or fictitious; the metaphysical, or abstract; and the scientific, or positive. In other words, the human mind, by its nature, employs in its progress three methods of philosophising, the character of which is essentially different, and even radically opposed: namely, the theological method, the metaphysical, and the positive. Hence arise three philosophies, or general systems of conceptions on the aggregate of phenomena, each of which excludes the others. The first is the necessary point of departure of the human understanding, and the third is its fixed and definitive state. The second is merely a state of transition. (Comte 1989, pp. 35–6)

Such “great fundamental law,” which Comte claims to be as universal as Newton’s law of gravitation, has given the traditional historian of science, or at least the positivist historian, a generalization, a basis for the extension in time of what I have called the justification core of the OHS.⁹

9. As Koyré says, according to Condorcet the history of humanity shows that progress is real but it “is by no means inevitable and fatal” (Koyré 1948, pp. 134–5).

Concluding this section, one can say that the Newtonian physics' extraordinary success has led to the idea of autonomy of science in relation to philosophy, by delimitating a territory of its own and a concept of science, which was identified with Newton's science. And this idea of autonomy of science ended up promoting the constitution of the history of science itself as an autonomous discipline (See Koyré 1963, p. 849, quoted above). This discipline, which we now identify as OHS, tells the story of how the extraordinary conquest of science happened. A science that, with Newton, as it was believed, "once and for ever, solved the riddle of the universe" (Koyré 1965, p. 18, in passage quoted above).

The OHS tells the story of the process known as the Scientific Revolution, the "history of progress" (Koyré), the history of man's struggle to conquer science against ignorance and superstition, as Kuhn said in *Structure* (Kuhn 1970a, pp. 1–2, quoted in the next section). Or the history of overcoming the stages of religion and metaphysics, in Comte's terms, with emphasis on the episodes that contributed to the achievement of the scientific or positive stage, which he regarded as excluding and definitive.¹⁰

The OHS would be questioned by a competing historiography, the NHS, only at a time when Newtonian science would not be seen with the same eyes, due to a rupture as represented by the theory of relativity. But it is necessary to remember, as Kuhn does in *Structure*, that classical physics at that time was still considered by some interpreters as a special case of contemporary physics (see below). This is to say that these interpreters did not conceive a rupture between Einstein's physics and Newton's—which would extend the reach of OHS by also including the twentieth century as part of the cumulative development of science.

3. Kuhn's Critiques of OHS

One can say that Kuhn proposes his NHS essentially as a positive counterposition to two negative aspects he saw in the OHS. Firstly, whiggism or anachronism. Secondly, what can be called, for lack of a better denomination, false reduction or false compatibility between theories.¹¹

With regard to whiggism or anachronism, Kuhn writes in *Essential Tension* that the aim of the older histories of science "was to clarify and deepen an understanding of *contemporary* scientific methods or concepts

9. As Koyré says, according to Condorcet the history of humanity shows that progress is real but it "is by no means inevitable and fatal" (Koyré 1948, pp. 134–5).

10. For an interpretation from the NHS perspective, see, for example, Koyré 1971, p. 254.

11. I do not include here Kuhn's considerations that I have associated in Pinto de Oliveira 2012 with the genesis of the NHS. They will be discussed in a further paper on the justification of the NHS.

by displaying their evolution.” To this end, the historian “described when, where, and how the elements that in his day constituted its subject matter and presumptive method had come into being” (Kuhn 1977, p. 107; see also Hoyningen-Huene 2012). And Kuhn is even more explicit in a well-known passage of *Structure*:

If science is the constellation of facts, theories, and methods collected in current texts, then scientists are the men who, successfully or not, have striven to contribute one or another element to that particular constellation. Scientific development becomes the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation. (Kuhn 1970a, pp. 1–2; see the entire paragraph)

In opposition to this, the NHS would naturally offer a non-whiggish approach, a contextualized reading of historical texts. Kuhn directly compares the two approaches in another well-known passage of *Structure*:

Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. They ask, for example, not about the relation of Galileo’s views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinions of that group and other similar ones from the viewpoint—usually very different from that of modern science—that gives those opinions the maximum internal coherence and the closest possible fit to nature. (Kuhn 1970a, p. 3)¹²

With respect to what I consider Kuhn’s second main critique of the OHS—the critique to false reduction or false compatibility between theories—Kuhn expresses it directly through the relations between Newton and Einstein’s theories. He writes in *Structure* that the logical positivist interpretation

12. And that is, according to Kuhn, what Koyré exemplarily does (Kuhn 1970b, p. 68, 1970a, p. 3). On Kuhn and Koyré, see Pinto de Oliveira 2012, especially sections 3 and 4.

would restrict the range and meaning of an accepted theory so that it could not possibly conflict with any later theory that made predictions about some of the same natural phenomena. The best-known and the strongest case for this restricted conception of a scientific theory emerges in discussions of the relation between contemporary Einsteinian dynamics and the older dynamical equations that descend from Newton's *Principia*. (...) The gist of these objections can be developed as follows. Relativistic dynamics cannot have shown Newtonian dynamics to be wrong, for Newtonian dynamics is still used with great success by most engineers and, in selected applications, by many physicists. (...) For example, if Newtonian theory is to provide a good approximate solution, the relative velocities of the bodies considered must be small compared with the velocity of light. Subject to this condition and a few others, Newtonian theory seems to be derivable from Einsteinian, of which it is therefore a special case. But, the objection continues, no theory can possibly conflict with one of its special cases. (Kuhn 1970a, pp. 98–9)

This seems to have been the interpretation that prevailed during Kuhn's formative years as a physicist at Harvard, or at least until 1947. I say this because Kuhn states in the introduction to *The Essential Tension* that he discovered his "first scientific revolution" that year when investigating, from a historical point of view, the relationships between Aristotle's physics and Galileo's and Newton's theories (Kuhn 1977, p. xiii). In *Structure*, however, he already has a critical perspective on the relationship between Newton and Einstein and replies:

Though an out-of-date theory can always be viewed as a special case of its up-to-date successor, it must be transformed for the purpose. And the transformation is one that can be undertaken only with the advantages of hindsight, the explicit guidance of the more recent theory. Furthermore, even if that transformation were a legitimate device to employ in interpreting the older theory, the result of its application would be a theory so restricted that it could only restate what was already known. Because of its economy, that restatement would have utility, but it could not suffice for the guidance of research. (Kuhn 1970a, pp. 102–103)¹³

13. See also pp. 139–40 on Galileo-Newton relation.

Kuhn explicitly refers only to Philip Wiener, who is not an historian, as an example of the traditional conception (Kuhn 1970a, p. 98n2). But he could also criticize an historian for defending such a position. In a passage from the preface to the first edition (1954) of his book on the Scientific Revolution (and, very significantly, excluded from the second edition, published 30 years later), Rupert Hall writes:

However great the revision of ideas of matter, time, space and causality enforced during the last half-century, it was a revision of the content, not the structure of science. In its progress since 1800 the later discoveries have always embraced the earlier: Newton was not proved wrong by Einstein, nor Lavoisier by Rutherford. The formulation of a scientific proposition may be modified, and limitations to its applicability recognized, without affecting its propriety in the context to which it was originally found appropriate. (Hall 1954, p. xiii).¹⁴

Although Hall maintains this position in 1954, Koyré writes in 1951 from a different point of view, closer to Kuhn's *Structure*:

We no longer live in the world of Newtonian ideas, nor even of Maxwellian ideas. That is why we are able to look at them from within and from outside, to analyze their structures, to perceive the causes of their insufficiencies. We are better equipped to understand the sense of medieval speculation about the composition of the continuum and the "latitude of forms", the evolution of the structure of mathematical and physical thought over the last century in its effort to create new forms of reasoning, and the critique of the intuitive, logical, and axiomatic foundations of its validity. (Koyré 1973, p. 15; see the entire passage, pp. 14–15)¹⁵

Reviewing Kuhn's two critiques of the OHS, we might be inclined to say that they do not contemplate what I considered the justification core of

14. Hall's work is cited in *Structure* (p. 67n1), but Kuhn refers to another passage and to another subject (the Copernican revolution).

