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Dossier Pierre Duhem

Duhem's Legacy for the Change in the Historiography of Science: An Analysis Based on Kuhn's Writings

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Abstract:

What is the contribution of Duhem's work to the modern historiography? His interpreters have been discussing this question and ordinarily have recognized that the main aspect in his extensive work is connected with his research of medieval science. It has become customary to speak of the "discovery of medieval science" as his foremost historiographic achievement. This paper aims to discuss some aspects of Duhem's historiography more for its promotion of a new historical perspective than for its results. Duhem's legacy for modern historiography can be investigated from the characteristics that mark this new perspective, as regarded by Thomas Kuhn.

Keywords:

Pierre Duhem; Thomas Kuhn; historiographic revolution; new historiography of science

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Introduction

Pierre Duhem is commonly regarded as a pioneer in the study of medieval contributions to the development of modern science.² For many of his interpreters, his extensive historical work has generated a true revision of scientific development, to the point of speaking of a historiographic revolution. However, in *The structure of scientific revolutions* (SSR), precisely when announcing an ongoing revolution in the historiography of science and citing the names of some influential historians, Kuhn does not mention Duhem. It is only in an article published a few years later – "The history of science"³ –, in which he globally examines the development of this field of study, that Duhem is remembered for his contribution to the modern historiography of science. In this work, I try to show that although Kuhn does not include Duhem's name among the 'new' historians nor does he refer to any of his works in SSR, Duhem's work is strongly present through its influence on some of the historians that Kuhn cites.

Initially, I begin by identifying the citations Kuhn makes of Duhem's name in his published work and by briefly mentioning some considerations of interpreters who have tried to establish relationship between Kuhn and Duhem. Next, I will try to indicate, according to Kuhn, the fundamental distinctive features between

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² For example, see Koyré (1973 [1966], 61) and Butterfield (1966 [1949], 27).

³ Published in 1969. Reprinted in his *The essential tension* (1977).



an older historical tradition and the emerging new historiography of science. I expect to demonstrate how these features were already present in Duhem's work and how his influence can be felt in his followers' works. Finally, considering that the rise of a new historiography of science is undeniably due to Duhem's legacy, and that the central features of this new historiography mark his historical production, I will try to understand why his name was entirely neglected by Kuhn in his study of scientific revolutions.

Duhem in Kuhn's Work

In a work published to mark the 50th anniversary of the publication of SSR, Brad Wray (2015, 168) says that although an author may be influenced by more sources than he cites, "citations are a good place to start looking, in order to understand the influences" on him. Curiously, he affirms that "Kuhn does not cite some of the sources that clearly influenced him".⁴

If Kuhn does not mention Duhem in SSR, he does so in some of his other works. In his first article⁵ – "Robert Boyle and structural chemistry in the Seventeenth Century" – Kuhn cites Duhem in two notes. In the first, he (1952, 12) considers that Duhem, among other historians, explores a simplification "which treats all atomisms as mere particulate theories" and although he bears in mind that this simplification had illuminated portions of the history of Chemistry, it had also been misleading. In the second, Kuhn (1952, 13) says that "Duhem sees Boyle's application of the corpuscular theory of matter as the first source of 'the notion of a simple substance such as that provided by Lavoisier and his contemporaries'".⁶

It is interesting to note that Kuhn, in this article, contests almost all of the history of Boyle's role in chemistry in the seventeenth century, maybe with the exception of Marie Boas's studies. And he cites a number of historians in his 90 notes, many of which are cited in SSR.⁷

It is also in notes that Duhem's name appears in *The Copernican revolution*, Kuhn's book published in 1957. In the introductory part of his bibliographical notes, Kuhn asserts that Duhem's *Le système du monde* could have been used very often in the composition of *The Copernican revolution*, but that it was only consulted "for special topics" (1970b [1957], 284).⁸ When Kuhn (1970b [1957], 286) indicates bibliographies for the chapters, he refers to Duhem's *Le système* for research on Arabic and medieval European astronomy.

In the same manner as in the article on Boyle, the bibliographical notes in *The Copernican revolution* are very extensive and anticipate the mentions of many scholars cited in SSR.⁹ It is also important to note that both the article on Boyle and the book on the Copernican revolution are works on the history of science and, although Kuhn traces some historiographic considerations in those works, he did not do so in relation to Duhem.

