Kuhn and the historiographical revolution

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Kuhn devoted great attention to scientific revolutions, but did not investigate what he called the "historiographical revolution", a revolution in the Humanities, in the writing of the history of science, of which he was one of the main protagonists. In this article, after setting the stage in previous works, I attempt to outline how this revolution took place. I will argue that there was a revolutionary change in the historiography of science, but without a debate that corresponded to a revolution in the Kuhnian sense. This, as I will also try to show, does not mean that there has not been a debate of this kind. I argue that, due to theoretical and historical circumstances, the debate has shifted to the philosophy of science. The conflict, which began with a tension between the new historiography of science and the "old" philosophy of science (and led to the development of a new philosophy of science), had as one of its immediate and most important fronts precisely the relationship between history of science and philosophy of science.

1. Introduction

Kuhn said that his book *The Structure of Scientific Revolutions* (hereafter only *Structure*) depends on the new historiography of science (Kuhn 1977, p. xv). However, although he had devoted great attention to scientific revolutions, he did not investigate what he called the "historiographical revolution" (Kuhn 1970b, pp. 67 and 69), a revolution in the Humanities, in the writing of the history of science, of which he was one of the main protagonists.

By investigating how this revolution took place, I found no evidence of anything that could be considered a debate within the community of historians of science in the relevant historical period. But how could there not have been a confrontation of ideas if Kuhn was referring to a historiographical *revolution*? How to understand the transition from the traditional historiography to the new historiography of science without the resistance and controversy that Kuhn considers characteristic of a revolutionary change?[1]

Furthermore, one cannot neglect the fact that the debates expected in a Kuhnian revolution are broad and of a very complex nature, which led me, in a previous work, to speak of the incommensurability between the OHS and the NHS (see Pinto de Oliveira 2020).[2] As Kuhn describes, for example, in *Structure* for the case of science:

> To the extent, as significant as it is incomplete, that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm will be shown to satisfy more or less the criteria that it dictates for itself and to fall short of a few of those dictated by its opponent. There are other reasons, too, for the incompleteness of logical contact that consistently characterizes paradigm debates. For example, since no paradigm ever solves all the problems it defines and since no two paradigms leave all the same problems unsolved, paradigm debates always involve the question: Which problems is it more significant to have solved? Like the issue of competing standards, that question of values can be answered only in terms of criteria that lie outside of normal science altogether, and it is that recourse to external criteria that most obviously makes paradigm debates revolutionary (Kuhn 1970a, pp. 109-110).

This article aims to show that there was not the expected debate in the transition from the OHS to the NHS and, at the same time, how it happened. This is not a paradox. What I want to sustain, by outlining the contours of the historiographic revolution, is that things seem to have happened in a more complex way than one might initially suppose. In section 2, I point out why there has not been a broad debate among historians of science in the historiographical revolution.[3] In section 3, I argue that, due to theoretical and historical circumstances, the pertinent debate shifted to and was actually carried out within the philosophy of science. The conflict, which began with a tension between the NHS and the "old" philosophy of science, and spread with the development of a new philosophy of science, had as one of its immediate and most

[1] From now on, I refer to the "new historiography" of science (as Kuhn says) and to the "old" historiography of science respectively as NHS and OHS.
[2] It must be said that the many references here to my previous works are due to the fact that they are related to each other and to this article almost like chapters in a book. From now on, these references are abbreviated as PO.
[3] My focus here is the intellectual revolution, the first stage of the historiographical revolution, according to Kuhn (see Kuhn 1970b, p. 67).

important fronts precisely the relation between the history of science and the philosophy of science.[4] The final section seeks to reflect on this peculiarity.

2. The community of historians of science and the absence of a debate

As I have indicated in previous articles, although *Structure* is all permeated by the issue of historiographic change, no OHS adherents are mentioned in the book. But, in *The Essential Tension*, Kuhn makes a direct reference as 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). Thus, Kuhn signals the 18th century with Condorcet as the moment of emergence of the OHS and Sarton as the other end of the spectrum.

It is George Sarton, the contemporary end of Kuhn's spectrum, who interests us here. It is he who can be taken as a reference for the OHS at the moment when the change that Kuhn calls the "historiograhical revolution" takes place. In Pinto de Oliveira & Oliveira 2018, we demonstrate how Sarton fulfills the role of "old" historian that Kuhn assigns to him. There we quote an expressive phrase by Dorothy Stimson, which opens her preface to *Sarton on the History of Science,* published in 1962: "For forty years the name of George Sarton has been practically synonymous with the history of science" (Stimson 1962, p. v).

It was also in 1962 that Kuhn, with *Structure*, announced the emergence of a new history of science in response to an older history of science which, as an autonomous academic discipline, existed thanks mainly to Sarton's tireless activity. It is true that Stimson's observation remains valid as recognition of this work. But it is also true that, after sixty years, the statement became very questionable, in relation to the recognition of Sarton's actual historical and historiographical work. In general, NHS supporters negatively assess this work and so it can be said that, in this sense, Sarton has become practically an antonym for the history of science. Kuhn refers to this situation when he writes:

[4] Symmetrically to the abbreviations in relation to the historiography of science (see note 1), I will use the expressions NPS and OPS from now on to refer, respectively, to the new and the old philosophy of science.

> Though I know it will give offense to some people whose feelings I value, I see no alternative to underscoring the point. Historians of science owe the late George Sarton an immense debt for his role in establishing their profession, but the image of their specialty which he propagated continues to do much damage even though it has long since been rejected. (Kuhn 1977, p. 148)[5]

In the 2018 article on Kuhn and Sarton (p. 290), we quote the following passage, in which Sarton displays a "photographic negative" of Kuhn's normal science. Sarton sees negatively the same scene that Kuhn sees positively, characterizing something that could be called Sarton's "abnormal science":

> The shackles of the medieval anatomists were less religious than scholastic. Medical men had not acquired the habit of seeing with their eyes open without prejudices. Indeed, they were so much dominated by older masters such as Galen and Avicenna that they were not only blind to reality but able to see things which were not there at all; Galen's words were more convincing to them than reality itself! It is a bit difficult for us to imagine such a state of mind, though it has not yet completely disappeared. The renovation of anatomy was finally accomplished by men who were good observers, had dexterous hands and sharp eyes, and were not inhibited by prejudices. (Sarton 1962, p. 134) [6]

These characterizations of the OHS through Sarton's work fit neatly into Kuhn's summary of the OHS in *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)

[5] In other passages, Kuhn highlights the discrepancy between his point of view and that of Sarton and a few other contemporaries. According to him, what they did was not "quite history; it was textbook history" (See Kuhn 2000, p. 282. See also Kuhn 1970a, pp. 1 and 137–138).

[6] In Thackray & Merton 1972 the authors warn that Sarton is a 19th century man. But it is necessary to remember that, despite his naive way of "evangelizing" in favor of the history of science, he is linked to the logical positivists in the 20th century, who even asked him to write the monograph on the history of science in the positivist *Encyclopedia*. On Sarton, logical positivism and the traditional image of science, see PO 2015; 2017 (section 3) and 2021.