15. Nous ne vivons plus dans le monde des idées newtoniennes, ni même maxwelliennes, et de ce fait nous sommes capables de les envisager à la fois du dedans et du dehors, d'analyser leurs structures, d'apercevoir les causes de leurs défaillances comme nous sommes mieux armés pour comprendre et le sens des spéculations médiévales sur la composition du continu et la "latitude des formes," et l'évolution de la structure de la pensée mathématique et physique au cours du siècle dernier dans son effort de création de formes nouvelles de raisonnement, et son retour critique sur les fondements intuitifs, logiques, axiomatiques de sa validité.

the OHS, in the sense that one can say that the OHS, on the one hand, and the NHS of normal periods, on the other, converge or overlap as a history of cumulative progress. That is, Kuhn would reject the projection of the justification core in the past (the first critique) and its extension to the future (the second critique), but not the core itself.

Condorcet himself may be summoned to a rapprochement between the justification core of the OHS and Kuhn's concept of normal science. In the passage quoted above, in which he says that a hundred years of work have confirmed Newton's law, Condorcet also points out that "every time in which an apparent deviation has presented itself, the transient uncertainty has soon become a subject of new triumph to the science" (Condorcet 2011, p. 104). And ahead he states:

Natural philosophy, gradually delivered from the vague explanations of Descartes, in the same manner as it before was disembarassed from the absurdities of the schools, is now nothing more than the art of interrogating nature by experiment, for the purpose of afterwards deducing more general facts by computation. (...) so that philosophy *is not only enriched every day with new truths, but the truths already known have been more exactly ascertained; so that not only an immense mass of new facts have been observed and analysed, but the whole has been submitted in detail to methods of greater strictness.* (Condorcet 2011, pp. 105–106, my emphasis; see also p. 133)

Kuhn, in his turn, in dealing with normal science in *Structure*, makes extensive use of the case of Newtonian theory. According to him, "no other work known to the history of science has simultaneously permitted so large an increase in both the scope and precision of research" (Kuhn 1970a, p. 30). This development was a task of Newtonian normal science, which advanced in the nineteenth century (see Kuhn 1970a, pp. 30–34).

Thus, Condorcet presents a description that could be considered typical of the development of a Kuhnian normal science. So Kuhn, at least in principle, could not refuse what we have called the justification core of the OHS, about two hundred years of success, corresponding to the development of what would be Newtonian normal science.

However, although Kuhn does not expressly refer to it in *Structure*, it is possible to show that he also makes serious restrictions to the justification core of the OHS. He seems to reject the idea that it can be understood as the history or description of Newtonian normal science. Essentially, what Kuhn writes about Einstein and Newton's physics in *Structure* would apply to the relationship between the NHS and the OHS:

Though an out-of-date theory can always be viewed as a special case of its up-to-date successor, it must be transformed for the purpose. And the transformation is one that can be undertaken only with the advantages of hindsight, the explicit guidance of the more recent theory. (Kuhn 1970a, pp. 102–103)

In later texts he seems to reject the idea that the justification core of the OHS can be understood as the history or description of Newtonian normal science. He says in *Encounter* that for long years the scientific development was viewed as the routine result of applying “the scientific method” (Kuhn 1970b, p. 67). In another text from the same period, reinforcing the quotation marks in “the scientific method,” Kuhn even speaks of “the myth of method” (Kuhn 1977, p. 150).¹⁶

When he uses this expression, he disqualifies the OHS, not only as being incapable of dealing specifically with normal Newtonian science, but also because of its inability to account for the development of science as a whole. He speaks of “the myth of method” precisely to restrict OHS’s idea that the Scientific Revolution would mean the ultimate conquest of *the* scientific method. And it would then be necessary to refer explicitly to the NHS perspective to determine the cut, establish the *mutatis mutandis* adequacy limit of traditional historiography. It would apply to normal Newtonian science only at the given limit and with the constraints determined by the NHS.