In fact, Kuhn presents a historiographic analysis in his (autobiographical) preface to SSR. There he writes about his immersion in the study of the history of science and about a group of historians that "has shown what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today" (Kuhn 1996 [1962], viii). He mentions the names of Alexandre Koyré, Émile Meyerson, Hélène Metzger and Anneliese Maier and their works that were "particularly influential" to a new historical perspective. By describing the functions of the history of science, as bequeathed by tradition,

⁴ Although the focus of Brad Wray's discussion is different from the one here intended, his analysis of citations made by Kuhn in SSR is informative: "Kuhn cites 127 different sources in the first edition of *Structure*, with a total of 206 citations. An analysis of these sources suggests that, even though *Structure* profoundly influenced scholarship in history, philosophy, and sociology of science, Kuhn drew mostly on work in the history of science" (Wray 2015, 168).

⁵ See Kuhn (2000, 291).

⁶ This view provided by Duhem in *Le mixte et la combinaison chimique* (1902), according to Kuhn, was dominant among the historians of science. So far as he knew, only Marie Boas would have explicitly indicated (until then) that Boyle did not believe in the existence of elements – a thesis defended by Kuhn in his article.

⁷ Among others, E. Meyerson, E. Burt, M. Boas, H. Kopp, J. R. Partington, D. Mckie, H. Butterfield, H. Metzger, A. Koyré and R. Hooykaas are cited in SSR.

⁸ Kuhn cites six out of ten volumes of *Le système du monde*. The seventh volume was published only in 1956, shortly before Kuhn published his book.

⁹ A. Koyré, H. Butterfield, E. J. Dijksterhuis, A. C. Crombie, E. Burt, M. Clagett, A. Maier and M. Boas are examples. In SSR, Kuhn's own *Copernican revolution* is cited half a dozen times.

he announced a change that represents the beginning of a “historiographic revolution”, of which representative historians

have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. 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 1996 [1962], 3).

Compared with the writings of historians of the older historiographic tradition “these historical studies suggest the possibility of a new image of science” (Kuhn 1996 [1962], 3). Kuhn presents the SSR as an essay about this image emergent from the “new historiography”.

Some years later, in an article published in 1968 – “The history of science” – Kuhn speaks again of a “new historiography” or, in a correlative way, of a “modern historiography of science” (Kuhn 1977). In this work, Duhem's name is mentioned twice. Firstly Duhem is positioned in a historiographic tradition that was more philosophical in its objectives. His writings are remembered alongside Whewell's and Mach's as those in which the “philosophical concerns became a primary motive for creative activity in the history of science” (Kuhn 1977, 106). Kuhn's second mention of Duhem is more expressive, because it occurs when he is writing about factors that have contributed to the historiographic change and mentions “another decisive event on the rise of the contemporary profession” (Kuhn 1977, 108):

Almost a century after the Middle Ages had become important to the general historian, Pierre Duhem's search for the sources of modern science disclosed a tradition of medieval physical thought which, in contrast to Aristotle's physics, could not be denied an essential role in the transformation of physical theory that occurred in the seventeenth century. Too many of the elements of Galileo's physics and method were to be found there. But it was not possible, either, to assimilate it quite to Galileo's physics or to Newton's, leaving the structure of the so-called Scientific Revolution unchanged but extending it greatly in time. The essential novelties of seventeenth-century science would be understood only if medieval science had been explored first on its own terms and then as the base from which the “new science” sprang. More than any other, that challenge has shaped the modern historiography of science (Kuhn 1977, 108).

In this article, Kuhn cites Duhem's *Études sur Léonard de Vinci*¹⁰ in the bibliography, among about 60 other works that support his argument. Here, Duhem's work seems very significant in the formation of a modern historiography of science.¹¹ So, the question that may be raised is: If Kuhn considers the rehabilitation of Middle Ages science to be significant to the emergence of a new historiography of science, why does he not cite Duhem among so many other scholars in his main book?