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)

And that is, according to Kuhn, what Koyré exemplarily does (Kuhn 1970b, p. 68, and 1970a, p. 3. See also Cohen 1966, p. 161). This polarization pointed out by Kuhn is present in other authors. There is a criticism to Sarton and what he represents at the same time that an expressive adherence to Koyré's work is revealed. In a brief history of the historiography of science, for instance, Henry Guerlac cites Sarton in opposition to the "enlarged and deepened conception of the history of science", represented by Koyré (Guerlac 1963, pp. 808-809). And Bernard Cohen writes, comparing Sarton and Koyré:

> Sarton often professed an interest in philosophy, but in fact had little patience with philosophical history of science or the history of scientific ideas. He wanted a record of "positive" achievements (...) a record of people and events. By contrast, (...) Koyré showed us that the history of science need not be a linear chronology of progress but could be an exciting and stimulating subject of truly intellectual dimensions. (...) Koyré taught us the primary necessity of studying texts: line by line, thought by thought. He showed us that only by examining such writings, both those published by an author and those in manuscript, could we understand what these thinkers of the past intended. And in the explanation of those texts, one had constantly to be aware of the general background of science, philosophy, religion: the matrix of ideas without which the science of any great or lesser creative thinker could never be understood. He was, of course, a great master of this art, a true "magicien ès lettres," who set us an example of the life of scholarship that we might try to follow insofar as we were able. (Cohen 1987, pp. 55-57. See also Cohen 1966)

Cohen speaks in this passage about Koyré's influence, not only on himself, Cohen, but on a whole new generation of historians of science. He emphasizes this aspect when he writes: "I shall not attempt to make a catalogue of all scholars in America who either knew or were influenced by Koyré. Their number would include the major figures in history of science" (Cohen 1987, p. 62). He highlights the names of Marshall Clagett, Charles Gilliespie, Erwin Hiebert, Henry Guerlac, Marie Boas, Rupert Hall, Gerald Holton, Thomas Kuhn, Richard Westfall. His text is retrospective, but he refers to texts closer to the experience of these authors in their contact with Koyré (see Cohen 1987, pp. 59-64).[7]

Criticism to Koyré came from authors such as Aldo Mieli (see Chimisso 2008, pp. 131-132), Geymonat (see Casini 1987, p. 96) and Leonardo Olschki, who, according to Cohen 1987 (p. 58), would have anticipated Stilmann Drake's point of view. These objections, it should be noted, were not addressed to Koyré's method, which was his main influencing factor in the historiographic revolution (Redondi 1987b, p. 4), but to a radical anti-empiricist conception, about which Kuhn too has reservations (see PO 2012, p. 118).[8]

Of course, Koyré's influence on the history of science did not take place in a void. Even in his summary text, Cohen comments that "in America, furthermore, there has long been a movement spearheaded by Arthur O. Lovejoy within classical departments of history to encourage and develop the history of ideas as intellectual history" (Cohen 1987, p. 64). Kuhn also refers to Lovejoy and says that Koyré showed that one could do in the history of science what Lovejoy did in the history of ideas (Kuhn 2000, p. 285. See also Kuhn 1977, preface, p. vi). And it should be remembered that Koyré published some of his first influential articles in English in the *Journal of the History of Ideas*, created in 1940 by Lovejoy (see Cohen and Taton 1964, p. xiii).[9]

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[7] See also Thackray 1970, p. 116. Kuhn's relationship with Koyré is present in several of my works, especially in PO 2012 and 2020.

[8] Pierre Duhem's ideas could be suggested as the opposite pole in a possible debate with Koyré about continuity or discontinuity in the history of science. Nevertheless, the historians of science involved in the shift from the OHS to the NHS take Sarton as a point of reference. Kuhn, for example, does not even refer to Duhem in the context of the OHS. In fact, he considers Duhem's work on middle age as one of the factors contributing to the change characterized by the NHS (see PO 2012, section 2.2., and also Oliveira 2017, pp. 127-139).

[9] The first of these articles was "Galileo and Plato" (1943), whose English text was revised by Cohen (1987, p. 58). The others were "Louis de Bonald" (1946) and "Condorcet" (1948). Kuhn says in *Encounter*, in his review of *Metaphysics and Measurement* (1968), that the first of these works and "Galileo and the Scientific Revolution in the Seventeenth Century" (also published in 1943) "are brilliant distillations" of main themes from the *Études Galiléennes* and "they inaugurated the movement that has

Looking at the scene from a more institutional perspective, Thackray writes in 1995:

> Forty years ago the history of science was a new and fledgling field, enjoying its first glimpses of recognition in the American academy. Ironically, the death in 1956 of George Sarton -- who had by then spent forty years as lone American prophet of his chosen discipline -- coincided with the passing of the discipline itself into that promised land of which Sarton only dreamed. Sarton died in March 1956. In May of that year quite separate plans were officially launched for holding an ambitious "History of Science Institute" at the University of Wisconsin. The word "institute" had a Sartonian ring, but the plans in question owed little to Sarton. (Thackray 1995, p. vii)

A ten-day symposium, linked to the Institute's project, was carried out in 1957. Thackray says that the volume published in 1959, bringing together the works of the symposium (Clagett 1959), "provided a 'best practice' casebook of what the new, post-Sartonian, professional history of science aspired to be".[10] He also states that "the roughly two dozen individuals who acted as speakers and commentators at the Madison meeting came close to constituting the totality of professional historians of science in the English-speaking world of their Day" (Thackray 1995, p. vii).

In "Professionalization Recollected in Tranquility", Kuhn writes that in the early 1950s, when the history of science was not quite a profession, "there were only half a dozen people employed to teach it (...). Other people offered courses, too, but most of them were practicing scientists who occasionally lectured on the development of their own field". And he gives account of his experience in the history of science meetings:

> During those same years, attendance at History of Science Society meetings can never have reached fifty (half that number is probably far closer), simultaneous parallel sessions were unknown, and the audience at papers could have been accommodated comfortably in the sitting room of a mid-sized house. (Kuhn 1984, p 29)

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continued ever since (The *Études* had been published four years earlier but, because of the war, were little known)" (Kuhn 1970b, p 68).

[10] In Nickles 1995, the author draws attention to the distorting work of Father Clark. Nickles writes: "According to Clark, the good historian should begin from twentieth-century positivist conclusions about what constitutes a scientific theory, law, explanation, confirmation, and so on. We now know, he said, that the correct method of science is the hypothetico-deductive method. Moreover, the up-to-date historian should not hesitate to impose these results on the historical interpretation of all periods, back to the Presocratics" (p. 139).

Although imprecise, Thackray's and Kuhn's observations make it clear that the number of people who made up the community of historians of science was very small in the 1950s, when the history of science was beginning to become a profession in the USA (see also Harvey 1999). And virtually all of the authors who presented papers at the 1957 symposium at the University of Wisconsin are on the list of Koyré-influenced historians highlighted by Cohen. The names of Marshall Clagett, Charles Gilliespie, Erwin Hiebert, Henry Guerlac, Marie Boas, Rupert Hall, Thomas Kuhn, and Bernard Cohen himself appear on both lists.

And it can be said that all of them (as well as some that are in only one of the lists, such as Pearce Williams, Carl Boyer and Conway Zirkle) were also on the board of the History of Science Society (HSS) and/or the Editorial Committee of *Isis*, at the time that Sarton was still the editor of the journal and the grey eminence of the HSS, both founded by him at the beginning of the 20th century (1913 and 1924, respectively).[11] This can be seen, for example, at the HSS Council Meeting that instituted the Sarton Medal, which became the most prestigious award, the "Nobel prize" in the field of history of science. The meeting was held on December 27, 1952 in Washington and was attended by Dorothy Stimson (President-Elect, who presided), Marie Boas, Carl B. Boyer, Marshall Clagett, Father Joseph Clark, I. Bernard Cohen (Managing Editor of *Isis*), I. E. Drabkin, J. F. Fulton, Henry Guerlac (Vice-President), Thomas Kuhn, George Sarton (Editor of *Isis*), R. C. Stauffer, Owsei Temkin, W. J. Wilson, and F. G. Kilgour (Secretary-Treasurer) (Kilgour 1953, p. 205).[12]

Thus, it can be said that the young historians of science installed themselves in the incipient profession, institutionally following in Sarton's steps and, theoretically, the orientation of Koyré's project. And there was not a debate within the community of historians of science. Or at least a debate worthy of a historiographical revolution. The absence of a debate and the idea of a smooth transition between the OHS and the NHS are well characterized if we take into account that, during the period when Sarton was

[11]Many of the names associated here with the historiographic revolution (mentioned by Cohen and/or active in the HSS or *Isis*) are present in *Structure*'s bibliography. This is the case of Bernard Cohen, Marshall Clagett, Marie Boas, Henry Guerlac, Charles Gillispie and Rupert Hall. Alistair Crombie, Israel Drabkin, Dorothy Stimson, and Gerald Holton are not named there, but appear in *Copernican Revolution*.