Thus, Kuhn’s general critique of the OHS is completed by denial of the very justification core of the OHS. Kuhn denies that the OHS would be a special case of the NHS, that the core should be viewed *simpliciter* as the Newtonian normal science. For Kuhn, the OHS sees the Newtonian science superlatively as *the* science (and the conquest of *the* scientific method) and recognizes it as the culmination of *the* Scientific Revolution, not as a scientific revolution among others scientific revolutions.

4. Final Considerations

Kuhn does not seem to do justice to the OHS when he calls it unhistorical, suggesting a summary disqualification in the face of the NHS, which would be effectively historical. As Cassirer (to whom Koyré refers in his paper on Condorcet) observes regarding the vision of romanticism about the eighteenth century:

16. This idea is amplified in a long passage of *The Road since Structure*, of which a partial quotation does not avail (see Kuhn 2000, pp. 107–109, 119).

The common opinion that the eighteenth century was an “unhistorical” century, is not and cannot be historically justified. This opinion is rather a battle cry coined by the Romantic Movement when it entered the field against the philosophy of the Enlightenment. (...) This movement, which devotes itself so wholeheartedly to the past in order to grasp its pristine reality, fails to live up to its ideal when it encounters that past with which it is still in direct contact. The principle which it establishes for the historically remote proves unmanageable when applied to the immediate past. Romanticism was historically blind to the generation of its own fathers. It never attempted to judge the Enlightenment by its own standards, and it was unable to view without polemical bias the conception of the historical world which the eighteenth century had formulated. (Cassirer 1951, pp. 197–8)

Despite his concerns with contextualization as a historian, Kuhn, absorbed in his campaign in favor of the NHS, does not seem to give proper attention to the historical context of OHS. He could be a victim of historical blindness, as Cassirer puts it, or, getting to a more specific diagnosis, historical presbyopia, which refers to the difficulty of seeing up close. He himself seems to admit this when he writes about another subject (the isolation of the history of science from general history as disciplines):

My topic is one I have lived with rather than studied. The data I bring to its analysis are correspondingly personal and impressionistic rather than systematic (...). Partisanship I shall try to avoid, but without hope of entire success, for I take up the subject as an advocate, a man much concerned with some central impediments to the development and exploitation of his special field. (Kuhn 1977, p. 128)

It is not very interesting, in this context, to discuss whether Kuhn is a romantic or not, as David Bloor suggests (Bloor 1991, p. 62; see also Pinto de Oliveira 2012, p.121). Both Kuhn and the romantics seem to have a common, more general type of blindness or historical presbyopia that would affect all historical agents involved in a debate or who are interested in the results of the ongoing historical process. They are in direct dispute with their interlocutors and do not act towards them in a manner consistent with their general discourse.

And more than that, it should be remembered that in *Structure* Kuhn writes:

Like the choice between competing political institutions, that between competing paradigms proves to be a choice between

incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense. The resulting circularity does not, of course, make the arguments wrong or even ineffectual. (Kuhn 1970a, p. 94)

While it may be said that some historical presbyopia has been manifested in the case of the historiographies of science, I think it is fairer to say that Kuhn, when he considers the OHS as unhistorical, is in fact pronouncing himself from the point of view of the NHS. Perfectly inserted in the context of the passage just quoted, Kuhn would only be stating his own "paradigm" (as romantics did with theirs). The OHS is unhistorical in the perspective of the NHS. This means, as we have already pointed out, that Kuhn disqualifies the anachronism of the projection into the past, the extension to the future of the justification core, and the very core of the OHS.

Thus, if the OHS has the right, in its own view, to be "whiggish" (with quotation marks, because from its point of view there is a continuity of past and present, strict cumulative progress in science), Kuhn has the right, from the NHS's point of view, to consider the OHS as unhistorical.