This question invokes the attempts of some interpreters to establish a relationship between Duhem and Kuhn. Agassi (2002, 409), for example, in his review of the posthumous collection of essays by Kuhn, published in 2000 as *The road since structure* (RSS), asserts that “Kuhn ignored his debt to Duhem while respecting his leading followers (286-287)”. He indicates pages 286 and 287, where Kuhn mentions the names of philosophers and historians of science, such as Koyré, Meyerson, Metzger and Maier. Agassi

¹⁰ *Études sur Léonard de Vinci* (1906-1913). The first and second volumes have as subtitle *Ceux qu'il a lus et ceux qui l'ont lu*, the third, *Les précurseurs parisiens de Galilée*. Hereafter cited as *Études*.

¹¹ Kuhn cites, among others, A. Koyré, M. Boas, H. Butterfield, M. Daumas, H. Guerlac and H. Metzger. All these historians appear in SSR related to some aspects of the emergent new historiography of science.

(2002, 409) ironizes the fact that Kuhn remembers Duhem in RSS only because of the fact that he invented a term.¹²

Stanley Jaki (1987) and John Worrall (1995) also discuss the rapprochement between Duhem and Kuhn's views. The former, one of the greatest scholars of Duhemian work, by suggesting that Duhem made many contributions to the philosophy of science, claims it to be surprising to a judicious reader of Duhem that there is not a single reference to him in SSR (Jaki 1987, 370).¹³

John Worrall does not limit himself to presenting similar aspects between the two philosophers and historians of science, going so far as to say that there is

nothing of real relevance to this particular issue in *The Structure of Scientific Revolutions* that was not raised already in Duhem's *The Aim and Structure of Physical Theory*. Indeed many of the Kuhnian theses that have created such a stir in philosophy of science seem at root to be (often rather less clear) restatements of Duhemian positions (Worrall 1995, 77).

Those considerations can even be suggestive, but they are very fast and general. Moreover, they are presented with a focus on the philosophy of science and not on the historiography of science, which is the scope that interests me. In this sequence, I try to provide some material to help bring Duhem closer to the new historiography of science, from the characterization of historiographic change, as expounded by Kuhn.

From the Older to the New Historiography of Science: Duhem's Spot

When Kuhn recognizes an ongoing historiographic revolution in SSR, he exhibits the state of change in the historical perspective. Traditionally, the history of science was conceived as the discipline that recorded the successive increments of scientific technique and knowledge while at the same time registering the obstacles that have inhibited their accumulation. Under this conception, according to Kuhn (1996 [1962], 2), the historian had two main tasks: to "determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented" and to "describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text". This perspective is particularly found in science textbooks, in which discarded theories are considered unscientific¹⁴ and what is relevant is the identification of the individual contributions, the place and the date they occurred.¹⁵

In the ongoing historiographic revolution – "still in its early stages" (Kuhn 1996 [1962], 3) – a group of historians have begun to put together other kinds of questions. "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" (Kuhn 1996 [1962], 3). Kuhn's example is about the study of Galileo's contributions. Some historians no longer sought Galilean contributions in relation to modern science, but sought to understand it in their own context, that is, in "the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences". And, as says Kuhn, they also insisted upon studying the conceptions of these thinkers from a viewpoint that is very different from that of modern science, that gave those conceptions "the maximum internal coherence and the closest possible fit to nature" (Kuhn 1996 [1962], 3).¹⁶ In doing so, they presented works that provided a completely divergent image of science from the one supplied by writers in the older historiographic tradition.

¹² In RSS, Kuhn (2000, 235) says: "When I entered history of science, it was customary, largely due to the influence of Pierre Duhem, to speak of 'medieval science' and I often used that highly questionable phrase myself".

¹³ Jaki (1987, 370) cites works by Cardwell and Beauregard, who explore the comparison of similarities between Duhem and Kuhn, noting that the matter is dealt with politeness and fear by the former, and discretion by the latter. His considerations are limited to a note.

¹⁴ Kuhn (1996 [1962], 99) explores the example of the "much-maligned phlogiston theory".

¹⁵ "When oxygen was discovered?" is Kuhn's example (1996 [1962], 2), by which he refers again to the history of the development of chemistry.

¹⁶ Alexandre Koyré's writings are considered here as, "perhaps", the best example of this new way to investigate the past of science.