[12] Understandably, given the early stage of the profession, nearly everyone who attended the meeting that created the award received the Sarton Medal, starting with Sarton himself, who was the first to receive it in 1955. Fulton received it in 1958 and followed Temkin (1960), Guerlac (1973), Cohen (1974), Dibner (1976), Clagett (1980), Ruppert Hall and Marie Boas Hall (1981), Gillispie (1984). Koyré received the medal in 1961 and Kuhn in 1982.

editor of *Isis*, Koyré collaborated a few times with the Journal in the section "Notes and Correspondence", published two book reviews and two important articles in his bibliography. And Kuhn also published two of his first articles on the history of science in the HSS journal while it was still edited by Sarton.[13]

What could have happened that there was no debate between NHS and OHS supporters? Would it be the case to revise the idea of a revolution in the historiography of science, contrary to Kuhn's conception? Let's look at the issue from both sides, the OHS and the NHS.

Although Sarton surprisingly does not mention Koyré in his guide to the history of science (Sarton 1952), he did not represent effective opposition to him, unlike Aldo Mieli, for example (see Cohen 1987, p. 68, note 1, and Chimisso 2008, pp. 131–132). Suffice it to recall that in September 1940 Sarton wrote a letter of reference for Koyré, addressed to the director of the New School for Social Research in New York, which he concluded in the following terms:

> I am better informed concerning Prof. Alexandre Koyré, whom I take to be one of the most distinguished historians of science and historians of thought of our time. ln spite of his relative youth he has published many valuable studies on Descartes, Galileo, Copernicus, Jacob Boehme. I do not know him personally, but admire his activity as far as it is represented by his printed writings. (Koyré 1986, p. 63) [14]

Deeply engaged in his project of promoting the history of science as a discipline and profession, of increasing the number of people interested in 'his' subject, Sarton was certainly not very sensitive to methodological disputes, which would be secondary to this project and could mean an internal division. Furthermore, Koyré represented a prestigious acquisition for the nascent area of the history of science. Before becoming known as a historian of science, he was already an important author in the traditional area of the history of philosophy. As Cohen points out:

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[13] The articles are: "Traduttore-Traditore. A propos de Copernic et de Galilée" (1943) and "An Unpublished Letter of Robert Hooke to Isaac Newton" (1952), published by Koyré (see Cohen and Taton 1964, p. xiii); and "Newton's '31st Query' and the Degradation of Gold" and "Robert Boyle and Structural Chemistry in the Seventeenth Century", respectively published by Kuhn in 1951 and 1952 (see Kuhn 2000, p. 325).

[14] In turn, Koyré's references to Sarton are rare and when they occur are positive (and trivial). In *Études d'Histoire de la Pensée Scientifique*, for example, which is a collection of writings from different periods, Koyré only makes two circunstancial references to Sarton (Koyré 1973, pp. 61 and 108).

> By this time Koyre's name was well known for a variety of books and articles in French and in German, beginning with his first publication in 1912, " Sur les nombres de M. B. Russell," and including studies on Greek philosophy, the development of philosophical concepts [...] and works on Russian philosophy [...]. There was also a brilliant series of articles on Hegel [...] and a pioneering set of analyses of German mystics [...] Looking back from today's vantage point, it is easy to see the direction his work was taking: from St. Anselm and medieval philosophy to Paracelsus and the sixteenth-century mystics, and then on to Copernicus, Descartes, Galileo, and eventually Newton. (Cohen 1966, p. 158)

And we can take here Bernard Cohen himself as a representative of the NHS. Perhaps it could be said that he is an icon of the change in the historiography of science that we are considering. Cohen's case is significant because he was Sarton's student and *protégé*, having had Sarton's guidance in his doctoral work at Harvard and followed in his institutional footsteps, as in the case of *Isis*. After several decades as editor of the HSS journal, Sarton voluntarily left the post, which was occupied by Cohen (who was already its Managing Editor), certainly with Sarton's approval, without which this would have been virtually impossible at the time.[15]

On the other hand, Cohen, who was involved in work with Koyré as soon as he met him in person in the early 1940s, read the *Études Galiléennes* even before they were published in a single volume in 1939 (See Cohen 1987, p 55). And it was Cohen who later recommended the reading of the book to Kuhn, revealing to him Koyré's work that would, ultimately, largely influence his own work (See Kuhn 2000, p. 285). Moreover, it should be remembered that, in refusing Neurath's invitation to write the monograph on the history of science in the positivist *Encyclopedia*, Sarton named Cohen, who in turn named Kuhn, who eventually wrote the *Structure* (see PO 2015, section 2).

[15] In the June 1953 issue of *Isis*, Dorothy Stimson writes: "This is the first issue of Isis, since the establishment of the journal in 1912, be published under an editorship other than that of its distinguished founder, George Sarton. (...) Fully conscious of the difficulty of finding any single person with the range of knowledge, the tireless energy and manifold endowments of George Sarton to take his place at the head of Isis, the Council of the History of Science Society has decided to appoint, not a single Editor, but an Editorial Board. This Board consists of Marshall Clagett (University of Wisconsin), I. Bernard Cohen (Harvard University), I. E. Drabkin (City College, New York), John F. Fulton (Yale University, School of Medicine), Henry Guerlac (Cornell University), and Conway Zirkle (University of Pennsylvania). The Chairman of the Editorial Board and Editor of Isis is Professor I. Bernard Cohen who has been Managing Editor of Isis since 1947" (Stimson 1953, p. 3. See also Multhauf 1975, p. 461). On Cohen see Cohen 1984 and 1987, Harvey 1999, and Dauben 2009.

I believe that an issue I addressed in another article suggests a way to understand this absence of debate, an anomaly in light of what one would expect from a typical Kuhnian revolution. At the end of that text, it can be read that

> the history of science, still represented by the OHS, is suprahistorical 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. See also pp. 155 and 159). 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 circumstances? (PO 2020, p. 394. See all the section 4).

I think there are good reasons to suppose that the very birth of the history of science as a profession is linked to a reaction against such a conception. Historians as historians move away from science understood as an extraordinary epistemological object, supposedly emancipated from metaphysics and producing its equally extraordinary results through the application of the scientific method. And they are attracted by an object that presents vicissitudes, circumstances and contingencies. As Kuhn writes:

> ... 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)

And another passage allows us to think that Kuhn’s own interest in the history of science was practically born together with his reading of Koyré. Referring to his first encounter with James Conant in 1947, he writes that the summer was spent reading Aristotle, Galileo, and Koyré “and the results were for me transforming. By the early fall I was seriously considering transferring from science to its history. By spring the decision was made" (Kuhn 1984, p 30). Just ahead he says he was drawn into the history of science "by a totally unanticipated fascination with the reconstruction of old scientific ideas and of the processes by which they were transformed to more recent ones" (Kuhn 1984, p 31), which can be understood as a summary description of what Koyré does in *Études Galiléennes*.[16]

It could be said that the new historians of science -- those who will constitute the history of science as a profession -- are part of the historians' movement towards science (see Kuhn 1977, p. 131, on Butterfield 1966). It is the historians who emerge as historians precisely because they are interested in science and propose to specialize in the history of science, with science coming to be seen as a human activity similar to others. An activity with its peculiarity, but that is not exempt, like the others, from historical vicissitudes and circumstances. And that is what Koyré's work provides, it is the appeal of Koyré's work. It is this appeal that leads to a natural reception of Koyré and the (equally natural) abandonment of Sarton's perspective. A new historiography is opposed to the OHS. A historiography of an ordinary, "sublunary" object, a "trivial" historiography as those of the other disciplines (art, philosophy), more attentive to relevant vicissitudes and contingencies (see PO 2020, p. 394).