Therefore, it would not be a psychological issue, from mere human fallibility—the inability of the historical agent to elevate reason above his interests (historical presbyopia)—, but from something broader and more complex (see Kuhn 2000, p. 108). What happens in science, according to Kuhn, is that the use of the paradigm to defend the paradigm is an inseparable part of the complex situation of choice (between incommensurable theories).

And one can observe, in the historiography of science as well, with all its consequences, a new instance of inter-theoretical incommensurability.¹⁷ A critique of Kuhn by the Popperian Gerard Radnitzky reveals the nature of the inter-theoretical rupture in the historiography of science. He does not deny the fact that Kuhn does justice to what he calls "obvious sense of historicity" but maintains that Kuhn does not recognize the historicity proper to science. Radnitzky writes:

17. Kuhn himself says of a "historiographical revolution" (see section 1 above). On incommensurability and Kuhn's use of the concept out of the context of science see Oberheim and Hoyningen-Huene 2018 (section 2.2.4) and Pinto de Oliveira 2017.

Like any human activity, scientific research has a historical dimension, it is changing and it is contingent (...) This is a trivial observation. Over and above this obvious sense of historicity science is a historical phenomenon in a deeper sense, *a sense in which art, literature, political institutions etc. are not*. Science aims at cognitive progress, at the growth and improvement of knowledge, and the idea of cognitive progress is constitutive of the meaning of science. Moreover it is the only field where there can be no doubt that there has been and *is* progress. The relationship between the passage of time and the growth of knowledge is not a random affair (Radnitzky 1982, pp. 55–56, my emphasis).

Radnitzky respectively speaks of “trivial historicity” and “essential historicity,” a superficial sense and a deeper sense of historicity (pp. 63, 71), and this conception is counterposed by Kuhn’s following passage in *Essential Tension*:

Seen through their writings [new historians’ ones], science is not the same enterprise as the one represented in either of the older traditions. For the first time it has become potentially *a fully historical enterprise, like music, literature, philosophy, or law*. (Kuhn 1977, p. 150, my emphasis)¹⁸

Thus, from Kuhn and other new historians’ standpoint, science is not “superlunary,” to use the picturesque expression of Aristotle’s cosmology, and should be treated without privilege, in the same way as the other (“sublunary”) disciplines.¹⁹ In fact, the NHS is not *stricto sensu* a new historiography. It is the same historiography used for other disciplines, which in the twentieth century begins to be applied to science as well. In fact, it is the ‘old’ historiography of science that presents itself as new among the historical disciplines, defined by a new and very special type of object: science. Science would be a privileged discipline from the epistemological point of view, the natural place of objectivity, rationality and progress, and thus deserving of an equally special historiography, a special way of telling its history.

18. About the relation of science to art and other disciplines, see Pinto de Oliveira 2017.

19. “According to Aristotle, the underside of the sphere of the moon divides the universe into two totally disparate regions, filled with different sorts of matter and subject to different laws. The terrestrial region in which man lives is the region of variety and change, birth and death, generation and corruption. The celestial region is, in contrast, eternal and changeless” (Kuhn 1995, p. 91).

We must go back to what was exposed in section 2 of this article to understand this idea in its proper context. We have seen that, given the extraordinary success of Newton's physics, Condorcet and others speak in the eighteenth century of an autonomy of science in relation to philosophy. And Comte and the positivists, in the following century, speak of the emergence of the "scientific or positive stage," definitively overcoming, as Comte stresses, the religious and metaphysical stages of human knowledge.

We can say that for Kuhn the idea of science does not imply that philosophy (or metaphysics) lagged behind, as an outdated stage of the development of knowledge. There is no autonomy of science in relation to philosophy in the face of the success of Newton's theory. There is a reversal from this perspective in twentieth-century science. In fact, for Kuhn, science is, as it was in the seventeenth and eighteenth centuries, embedded in philosophy. Not in its more pragmatic institutional aspects, of course, but from an epistemological point of view.²⁰ This is also what Koyré thinks, as can be seen in the book by Gérard Jorland, significantly named *La Science dans la Philosophie*.²¹ Jorland writes:

The unity of thought, as conceived by Koyré in his study on scientific revolution of the seventeenth century, is the meeting or a mixture of science, philosophy, and theology, that is, of physics, metaphysics, and theology. What positivism establishes in history as stages or states of development of thought, Alexandre Koyré gathers as a unit. The principle of all his investigations is, therefore, an antipositivist principle. (Jorland 1981, p. 50)

And Koyré himself writes in a text in which he criticizes the positivist Philip Frank:

the history of scientific thought teaches us therefore (at least I will try to sustain this): 1. That scientific thought has never been entirely separated from philosophical thought; 2. That the great scientific evolutions have always been determined by subversions or changes of philosophical conceptions; 3. That scientific thought—I speak of the physical sciences—does not develop *in vacuo*, but is always within a

20. Considering the caveat, this is not incompatible with what Kuhn writes in "The History and the Philosophy of Science": "Until the end of the seventeenth century, much of science was philosophy. After the disciplines separated, they continued to interact in often consequential ways" (Kuhn 1977, p. 10).

21. See also Larvor 2003, p. 381. One of the anonymous reviewers called my attention to this text. I was already acquainted with it, but I did not remember some of Larvor's remarks about Koyré (which include a reference to Jorland), similar to my own.

framework of ideas, of fundamental principles, of axiomatic evidences that, in general, were considered as belonging exclusively to philosophy. (Koyré 1971, p. 256)²²

Therefore, Kuhn and Koyré reject the supposed independence of science, which would have occurred in the eighteenth century from the consolidation of the Scientific Revolution with the work of Newton.²³ This idea of the epistemological emancipation of science was for a long time a common historical place, suggested as a trace of elementary historical knowledge. But for Kuhn and Koyré, it is, in fact, a very clear trait of an enlightened, positivist conception of knowledge, understood as an overcoming of prejudices and superstition, and also of religion and philosophy.²⁴

To conclude, let's turn from science to its historiography. The OHS is the historiography of an extraordinary epistemological object—science—supposedly emancipated from metaphysics and producing its equally extraordinary results through the application of the scientific method. This contributes to the understanding of the isolation of the history of science from the (general) history that Kuhn observed in the 1970s, and about which he wrote a specific article, reproduced in *Essential Tension*. There, he says that historians are victims of

a widespread conviction that scientists discover truth by the quasi-mechanical (and perhaps not very interesting) application of scientific method. Having accounted for the seventeenth-century discovery of method, the historian may, and indeed does, leave the sciences to shift for themselves. (...) With their method in hand, the sciences

22. “L’histoire de la pensée scientifique nous enseigne donc (du moins essaierai-je de le soutenir): 1° Que la pensée scientifique n’a jamais été entièrement séparée de la pensée philosophique; 2° Que les grandes évolutions scientifiques ont toujours été déterminées par des bouleversements ou changements de conceptions philosophiques; 3° Que la pensée scientifique—je parle des sciences physiques—ne se développe pas *in vacuo*, mais se trouve toujours à l’intérieur d’un cadre d’idées, de principes fondamentaux, d’évidences axiomatiques qui, habituellement, ont été considérés comme appartenant en propre à la philosophie.” See also Koyré 1971, p. 264, and Koyré 1944, pp. 93–4.

23. For the relations between science and philosophy, see Pinto de Oliveira 2020 (forthcoming).

24. It is present even in an unorthodox logical positivist such as Neurath: “One science after another has cut itself loose from philosophy. (...) The last science to have the umbilical cord connecting it to philosophy severed is *psychology*. And what remains behind is a dead, deaf mass. (...) *The end of metaphysics is demonstrable precisely in the case of psychology*. Of course, from the fact that a regular science is now *possible* without any academic philosophy, without any metaphysics, it does not follow that all scientists make use of the possibility now open to them” (Neurath 1987, pp. 10–11).

cease to be historical, a perception for which there is no parallel in the historian's view of other disciplines. (Kuhn 1977, pp. 137, 155)

And also:

... just because Hermeticism was an avowedly mystical and irrational movement, recognition of its roles [in the history of science] should help to make science more palatable to historians repelled by what many have taken to be a quasi-mechanical enterprise, governed by pure reason and cold fact. (Kuhn 1977, p. 159)