From these brief considerations, it is possible to extract some important features that distinguish the work of some historians from those that represent the older historiography. Those who were promoting the revolution showed that the history of science (1) could and should be more than a repository for anecdotes or chronologies, (2) can no longer be conducted by the debates about priorities and (3) was based on an attempt to understand the past in its own terms. These features are interrelated and have implications in other aspects that, as we shall see, would constitute the modern history of science. Now I will try to show that those features are fundamental in Duhem's work.

Duhem: A Source for the New History of Science

Duhem's work in no way resembles the old manuals of the history of science, which are a repository for more of the anecdotes or chronologies. It is a repository of ideas, discussion and reflections. If we compare it with, for example, Sarton's, the differences are enormous, although Sarton had also been a historian devoted to the study of medieval science.¹⁷ Crombie (1959, 164), by the way, helps us think in this comparison in his review of two of Sarton's works, by saying that "Sarton was a man of facts rather than ideas", that "his most substantial contribution was to the bibliography of early science" and that "he did not work with the philosophical and analytical approach to the history of science such as is now, in the hands of younger scholars, throwing so much light on the development and character of scientific thinking".¹⁸ We have here the suggestion of a change in the history of science and we can suggest that the "younger scholars" were, like Crombie, the historians that, according to Kuhn, are making the revolution.

In Duhem's work, biography or bibliographical considerations are justified by the insertion of new, previously unknown characters in the history of science. Such is the case, for example, for his *Études sur Léonard de Vinci*. Before speaking of Albert of Saxony's influence on Leonardo da Vinci, Duhem dedicates a section to explain who Albert of Saxony, a name hardly pronounced in the history of science, is. In the beginning of his *Études*, Duhem (1984 [1906], v.1, 1) states that the history of science is misrepresented by two prejudices, so similar to each other that they could be taken as one: that scientific progress occurs through sudden and unforeseen discoveries and that the works of genius men have no precursors at all. He insists that the great discoveries

are almost always the result of a slow and complicated preparation, chased in the course of the centuries. The doctrines professed by the most influential thinkers come from a multitude of efforts, accumulated by a series of obscure workers. Those who we are accustomed to call creators, Galileo, Descartes, Newton, did not formulate any doctrine that was not bound by the innumerable lines to the teachings of those who preceded them (Duhem 1984 [1906], v.1, 1-2).

This passage is only one example among many other of Duhem's manifestations against the "eureka-moment"¹⁹ notion of a scientific discovery. He invariably criticizes the history of science that celebrates only the great discoveries and suggests the innovative character of his work that includes unknown contributions. The preface to Maire's work (1912) is a good example of Duhem's discussion about the difficulties involved in trying to determine priorities in the history of science:

There is a fine line between a scientific discovery and the personality who made it. In many circumstances, time quickly dissolves it. Sometimes, over the centuries, treatises and manuals continue to link the inventor's name to a mathematical proposition; to the law of physics, the name

¹⁷ In the introductory chapter of his most famous work, Sarton (1927) recognizes the need to consider the medieval science, but from an evaluative perspective very different from Duhem's. Kuhn (2000, 282) suggests the mention of his name as an example of "history of manuals". See also Preston (2008, 80-10) and Pinto de Oliveira and Oliveira (forthcoming).

¹⁸ It is worth noting that, in spite of his criticism, Crombie points out the importance of Sarton's work, "a devoted pioneer". Similarly Clagett (1957, 321) considers that Sarton's writings do not present a discussion of ideas and scientific activities and that his approach to the history of science is basically bibliographical and classificatory.

¹⁹ This expression is used by McEvoy (2010, 32) to refer to the vision of scientific discovery as a "single event of individual labor".

of the one who first enunciated it. One names: the theorems of Apollonius, the principle of Huygens. But, except some curious scholars, who then wants to carry out some research into the one who bears that name? When and where did he live? Who was he? By what sequences of meditations and essays did he come to know that truth for which he had not been entirely forgotten? These are questions that we never imagine to ask, that we do not suffer at all by seeing them unanswered (Duhem 1912, I).