This would be consistent with Kuhn's references to the OHS as unhistorical (see PO 2020, pp. 375- 376). Moreover, with the NHS, the history of science becomes also institutionally linked to history departments rather than science-related departments. According to Guerlac, as Cohen writes, Koyré was responsible for the "transmutation of the history of science into a subject that was intellectually interesting and respectable, as well as one that was related to other aspects of the history of thought, that had made it acceptable as a subject taught in programs of history departments" (Cohen 1987, p 61).

To conclude this section, it is worth to say that the NHS is not *stricto senso* a new historiography. In fact, it is the OHS that presents itself as new among the historical disciplines, defined, according to Condorcet and others, 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. The NHS is the same historiography used by general historians and historians of other disciplines (philosophy, art), which in the 20th century began to be applied to science as well. It is a new historiography in the context of science (see Kuhn 1977, p.

[16] Kuhn 1984 and 1986 are two short articles that directly concern the development of the history of science as a profession. See also George Reisch's works on post-war reflections about science, such as Reisch 2019.

xiii and xv, and also PO 2020, p. 391). The NHS is incommensurable with the OHS insofar as it presents, as we have seen, a new conception of historiography (of science) and also, as we shall see later, a new conception of science.

3. The debate: the relation between history of science and philosophy of science

Alongside this smooth transition within the community of historians of science -- in the sense that the historiographic revolution was not accompanied by a significant internal debate -- there was a few years later a strong controversy involving the history of science: the debate on the relation between the history of science and the philosophy of science. It can be said that the main milestone of the debate was the publication of Kuhn's *Structure*. As he says in the preface, the book sought to make explicit the image of science revealed by a "new historiography" of science. The confrontation, which had already been happening quietly with the creation of an area of friction or tension between the NHS and the traditional philosophy of science, now becomes stronger and broader.

In the 1950s (and even before) the "History and Philosophy of Science" was already talked about as a single discipline (see, for example, Crombie 1963, pp. 757-761 and Clagett 1959, p. iii). At Cambridge, Crombie says, "there is (...) no attempt to separate history from philosophy, and each is used to illuminate the other" (Crombie 1963, p. 761). The relation between the two areas or disciplines was trivial, articulated, and tension-free. Both worked with the same image of science. Kuhn writes:

> The older history of science, addressed primarily to scientists, saw itself as philosophy teaching by example. It displayed the irresistible march of humanity towards objective truth, the inevitable triumph of reason and method over ignorance and superstition. In those years one knew how science worked and what scientific progress was" (Kuhn 1986, p. 33).[17]

[17] In Thackray 1980 (p. 469) the author writes: "To become part of academic history, the subject [history of science] would necessarily have to renounce its pretence that 'the history of science is the only history which can illustrate the progress of mankind'. Although this claim was agreeable to scientific statesmen and was pleasing to the proto-historians of science in their many struggles, academic historians did not like it. Forced to choose between science and history as proper patron within the departmentally organised university, the history of science came to depend on the more powerful, more visible and more numerous body of natural scientists, but this dependence cut it off from departments of history and from graduate students trained in historical methods".

The question regarding the relation between history of science and philosophy of science begins with the NHS, around Koyré, when it gradually reveals an image of science that clashes with OPS. Kuhn refers to this in his famous first sentence of *Structure*. It concerns to the method of the history of science and, at the same time, to the method of the philosophy of science: proposes a new historiography of science and a role for the new historiography of science in the philosophy of science. It is in this context that a broad discussion of the relation between history of science and philosophy of science begins, soon after the publication of *Structure*.[18] In this debate, in which the traditional view seeks to disqualify the role of the history of science in the philosophy of science (although before they were innocently associated) or to consider the NPS as irrationalist, the most characteristic aspect of the opposite side seems to be precisely in the acceptance of the NHS. In this perspective, I believe that Kuhn's project in *Structure* can be summarized by quoting a passage from his article "The Histories of Science: Diverse Worlds for Diverse Audiences". There, referring to the idea that the scientific objects are constructions, Kuhn writes:

> As far as it goes, that way of speaking seems to me just right, but it does not go very far. What it leaves open are such questions as: What are the materials out of which these constructions are made? What are the principles of sound construction? What is the relation between older constructions and their newer replacements, the relation that makes the latter seem so much more powerful than the constructions they replace? *It is not, of course, the responsibility of historians to answer such questions. That is more nearly the philosopher's job. But history -- not every historian, but the historical profession -- has a responsibility for helping with the problem, partly because historians played a primary role in the destruction of the traditional viewpoint, and partly because their works are going to be read as supporting one or another answer whether or not the authors of those works intend that they should* (Kuhn 1986, p. 33, my emphasis. See also Kuhn 2000, p. 129).

Although the debate is dispersed over a relatively long period and not yet delimited, I will concentrate my investigation here on the point where it seems to have

[18] In *Structure* Kuhn speaks of a "historiographic revolution in the study of science" (Kuhn 1970a, p 3). And Nickles writes: "Since the original Critical Problems Congress on the history of science, organized in 1957 by Marshall Clagett, there has been a near reversal in the relations of history of science and philosophy of science. Then methodologists confidently advanced normative theses that were supposed to be virtually immune to criticism by both scientists and historians. Historical work, too, merely furnished illustrative material for rational reconstruction by philosophers. Only five years after the 1957 congress, and in the same year that the congress volume appeared, Thomas Kuhn famously asserted the priority of history to logic" (Nickles 1995, p. 139).

started, or at least on the point that can be considered the most important in a first characterization of the debate. I refer to the so-called Bedford College Colloquium, held in London in 1965, and more specifically to a symposium (on the 13th of July) that brought together Popper and some Popperian authors to discuss Kuhn's book. Kuhn was invited to open the session by presenting a paper on his conception of science in comparison with Popper's conception. The works presented were later gathered in the well-known volume edited by Lakatos and Musgrave, which preserved the symposium's title -- *Criticism and the Growth of Knowledge*. In fact, as the editors write in the preface, what one reads in the book is "a rational reconstruction and expansion rather than a faithful report of the actual discussion". And they clarify:

> The texts of the papers as here printed were finished at different times. Professor Kuhn's paper is printed essentially in the form in which it was first read. The papers by Professors John Watkins, Stephen Toulmin, Pearce Williams and Sir Karl Popper are slightly amended versions of their original contributions. On the other hand, Miss Masterman's paper was finished only in 1966; while Dr Lakatos's and Professor Feyerabend's papers, together with Professor Kuhn's final reply, were finished in 1969.

It can be said, therefore, that the book not only characterizes the beginning of the debate that interests me here, but also marks "the growth" of the debate. This is clear in the note on the second edition (1972), in which the editors are keen to add that the ideas discussed in the book were later developed by some authors and cite the second edition of *Structure*, Toulmin's *The Human Understanding*, Feyerabend's *Against Method*, Popper's *Objective Knowledge* and Lakatos's *History of Science and Its Rational Reconstructions*. It is also worth mentioning that the Spanish edition of the volume (1975) includes this work by Lakatos and Kuhn's article "Notes on Lakatos", at Lakatos's suggestion (p. 7).