Thus, the history of science, still represented by the OHS, is supra-historical or unhistorical for general historians (and also historians of other disciplines) because they are led to consider science an activity almost mechanical, almost entirely due to the mechanical application of the scientific method (Kuhn 1977, p. 137). And this, above all, is why these historians somehow segregate the history of science. After all, what is the interest in being the historian of an unhistorical activity? An activity in which the context is practically irrelevant and there is almost no circumstance? Historian of a single, vast, "normal science"? (see Kuhn 2000, p. 13).

A "new" historiography is opposed to the OHS, a historiography of an ordinary, "sublunary" object, a trivial historiography (in the expression of Radnitzky) as those of the other disciplines (art, philosophy), more attentive to vicissitudes and contingencies. But it must be said that the complex nature of the historiographical choice between the OHS and the NHS as yet another instance of inter-theoretical incommensurability does not allow the choice to be solved only on the basis of logical arguments. As in the case of science for Kuhn, it is necessary to resort to the community—in this case, the community of historians of science. Therefore, what I have sought to offer here is only an outline of the OHS's justification strategy ("the required stage setting" as Kuhn says) and the arguments with which Kuhn seems to have participated in the debate. The debate itself and the broader historical process of which it is part are subjects for a future paper.²⁵

25. On the stage setting, see Kuhn 2000, pp. 112, 141, 234. On the social character of science, see Wray 2011. In the first of the *Thalheimer Lectures* (1984), edited by Pablo Melogno in Spanish (Kuhn 2017), Kuhn seems particularly clear on this subject. See also Melogno 2019.

References

- Bloor, David. 1991. *Knowledge and Social Imagery*. Chicago: University of Chicago Press.
- Casini, Paolo. 1983. *Newton e la Coscienza Europea*. Bologna: Il Mulino.
- Cassirer, Ernst. [1932] 1951. *The Philosophy of the Enlightenment*. Translated by Fritz Koelln and James Pettegrove. Princeton: Princeton University Press.
- Cohen, I. Bernard. 1957. "George Sarton." The George Sarton Memorial Issue. *Isis* 48(3): 286–300.
- Comte, Auguste. [1830] 1989. A Positive Method in the History of Science. In *The History of Sciences: The French Debate*. Edited by Pietro Redondi. Translated by Arundhati Virmani from the French, *Cours de philosophie positive*. Paris: A. Costes, t.1, première leçon. Hyderabad, New York: Orient Longman.
- Condorcet. [1795] 2011. *Outlines of an Historical View of the Progress of the Human Mind*. Anonymous Translation from the French. Philadelphia: Carey and Rice. The Online Library of Liberty Fund. <https://oll.libertyfund.org/titles/condorcet-outlines-of-an-historical-view-of-the-progress-of-the-human-mind>
- D'Alembert, Jean le Ronde. [1759] 1986. *Essai sur les éléments de philosophie ou Sur les principes des connaissances humaines*. Paris: Fayard.
- Hall, A. Rupert. 1954. *The Scientific Revolution—1500–1800. The Formation of the Modern Scientific Attitude*. London: Longmans, Green and Co.
- Hoyningen-Huene, Paul. 2012. "Philosophical Elements in Thomas Kuhn's Historiography of Science." *Theoria* 75: 281–292.
- Jorland, Gérard. 1981. *La Science dans la Philosophie: Les Recherches Épistémologiques d'Alexandre Koyré*. Paris: Gallimard.
- Koyré, Alexandre. 1944. *Entretiens sur Descartes*. Paris: Brentano's.
- Koyré, Alexandre. 1948. "Condorcet". *Journal of the History of Ideas* 9(2): 131–152.
- Koyré, Alexandre. 1963. "Commentary by Alexandre Koyré". In: *Scientific Changes: Historical Studies In the Intellectual, Social and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present*. Edited by Alistair C. Crombie. New York: Basic Books.
- Koyré, Alexandre. 1965. *Newtonian Studies*. Edited by I. B. Cohen. Cambridge, Mass: Harvard University Press.
- Koyré, Alexandre. [1961] 1971. *Études d'Histoire de la Pensée Philosophique*. Paris: Gallimard.
- Koyré, Alexandre. [1966] 1973. *Études d'Histoire de la Pensée Scientifique*. Paris: Gallimard.
- Kragh, Helge. 1987. *An Introduction to the Historiography of Science*. Cambridge: Cambridge University Press.