Duhem places himself in a distinct position from that which prevailed in the traditional history of science. He insists on presenting a different view, according to which the historical analysis of a discovery or scientific creation is fairly complex because there is no way to establish an exact moment of its occurrence, to indicate a single name as being responsible for it, without incurring injustice and inaccuracy. "No scientific discovery is a creation *ex nihilo*", says Duhem (1912, III), and if that is truth, then

we have to explore a singularly extended domain whenever we wish to retrace the history of a discovery. It will not be enough (it's quite the opposite, actually) to meditate on the writings of the one to whom that discovery is commonly attributed. We have to search, read, compare the books of all those who, more or less directly, have been the auxiliaries of that person: the precursors who had prepared the new idea; the collaborators who assisted the inventor; the opponents who forced him to define, clarify and consolidate his thoughts; the successors who highlighted the latent fertility of this thought. We will have to review those of whom our author has spoken, those with whom he spoke and those who spoke of him (Duhem 1912, VIII).

Today, Duhem's insistence upon this question may seem exaggerated, but it is important to note the context in which he puts his argumentation. As indicated by Kuhn (1977, 106), the oldest traditions of history of science had produced "little significant historical research" before the nineteenth century. In other words, in his text of 1912 (as well as in his previous works) Duhem was writing against a point of view that was still dominant in his day, when heroic biographies were in vogue.

Duhem was aware of the innovative character of his work in relation to the traditional history of science. This can be observed, for example, in the preface to *Les origines de la statique* (1905), where he draws the reader's attention to the novelty of the content of his work, which would be singular in relation to other historical texts on the subject. The perspective presented there changed the history of static, which would entail a new ordering and characterization. On the one hand, Duhem stresses this innovation with enthusiasm, and on the other hand critically regrets classical history, which ignored the Middle Ages contributions to mankind (in science and art).

Duhem's comprehensive and investigative attitude in relation to texts of the past, however, is not only evident in relation to works and manuscripts, previously ignored by other historians that had studied medieval science.²⁰ This attitude is already present in a 1894 article, "Quelques réflexions au sujet de la physique expérimentale", in which Duhem discusses his need to understand theories of the past in his own terms. Duhem writes:

If the theories admitted by this physicist [who is investigated] are those we accept, if we agree to follow the same rules in interpreting the same phenomena, then, we speak the same language and can understand each other. But it is not always so; it is not so when we discuss the experiences of a physicist who does not belong to the same school as we do; It is not so, above all, when we discuss the experiences of a physicist fifty years, a century, two centuries apart. It is necessary, then, to establish a correspondence between the theoretical ideas of the author and ours, and, through

²⁰ According to interpreters, Duhem disclosed the medieval science during the writing of the first tome of *Les origines de la statique*. See, for example, Brenner (1990, 144) and Martin (1991, 147). Leite (2015, 28) writes about this discovery as "the 'historiographical turn' [which] occurred in a very specific way: it was the (re)discovery of Medieval manuscripts, forgotten by tradition, in which the historian glimpsed contributions that announced the modern static, which made him produce a genuinely historical work".

symbols that we accept, reinterpret what he interpreted through symbols he accepted. If we succeed in doing this, the discussion of his experience will be possible (Duhem 1987 [1894], 176).²¹

The discussions on conceptual change are recurrent in Duhem's work. While presenting the history of theories, Duhem provides elements for the interpretation of these theories in their contexts and one of the aspects that he is attentive to is the elucidation of the meaning of scientific concepts in specific contexts. The following passages, extracted from his discussion about Aristotelian physics, are examples:

The meaning of the word *movement* takes, in Aristotle's language, an extreme extension; it does not absolutely have the narrowness it has in modern physics in which it designates only the movement by which a body is transported from one place to another, the *local movement*.²²

[...]

What Aristotle calls movement in a straight line is what modern geometers name *translation movement*; all points of the moved body describe, at the same time, equal and parallel lines. The circle movement considered by the Stagirian is what we call the *rotation movement around an axis* (Duhem 1988 [1913], 160-171).²³

The attempt to understand a scientific theory of the past in its own terms led Duhem to give a distinct view from the one provided by other historians. I mentioned above (note 14) Kuhn's reference to the phlogiston theory as an example of a theory considered unscientific by the older history of science. It is worth noting that Duhem, though he had briefly discussed this theory, sought to review the dominant history about it in his day. In doing so, he postulated the need to

read Stahl's, his master Becher's, some of his predecessors', his contemporaries', his successors' writings. Now it is not at all distracting to search the yellowed and dusty pages of the old treatises of "Chemistry", in which the kabbalistic form of language confounds no less than the strange antiquity of thoughts, the germ of an idea which had to grow one day and produce our science. [...] we wish to become attentive inquisitors of the old scientific texts (Duhem 1916, 7).