Turning to the content of the debate that interests us here, the relation between history of science and philosophy of science as disciplines, it should immediately be considered that the objection to assigning a fundamental role to history of science in philosophy of science is not new. Already in *The Logic of Scientific Discovery* (published in German in 1934), Popper took a position frankly contrary to what he called the "naturalistic approach". For him, the philosophy of science is not a logic and should not be considered an empirical science either:

> I do not believe that it is possible to decide, by using the methods of an empirical science, such controversial questions as whether science actually uses a principle of induction or not. And my doubts increase when I remember that what is to be called a 'science' and who is to be called a 'scientist' must always remain a matter of convention or decision. (Popper 1968, p. 52)

This critique still does not directly mention the history of science. Popper updates it precisely in "Normal Science and its Dangers", text of his communication at Bedford College, pointing his guns at Kuhn and the history of science:

> The suggestion that we can find anything here like 'objective, pure description' is clearly mistaken. Besides, how can the regress to these often spurious sciences help us in this particular difficulty? Is it not sociological (or psychological, or historical) *science* to which you want to appeal in order to decide what amounts to the question 'What is *science?'* or 'What is, in fact, normal in science?' For clearly you do not want to appeal to the sociological (or psychological or historical) lunatic fringe? And whom do you want to consult: the 'normal' sociologist (or psychologist, or historian) or the 'extraordinary' one? (Popper 1970, p. 58)

Imre Lakatos is going to develop these Popperian arguments, but not without charging for it: not only does he turns against Kuhn, but also against Popper. In fact, Lakatos intends to find a place between Popper and Kuhn, with one foot in Popper's theory and the other in Kuhn's. His main work on the history of science opens with the famous paraphrase to Kant: "Philosophy of science without history of science is empty; history of science without philosophy of science is blind" (Lakatos 1971, p. 91). The first sentence is evidently directed against those who show at least some distance from the history of science, such as Popper and the logical empiricists. The second is, in particular, a criticism to Kuhn.

In addition to the reason we have already pointed out with Popper -- the need for a concept of science -- the historian would be committed to a philosophy of science, according to Lakatos, in order to: (1) proceed with the selection of what we would call categories of facts or historical events and (2) establish the relations between facts required in an explanation. The two aspects are closely linked. As for (1), Lakatos says that the inductivist historian, for example, looks only for "hard factual propositions" and "inductive generalizations". A Popperian historian, on the other hand, "looks for great, 'bold', falsifiable theories and for great negative crucial experiment" (Lakatos 1971, pp. 93 and 97). This search and seizure, in history, of conceptual categories proper to a particular philosophy of science is justified precisely in view of (2). It is the identification of such or what events as authoritative conceptual categories that would allow the historian to make use of the logical relations defined in a philosophy of science and thus provide acceptable explanations.

Strictly speaking, the subsumption would line off those behaviors exempt from explanation: Once defined what the scientist's behavior is, one is at liberty to say that a particular scientist acts in accordance with that behavior precisely because he is a scientist.[19] With the exemplary behavior thus established, it would be up to the historian to explain the faults or deviations, through the intervention of external factors, resorting, according to Lakatos, to empirical theories. That part of the historiographical work that brings together the edifying behaviors Lakatos calls internal history. The other, the pages of "misbehaviors" from the point of view of scientific rationality, external history. Or, as Lakatos in a joke says, the first is the story that must be told in the body of the text and the second, only in the footnotes (see Lakatos 1971, p. 107).

Lakatos accuses strictly aprioristic or purely normative philosophies of science of emptiness. For such theories, what comes from the history of science does not reach them. He asks:

> Is it not then hubris to try to impose some a priori philosophy of science on the most advanced sciences? Is it not hubris to demand that if, say, Newtonian or Einsteinean science turns out to have violated Bacon's, Carnap's or Popper's apriori rules of the game, the business of science should be started anew? (Lakatos 1971, p. 121).

Popper admits in the *Logic*, at least rhetorically, that "'the whole of science' might err" (Popper 1968, p.29). But Lakatos's answer to the question is, like Kuhn's, affirmative. Lakatos will look for a way to make the history of science -- which, according to him, is methodologically dependent on the philosophy of science -- be able to, at the same time, play a role in the evaluation of philosophies of science.

His proposal can be outlined as follows: If every particular philosophy of science determines a particular program of historical investigation or, what is the same, a rational model for the reconstruction of the history of science, then the best philosophy of science will be the one that leads to the best historical reconstruction. In

[19] The model of explanation suggested here would essentially follow the Weberian ideal type. In Lakatos, the expression 'ideal' would have, however, an evaluative connotation that it does not have in Weber.

turn, the greater the portion of history told as internal history the better a historical reconstruction will be. In other words, the best reconstruction will be the one that manages to reconstruct the history of science in a more complete way within a mold of scientific rationality previously provided by a philosophy of science.

Thus, Lakatos judges his own philosophy of science superior, for example, to Popperian philosophy because, among other things, it was irrational in Popper's understanding, says Lakatos,

> to retain and further elaborate Newton's gravitational theory after the discovery of Mercury's anomalous perihelion; or again, it was irrational to develop Bohr's old quantum theory based on inconsistent foundations. From my point of view these were perfectly rational developments: some rearguard actions in the defence of defeated programmes - even after the so-called 'crucial experiments' - are perfectly rational (Lakatos 1971, p. 117).

What about Kuhn's theory? Lakatos's answer, one assumes, is that Kuhn's philosophy of science could, of course, be put to the test. But it would not pass it, for a very simple reason. While it may even be conceded that Kuhn's theory could guide the most accurate historical reconstruction, the problem is, Lakatos would say, that such a reconstruction *would not be rational* (see Lakatos 1971, p.116).[20] That is why he does not even bother to put Kuhn's theory of science to his historical test.

On the other hand, Kuhn would be affected by the blindness of the history of science that does not have a philosophy of science as a guide. To Lakatos (as to Popper), every historian of science has necessarily -- explicitly or implicitly, he stresses -- a commitment to a "normative philosophy of science", without which his work could not even be carried out (Lakatos 1971, p. 107). Thus the general question addressed to Kuhn is: What, in addition to naivety and confusion, could there be in a non-normative and non-rational philosophy of science, inadvertently based on a positivist historical work?

This kind of critique, as it seeks to undermine the very perspective taken by Kuhn's philosophy and thus discard it *in limine*, even before giving it the floor, is obviously quite uncomfortable for Kuhn. He seeks to directly respond to it on at least

[20] Lakatos admits that Kuhn's historical reconstruction would be "probably the most colourful" (Lakatos 1977, p. 192).

three occasions: in "The Relations between the History and the Philosophy of Science" (l968), "Notes on Lakatos" (1970) and "The Halt and the Blind" (1980).[21]

In the earliest article, revised in 1976 and first published in *The Essential Tension*, Kuhn initially focuses on his practice as a historian and philosopher of science, drawing attention to the fact that, despite his own dual expertise, these practices are very different and cannot be simultaneous. The rest of the article is unevenly focused on evaluating the relationships between the two disciplines, as these relationships "are far from symmetrical". He devotes most of it, then, to justifying the importance of the history of science for the philosophy of science, limiting himself, in the opposite direction, to stating that he very much doubts that a deeper knowledge of philosophy of science can be useful for the historian of science, in particular that "currently practiced in the English-speaking world" (Kuhn 1977, pp. 11 and 12).[22]

By turning in the article to the methodology or epistemology of the history of science, Kuhn's concern is to show that history is "a possible source for a rational reconstruction of science", as far as it is conceived in a different way than philosophers like Popper and Hempel. According to Kuhn, whatever the Hempelian model of explanation for the natural sciences is, its transfer to history is mistaken. Kuhn does not deny that every historian makes use of natural or sociological laws (and, above all, of "at once obvious and dubious" 'laws' of common sense). What he sustains is that laws are not essential to the explanatory power of the historical narrative. This is due, according to him, above all to the "facts the historian presents and the manner in which he juxtaposes them" (Kuhn 1977, pp. 14-16).