- Kuhn, Thomas S. [1957] 1995. *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*. Cambridge, MA: Harvard University Press.
- Kuhn, Thomas S. 1970a. *The Structure of Scientific Revolutions*, 2nd Edn. Chicago: University of Chicago Press.
- Kuhn, Thomas S. 1970b. "Alexandre Koyré & the History of Science – On an Intellectual Revolution." *Encounter* 34: 67–69.
- Kuhn, Thomas S. 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.
- Kuhn, Thomas S. 2000. *The Road since Structure: Philosophical Essays, 1970–1993, with an Autobiographical Interview*. Edited by James Conant and John Haugeland. Chicago: University of Chicago Press.
- Kuhn, Thomas S. [1984] 2017. *Desarrollo científico y cambio de léxico. Conferencias Thalheimer*. Melogno, P. (ed.). Montevideo: ANII/UdelaR/SADAF.
- Larvor, Brendan. 2003. "Why did Kuhn's *Structure of Scientific Revolutions* Cause a Fuss?" *Studies in History and Philosophy of Science* 34: 369–390.
- Melogno, Pablo. 2019. "The Discovery-Justification Distinction and the New Historiography of Science: On Thomas Kuhn's Thalheimer Lectures." *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 9(1): 152–178.
- Neurath, Otto. [1932] 1987. Unified Science and Psychology. Pp. 1–23 in *Unified Science*. Edited by Brian McGuinness. Dordrecht: D. Reidel.
- Oberheim, Eric and Paul Hoyningen-Huene. 2018. "The Incommensurability of Scientific Theories." *Stanford Encyclopedia of Philosophy*. <https://plato.stanford.edu/entries/incommensurability>
- Pinto de Oliveira, J. C. 2012. "Kuhn and the Genesis of the 'New Historiography of Science'." *Studies in History and Philosophy of Science* 43: 115–121.
- Pinto de Oliveira, J. C. 2017. "Thomas Kuhn, the Image of Science and the Image of Art: The First Manuscript of *Structure*." *Perspectives on Science* 25(6): 746–765.
- Pinto de Oliveira, J. C. 2020. Kuhn and Logical Positivism: On the Image of Science and the Image of Philosophy. In *Interpreting Kuhn*. Edited by Brad Wray. Cambridge: Cambridge University Press, forthcoming.
- Pinto de Oliveira, J. C., and Amelia J. Oliveira. 2018. Kuhn, Sartre, and the History of Science. Pp. 277–293 in *Hypotheses and Perspectives within History and Philosophy of Science. Homage to Alexandre Koyré 1964–2014*. Edited by Raffaele Pisano et al. Cham: Springer.
- Radnitzky, Gerard. 1982. "Truth and History in Science." *Manuscrito* 5(2): 55–75.

- Redondi, Pietro, ed. 1989. *The History of Sciences - The French Debate*. London: Sangam Books.
- Sarton, George. 1948. *The Life of Science. Essays in the History of Civilization*. New York: Henry Schuman.
- Voltaire. [1744] 1967. "Courte réponse au long discours d'un docteur allemand." In *Oeuvres complètes de Voltaire: précédée de la vie de Voltaire*. Edited by Louis Moland. Nendeln: Kraus Repr., vol. 23.
- Voltaire. [1738] 1977. "Lettre a Maupertuis." In *Correspondance*, vol. 1. Edited by Theodore Besterman. Paris: Gallimard.
- Wray, K. Brad. 2011. *Kuhn's Evolutionary Social Epistemology*. New York: Cambridge University Press.