For Duhem (1916, 6-7), "the victory of the oxygen theory over the phlogiston theory had in no way had the characteristics attributed to it": the inventor of phlogiston did not deserve the epithet of "mystic alchemist" and Lavoisier's victory was not "a victory of positivism over mysticism, of materialism over spiritualism". He announces one of his conclusions about the history of Chemistry in the foreword: Stahl's chemistry, in fact, contributed to Lavoisier's chemistry.

Duhem's attempt, manifested in many portions of his work, to provide a review of the past of science is closely related to some of the main features of the new historiography of science. In the following sequence, I discuss the influence of Duhem's historical analysis on some of the scholars chosen by Kuhn as his main inspirers.

Duhem and his Followers

Agassi (2002, 409) rightly notes that Kuhn acknowledged his debt to the main followers of Duhem in *The road since structure*. Koyré, Meyerson, Metzger and Maier are names presented there as those who brought forward, says Kuhn (2000, 187), "a sort of history, and an approach to history" that he admired and which he "encountered fairly early".

Kuhn invariably mentions Koyré's *Études galiléennes*, published in 1939, as an example of promising historical writing. In the introduction to this book, Koyré (1966 [1939], 11) writes: "Fortunately, it is no longer

²¹ The same passage appears in Duhem (1989a, 241).

²² Kuhn (2000, 17) also discusses the meaning of "motion": "When the term 'motion' occurs in Aristotelian physics, it refers to change in general, not just to the change of position of physical body. Change of position, the exclusive subject of mechanics for Galileo and Newton, is one of number of subcategory of motion to Aristotle".

²³ Other passages about "movement" can be found in Duhem (1988 [1913], 161, 171, 208; 1992 [1903], 10; 1989b, 466).

necessary to insist on the interest of the historical study of science. It is no longer even necessary, after the masterly work of a Duhem, of an Émile Meyerson; after those of M. Cassirer and M. Brunschvicg, to insist on the philosophical interest and fecundity of this study". The importance of Duhem's work to Koyré is not only manifest in a general way, but runs throughout the book. Besides praising Duhem's "masterly" work, Koyré (1966 [1939], 16) recognized that the history of scientific thought of the Middle Ages and the Renaissance became better known "thanks to the admirable works of Duhem".

In fact, Duhem's work is mentioned dozens of times in *Études galiléennes*, especially *Études sur Léonard de Vinci*, a source for discussion and argumentation. Although Koyré disagrees with some Duhemian interpretations, his recognition for his work is clear. In his "Rapport final" to the International Colloquium held in Paris in 1952, in commemoration of the 500th anniversary of Leonardo da Vinci's birth, Koyré writes:

A curious impression: that of the presence of a person, of a thought, of a work which was scarcely mentioned, which, even by those who have done so, has not been discussed, but which, as suggests by G. Santillana by the title of his communication – "*Leonardo da Vinci and those he did not read*" – seems to dominate us, or at least direct our work. It is in fact due to the admirable, but highly contestable work of Pierre Duhem and his *Études sur Léonard de Vinci - Ceux qu'il a lus et ceux qui l'ont lu*, that the problem of interpretation of Leonardo's personality and scientific work is set. (Koyré 1953, 237)²⁴

In another text about Leonardo, Koyré (1973, 100) echoes again the Duhemian historical perspective when he writes that, in order to suitably position Leonardo in the history of science, it is necessary to "confront him with his predecessors, his contemporaries and his successors."