The historian's job is, for him, like putting together a jigsaw puzzle: even though the historian may have an indefinite number of pieces, his task is to select and fit the pieces together until a "plausible narrative involving recognizable motives and behaviors" is formed. There are rules that govern the execution of the work. For example, the narrative must not violate natural and social laws, nor be inconsistent with

[21] I also consider here the articles "Reflections on My Critics" (written in 1969 and included in Lakatos & Musgrave 1970 and later in Kuhn 2000), "The History of Science" from 1968 (published in the Encyclopedia of Social Sciences and later in Kuhn 1977), the review of *Encouter* journal (Kuhn 1970b), as well as the 1971 article on the relation between history and history of science, also published in Kuhn 1977.

[22] In fact, the philosophy of science practiced in other languages, such as that of the neo-Kantians, does not enjoy a better reputation, as Kuhn says that he recommends its study only for its historical content and not for its philosophies (Kuhn 1977, p. 11).

facts omitted by it. These rules, according to Kuhn, do not determine the solution, but limit the number of possible solutions (see Kuhn 1977, p. 17).

Evidently, Kuhn recognizes the vagueness of this explanation of the nature of historical work. He admits that his modest attempt is only to identify and not yet defend "convictions" and is no more than the first step towards a proper philosophical investigation (see Kuhn 1977, p. 18). However, no further steps are taken by him in this direction. Kuhn was actually trying to justify the idea that science, until proven otherwise, would be an activity like any other and subject to the same historicity, the same historical circumstances and vicissitudes. And this is true, according to him, even though science is "the best example we have of rationality". As he writes in "Notes on Lakatos":

> Scientific behavior, taken as a whole, is the best example we have of rationality. Our view of what it is to be rational depends in significant ways, though of course not exclusively, on what we take to be the essential aspects of scientific behavior. That is not to say that any scientist behaves rationally at all times, or even that many behave rationally very much of the time. What it does assert is that, if history or any other empirical discipline leads us to believe that the development of science depends essentially on behavior that we have previously thought to be irrational, then we should conclude not that science is irrational but that our notion of rationality needs adjustment here and there (Kuhn 1971, p. 144).

This is not the place to proceed with the terms of the debate itself. What I had to do in this section was to show the existence of the debate and its nature. In the next section, I will argue that this debate is the debate surrounding the OHS and the NHS, developed in a complex way within the theoretical and historical circumstances surrounding change in the historiography of science. It started with some inconsistencies between the NHS and the OPS and that is why there is so much talk about the role of the history of science for the philosophy of science in a given period. In addition to the texts mentioned above, this can also be seen in articles by authors who, in the 1970s, discussed the relation between the disciplines talking about "intimate relationship or marriage of convenience", as in the case of Ronald Giere in his 1973 review article.[23]

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[23] See, for instance, Giere 1973, McMullin 1976 and Burian1977. It can be said that this debate is part of the broad Lakatos-Kuhn debate since Kuhn's last article on the subject ("The Halt and the Blind") is from 1980. For a recent outline of the subject see Schickore 2011 and Giere 2011.

In the work he reviews, the role of the history of science in the philosophy of science is generally discussed starting from the question "What can the history of science do for the philosophy of science and how?" (see Giere 1973, p. 286). My purpose here is not to discuss this question, but an associated historical question: What has the history of science *done* for the philosophy of science? It refers precisely to the fact that the NHS began to raise problems for philosophers of science, outlining an alternative image of science, which was later developed by Kuhn and others as an NPS. It was discussed, as we saw in the cases of Popper and Lakatos, whether the history of science could do this, but it is important to note that historically this is what it did.

4. Final considerations

It is in this context that one can understand the absence of a direct debate between the supporters of the OHS and the NHS and the transference of this debate to the arena of philosophy of science, bringing together the supporters of OPS and NPS. A debate about the image of science, which began inchoately as an area of friction or tension between the NHS and the OPS, and that was fully established when Kuhn (with *Structure*) and others presented a new image of science (a NPS). This new image seeks to preserve certain traits traditionally associated with science -- such as the idea that science is characteristically a rational activity and that it presents progress -- without abandoning the idea that science is a human activity, subject to the same vicissitudes of other "sublunary" activities. It is not necessary, as traditionally supposed, to think of science as a "superlunary" activity, with a purely logical rationality, a progress conceived as being strictly cumulative and a historiography of science compatible with it (the OHS).

As I tried to show in the article in which I use these Aristotelian expressions, this conception stemmed from a historical moment in which science was justifiably seen as fully realized in the Newtonian theory. Even after a considerable change in this conception, having Einstein's theory as its axis, the traditional idea of rationality and progress remained an active norm in positivism (see PO 2020, pp. 383-386 and 392). The positivists are certainly seduced by the fact that, as Kuhn says, "in those years one knew how science worked and what scientific progress was" (Kuhn 1986, p. 33).

As long as it was not possible to develop an image of science consistent with the ideas of rationality and progress, the NHS would not sustain itself, at least from Kunh's point of view. It would be just a kind of Lakatos's external history, offered to illustrate the deviations, the irrationalities of human behavior in its struggle for knowledge (see the previous section). Kuhn writes that he needed to reconcile what was revealed by the NHS and the notion of rationality traditionally associated with science (Kuhn 1971, p. 144, quoted above. See also Kuhn 1970a, p. 8, and Kuhn 2000, pp. 129-130).

This is much more than merely making explicit the image of science embedded in the NHS. The demand was that this image should not be irrational and detached from possible progress. The aim of the project, Kuhn's project in *Structure*, was therefore to justify the NHS. Without it, according to Kuhn, the NHS could not be admitted as a historiography of *science*. That is why I speak here of a shift of debate from the strictly historiographical level to the philosophical level. The move from the OHS to the NHS was almost natural, as we have seen, but it could not be safely taken without the support from the NPS, a philosophical revolution in the conception of science (see Kuhn 1970b, p. 67).[24]

Kuhn says that *Structure* was written for philosophers, although it did not initially come to their attention (see, for example, Kuhn 2000, p. 307). And he says that it was only when he was already at Princeton (1964-1978, see Marcum 2015, pp-18-19) that he began to receive philosophers' attention:

> I got invited to talk at a couple of places, and I was glad to, but I wasn't very well received. I was not really getting through to philosophers, although some of them were very interested. When I got to Princeton, I began to work a good deal with Peter [Carl Hempel]. This was the first philosopher, I guess of any sort, but certainly the first philosopher in the logical empiricist tradition who began to respond, and to respond seriously to what I was doing. (Kuhn 2000, p. 309)

And Kuhn admits that the Bedford College symposium may have broadened his access to philosophers (Kuhn 2000, pp. 306-307). The focus on Bedford College and its ramifications, as we have done here, is justified by the fact that it is the first broader reaction, with a well-characterized debate about the relation between history of science

[24] On Kuhn's philosophical perspectives see, for example, Hoyningen-Huene 1998, Sharrock and Read 2002, Kindi 2005 and Mladenovic 2007.

and philosophy of science. The positivists, although they included a monograph on the history of science in their famous *Encyclopedia*, did not flee from a context in which the relation between the two disciplines is still seen in a traditional way. Suffice it to remember that Neurath invited George Sarton, "synonymous" with OHS, to write the monograph on the history of science in 1938 (replacing Federigo Enriques).[25] Sarton declined the invitation due to lack of time and nominated Bernard Cohen, who, in turn, nominated Kuhn to do so. This choice seems to point to a continuity, the choice of "a third Harvard historian of science", as Peter Galison says (Galison 1995, p. 30), but a great discontinuity in the historiography of science and in the relationship between history of science and philosophy of science already was on the way (see PO 2015, section 2, and 2007, pp. 152-153).[26]

Popper brought historical elements to the philosophy of science debate, despite taking the history of science almost entirely as an illustration of his philosophy of science, at least after Kuhn's critique. It is true that the debate on *Structure* or Kuhn's philosophy of science involved other aspects, which became dominant, such as the notions of paradigm and incommensurability, but the question of the relation between history of science and philosophy of science was debated as a previous issue. As I said in section 3, the critique of the Popperians intended to undermine the very perspective assumed by the Kuhnian philosophy and thus *in limine* discard it for violating basic methodological or epistemological principles even before giving it the floor.

The relation between history of science and philosophy of science is the theme of the debate that interested us here, as issues relevant to the historiographical revolution in the history of science are discussed through it. A debate that is established as a debate between the OHS-OPS and NHS-NPS "packages", with the participation of historians as philosophers as well. According to Kuhn, as we saw above, historians of science have a responsibility for helping the philosopher's job "because historians played a primary role in the destruction of the traditional viewpoint" about science (Kuhn 1986, p. 33).

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[25] At that same time, Neurath also wrote about the relation between philosophy and science in a traditional way (see PO 2021, pp. 68-69, and 2015, section 2).

[26] In Rheinberger 2010 the author speaks of "the historicization of the philosophy of science" and "the epistemologization of the history of science". Both movements, he says, "which are to be combined under the concept of historical epistemology, give the resulting history its robustness and strength" (pp 3-4 and 51).

It is worth noting that the so called 'historicist turn' in the philosophy of science was one of the most prominent consequences of Kuhn's *Structure*. And this was the outcome of a relatively long historical process. Only after the change of meaning of the term "philosophy of science" was Kuhn himself fully considered a philosopher of science (see PO 2007, pp. 152-153). And, in what directly interests us here, we can say not only that the debate was shifted from historiography of science to philosophy of science but that the historians themselves, or at least Thomas Kuhn with *Structure*, did it.

I think that the absence of a debate in the historiography of science does not result from the fact that it is a historiographic revolution (a revolution in the Humanities) and not a scientific revolution. When the article was almost finished, it occurred to me to ask whether a complexity like that of the historiographic revolution had been observed by Kuhn in any scientific revolution. I was soon taken to *The Copernican Revolution* and found several passages in the book that could be considered here. The debate is plural or the revolution is plural, says Kuhn about the Copernican revolution, right at the beginning of the preface. According to him, this plurality (and he speaks of "creative interdisciplinary ties") is what is new in his book on the subject. He writes:

> Because of its plurality, the Copernican Revolution offers an ideal opportunity to discover how and with what effect the concepts of many different fields are woven into a single fabric of thought. (...) Though his [Copernicus'] *De Revolutionibus* consists principally of mathematical formulas, tables, and diagrams, it could only be assimilated by men able to create a new physics, a new conception of space, and a new idea of man's relation to God. Creative interdisciplinary ties like these play many and varied roles in the Copernican Revolution. (Kuhn 1995, p. vii)

And I believe I can add another quote, which refers to the Copernican revolution but also allows us to think about the apparently mild character of historiographical change in the history of science:

> ...if the decision between the Copernican and the traditional universe had concerned only astronomers, Copernicus' proposal would almost certainly have achieved a quiet and gradual victory. But the decision was not exclusively, or even primarily, a matter for astronomers, and as the debate spread from astronomical circles it became tumultuous in the extreme. To most of those who were not concerned with the detailed study of celestial motions, Copernicus' innovation seemed absurd and impious. Even

> when understood, the vaunted harmonies seemed no evidence at all. The resulting clamor was widespread, vocal, and bitter. (Kuhn 1995, p. 188)

Up to a point, this seems to have happened in the historiographical revolution. If the decision were only for historians of science, there would have been a "quiet and gradual victory" for the NHS. But the decision was neither exclusively nor primarily up to the historians. In terms of philosophy of science, from the traditional point of view, Kuhn's theory "seemed absurd and impious" and it can be said that the "widespread, vocal, and bitter" debate, which then began, has not yet come to an end.

To summarize the main features of the subject discussed in the previous pages with their nuances and caveats, I could say that:

1. Kuhn says that there is a "historiographical revolution" in the history of science. But there is no evidence of a debate on the historiography of science during the process of shifting to what Kuhn calls the "new historiography" of science.

2. The absence of a debate or a mild change in the historiography of science would, in principle, undermine the Kuhnnian idea of a historiographical *revolution* insofar as it could suggest the absence of incommensurability between the OHS and the NHS.

3. In an attempt to overcome the impasse, I argue in three directions:

A. I admit that there is incommensurability between the OHS and the NHS, and a historiographical revolution.

B. I try to explain the absence of a debate on the historiography of science, showing that the NHS, since Koyré, has naturally adjusted to a historiography already practiced in relation to other disciplines (philosophy, art) and in the so-called general history. The NHS is new and revolutionary only in the context of science. On the other hand, Sarton was deeply engaged in a project of promoting the history of science as a discipline and was not very sensitive to methodological disputes, which could mean an internal division.

C. I suggest that the wide-ranging debate about the relations between the history of science and the philosophy of science after *Structure* is part of the change in the historiography of science. This is true at least for Kuhn, since he considers it essential that the NHS be consistent with the generally accepted idea that science is the best example of rationality. He writes *Structure* to philosophically justify the NHS. It can therefore be said that the NHS, which began with Koyré's work in the 1940s, was consolidated with *Structure* and the proposal of a NPS.

To conclude: If what history of science describes as science can be understood as rational, then a role for the history of science in the philosophy of science is justified. According to Kuhn, this role can be played whenever what is revealed by the empirical study of science can be considered rational. Even if for this it is necessary to seek a new concept of rationality. And it should be added that the history of science, the sociology of science and other disciplines seem to reveal contexts and practices of rationality that need to be investigated in order to understand science.

References

Buck, Roger and Robert Cohen (eds.) 1971. *PSA 1970: In Memory of Rudolf Carnap. Proceedings of the 1970 Biennial Meeting, Philosophy of Science Association. Boston Studies in the Philosophy of Science*, vol. 8. Dordrecht: Reidel.

Burian, Richard. 1977. "More than a Marriage of Convenience". *British Journal for the Philosophy of Science*, 44.

Butterfield, Herbert. 1966 [1949]. *The Origins of Modern Science 1300-1800.* The Free Press, New York.

Casini, Paolo. 1987. Considérations sur Koyré et l'Italie. In Redondi 1987, pp. 93-99.

Chimisso, Cristina. 2008. *Writing the History of the Mind: Philosophy and Science in France, 1900 to 1960s*. Hampshire: Ashgate.

Clagett, Marshall (ed.). 1959. *Critical Problems in the History of Science*. Madison: The University of Wisconsin Press.

Cohen, I. Bernard and René Taton (Eds). 1964. *Mélanges Alexandre Koyré.* Paris, Hermann, 1. *L'aventure de la science.*

Cohen, I. Bernard. 1966. "Alexandre Koyré (1892-1964): Commemoration" (with a text by Marshall Clagett). *Isis*, 57, 157-166.

Cohen, I. Bernard. 1984. "A Harvard Education" - Sarton, Science, and History. *Isis*, 75, 13-21.

Cohen, I. Bernard. 1987. Alexandre Koyré in America: Some Personal Reminiscences. In Redondi 1987, pp. 55–70.

Crombie, Alistair (ed). 1963. *Scientific Change: Historical studies in the intellectual, social and technical conditions for scientific discovery and technical invention, from Antiquity to the present – Symposium on the history of science, University of Oxford, 9–15 July, 1961*. London: Heinemann.

Dauben, Joseph et al. 2009. "Seven Decades of History of Science: I. Bernard Cohen (1914–2003), Second Editor of Isis". *Isis*, 100, 4-35.

Galison, Peter. 1995. Context and Constraints. In J. Buchwald (Ed.), *Scientific Practice: Theories and Stories of Doing Physics*. Chicago: University of Chicago Press.

Giere, Richard. 1973. "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?". *British Journal for the Philosophy of Science*, 24, 282-297.

Giere, Richard. 2011. History and Philosophy of Science: Thirty-Five Years Later. In: Seymour Mauskopf and Tad Schmaltz: *Integrating History and Philosophy of Science: Problems and Prospects - Boston Studies in the Philosophy of Science*, vol. 263, 59-65.

Guerlac, Henry. 1963. Some Historical Assumptions of the History of Science. In Crombie 1963, pp. 797-812.

Harvey, Joy. 1999. "History of Science, History and Science, and Natural Sciences. Undergraduate Teaching of the History of Science at Harvard, 1938-1970". *Isis*, 90, 270-294.

Hoyningen-Huene, Paul. 1998. "On Thomas Kuhn's Philosophical Significance". *Configurations*, 6, 1-14.

Kilgour, Frederick. 1953. Administrative Documents. *Isis*, Vol. 44, No. 1/2, pp. 205-210. URL: https://www.jstor.org/stable/227673 Accessed: 30-10-2020 11:08

Kindi, Vassiliki. 2005. "The Relation of History of Science to Philosophy of Science in *The Structure of Scientific Revolutions* and Kuhn's Later Philosophical Work" *Perspectives on Science*, 13, 4, 495–530.

Koyré, Alexandre (1939). 1966. *Études Galiléennes.* Paris: Hermann.

Koyré, Alexandre. 1952. "An Unpublished Letter of Robert Hooke to Isaac Newton". *Isis*, 43, 4, 312-337.

Koyré, Alexandre (1966) 1973. *Études d'Histoire de la Pensée Scientifique*. Gallimard, Paris.

Koyré, Alexandre. 1986. *De la Mystique à la Science: Cours, Conférences et Documents 1922–1962*. Redondi, P. (ed). Paris: EHESS Éditions.

Kuhn, Thomas S. 1970a. *The Structure of Scientific Revolutions*. 2nd edition. 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. 1971. Notes on Lakatos. In Buck and Cohen 1971.

Kuhn, Thomas S. 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.

Kuhn, Thomas S. 1980. "The Halt and the Blind: Philosophy and History of Science". *British Journal for the Philosophy of Science*, 31: 181-192.

Kuhn, Thomas S. 1984."Professionalization Recollected in Tranquility. *Isis* 75: 29-32.

Kuhn, Thomas S. 1986. "The Histories of Science: Diverse Worlds for Diverse Audiences". *Academe*, Vol. 72, No. 4, pp. 29-33

Kuhn, Thomas S. (1957) 1995. *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*. Cambridge, MA: Harvard University 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.

Lakatos, Imre. 1971. History of Science and its Rational Reconstructions. In Buck and Cohen 1971.

Lakatos, Imre. 1977. *The Methodology of Scientific Research Programmes* (Philosophical Papers, vol. 1). Cambridge, Cambridge University.

Lakatos, Imre & Alan Musgrave (Eds). 1970. *Criticism and the Growth of Knowledge*. Cambridge, Cambridge University.

Marcum, James. 2015. *Thomas Kuhn's Revolutions: A Historical and an Evolutionary Philosophy of Science?*. London: Bloomsbury.

McMullin, Ernan. 1976. History and Philosophy of Science: A Marriage of Convenience? PSA 1974: Proceedings of the Biennial Meeting of the Philosophy of Science Association. Dordrecht: Reidel

Mladenovic, Bojana. 2007. "'Muckraking in History': The Role of the History of Science in Kuhn's Philosophy". *Perspectives on Science* 15, 3, 261–294

Multhauf , Robert. 1975. "Fiftieth Anniversary Celebration of the Society". *Isis*, 66, 4, 442-482

Neurath, Otto. [1932] 1987. Unified Science and Psychology. Pp. 1–23 in *Unified Science*. Edited by Brian McGuiness. Dordrecht: D. Reidel.

Nickles, Thomas. 1995. "Philosophy of Science and History of Science". *Osiris*, 10, 138-163

Oliveira, Amelia. 2017. "Duhem's Legacy for the Change in the Historiography of Science: An Analysis Based on Kuhn's Writings". *Transversal: International Journal for the Historiography of Science*, 2, 127-139.

Pinto de Oliveira, J. C. 2007. "Carnap, Kuhn, and Revisionism: On the Publication of Structure in Encyclopedia." *Journal for General Philosophy of Science* 38: 147-157.

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. 2015. "Carnap, Kuhn, and the History of Science: A Reply to Thomas Uebel." *Journal for General Philosophy of Science* 46: 215–223.

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, Condorcet, and Comte: On the Justification of the 'Old' Historiography of Science." *Perspectives on Science* 28(3): 375–397

Pinto de Oliveira, J. C. 2021. Kuhn and Logical Positivism: On the Image of Science and the Image of Philosophy. In *Interpreting Kuhn*. *Critical Essays*. Edited by Brad Wray. Cambridge: Cambridge University Press

Pinto de Oliveira, J. C., and Amelia J. Oliveira. 2018. Kuhn, Sarton, and the History of Science. In *Hypotheses and Perspectives in the History and Philosophy of Science. Homage to Alexandre Koyré 1964–2014*. Edited by Raffaele Pisano and Joseph Agassi. Dordrecht: Springer.

Popper, Karl. (1934) 1968. *The Logic of Scientific Discovery.* N.York, Harper.

Popper, Karl. 1970. Normal Science and its Dangers. In Lakatos & Musgrave 1970.

Redondi, Pietro (ed). 1987. Science: The Renaissance of a History. Special Issue. *History and Technology,* vol 4. Proceedings of the International Conference Alexandre Koyré, Paris, Collège de France, 10–14 June 1986.

Redondi, Redondi. 1987b. Foreword. In Redondi 1987, pp. 1-6.

Reisch, George. 2019. *The Politics of Paradigms: Thomas S. Kuhn, James B. Conant, and the Cold War "struggle for men's minds"*. Albany: State University of New York.

Rheinberger, Hans. 2010. *On Historicizing Epistemology*. Translated by David Fernbach. Stanford: Stanford University Press

Sarton, George. 1952. *A Guide to the History of Science*. Chronica Botanica Company, Waltham, Massachusetts.

Sarton, George. 1962. *Sarton on the history of science. Essays by George Sarton*. Stimson, Dorothy (ed). Cambridge: The Harvard University Press.

Schickore, Jutta. 2011. "More Thoughts on HPS. Another 20 Years Later". *Perspectives on Science.* 19, 4, 453-481.

Sharrock, Wes and Rupert Read. 2002. *Kuhn. Philosopher of Scientific Revolutions.*

Cambridge: Polity Press.

Stimson, Dorothy. 1953. "The Editing of Isis". *Isis* , Vol. 44, No. 1-2, Jun., pp. 3-4

Stimson, Dorothy. 1962. Preface. In Sarton 1962. pp. v–x.

Thackray, Arnold. 1970. Science: Has its Present Past a Future? In Stuewer, R. (Ed.). *Historical and Philosophical Perspectives of Science*. Minneapolis: Univ of Minnesota Press, 1970, pp. 112-133

Thackray, Arnold. 1980. "The Pre-History of an Academic Discipline: The Study of the History of Science in the United States, 1891-1941". *Minerva* 18 (3):448-473

Thackray, Arnold. 1995. "Constructing Knowledge in the History of Science". *Osiris*, 2nd Series, Vol. 10, pp. vi-viii.

Thackray, Arnold and Robert Merton. 1972. "On Discipline Building: The Paradoxes of George Sarton". *Isis* 63/4:472–495.