Koyré's *Études galiléennes* was certainly one of the works that influenced Kuhn and it is worth remembering his example in SSR of studies about Galileo that focused on the relationship between that scientist's views and "those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences" (1966, 3). By the way, Anneliese Maier's researches into the precursors of Galileo have in Duhem's work an important secondary source.²⁵

It is common knowledge that historians of medieval science have contested several of Duhem's interpretations. Contemporary interpreters agree with their criticisms which, in most cases, relate to the discussions about the origin of modern science.²⁶ Maier, for example, writes:

Pierre Duhem, who must be credited with having opened up this new field of medieval studies, viewed fourteenth-century "physics" predominantly through the eyes of a natural scientist. He looked for the first glimmerings in the past of later discoveries without paying much attention to the intellectual milieu in which this "physics" belonged and without which it cannot be really understood. Since then much has changed, and scholars have for some time been treating this chapter of intellectual history and the history of science like all others, that is, as the history of ideas [...]. But despite these changes, the old controversy still arises about whether and to what extent the *physics* of the fourteenth century anticipated the theories of later classical mechanics [...] (Maier 1982 [1960], 146).

As we can see, Maier accuses Duhem of not paying attention to the context investigated, that is, of not following his own recommendations. Now, it is interesting to note that the discussion he makes, according to her, remained controversial. This corroborates the view that Duhem, instead of worrying about dates and chronologies, posed other types of problems for historians of science. Maier (1982 [1960], 77) discusses, for example, the approach of the theory of impetus as an "anticipation of the system of mechanics based on the law of inertia". Discussed by Duhem, "who first drew attention to the scholastic theory [...] the problem has been discussed repeatedly".

²⁴ I discuss the differences between Duhem and Leonardo's other interpreters in another work. See Oliveira (2016).

²⁵ The title of his work – *Die Vorläufer Galileis* – already establishes the bond with the Duhemian *Études*.

²⁶ David Lindberg (2007, 358-359) considers that Duhem's followers, such as Anneliese Maier, Marshall Clagett, and Lynn Thorndike, drew a more careful history of science. See also Brenner (1990; 1997).

Herbert Butterfield (1966 [1949], 27), who also writes about the importance of the theory of impetus, asserts: "the work of Duhem in the field that we have been considering has been an important factor in the great change which has taken place in the attitude of historians of science to the middle ages".²⁷

Another example of a "follower" of Duhem is Hélène Metzger. In spite of her work being focused on the history of chemistry, a field in which Duhem wrote very little, her writings suggest interesting parallels between them. By considering that her work distances itself from others dedicated to the history of chemistry, Metzger (1969 [1923]), provides a series of distinctive features between her work and other historians', explaining to the reader her motivations. Inclusion of scientists and works previously disregarded, as well as the non-insertion of biographies, are some of them.²⁸ She wrote: "Most of our predecessors, in fact, have reduced their work to establish who have been the craftsmen of the discoveries of which science can boast" (Metzger 1969 [1923], 11). In a note, she considers Duhem as an "exception", pointing out that he "unfortunately only accidentally touched the history of chemistry".

It is worth noting that she refers to the discussion of priorities in history as "irritant"²⁹ and adopts the Duhemian view that the "hypotheses are not the product of a sudden creation, but the result of progressive evolution".³⁰ Her work does not contemplate "the succession of abrupt revolutions" that altered chemical theory, but "the slow evolution" that it underwent by the work of many minds (Metzger 1969 [1923], 9).³¹

A further aspect of Metzger's work, reminiscent of Duhem's attitude, concerns her considerations about the need of the modern historian to look at the changes that have occurred since that period of time and refer to antecedents, material conditions and conceptual change.³² She insisted on the need to pay attention to the differences between the context of her day and that of the investigated one, signaling her effort to describe past theories, "as they should appear to the studious disciples of their masters" (Metzger 1969 [1923], 342). For example, with respect to the history of phlogiston theory, she states that her aim "was to reconstitute the whole of the Stahl's doctrine as it appeared at the time of its elaboration, without worrying about the oversimplifications or modifications which might have altered its aspect with regard to posterity" (Metzger 1930, 5).³³

Metzger, as well as Koyré, Maier and others³⁴ recognized the importance of Duhem's work to the development of their own. In a different way, they had his writings as a source for discussing problems that were gradually changing the historical research of science. They were Duhem's followers in the face of the "older" – or, as said Duhem, of the "classic (*classique*)" or "senseless (*insensée*)" history of science (Duhem 1906, 278). And as much as his followers had disagreed with his analysis and conclusions, the role he played in promoting new and unsuspected researches is undeniable. Duhem's followers were, like Duhem himself, changing the history of science with their revisions.

It is worth noting Bernard Cohen's (1987, 56-57) account of his "painful experience of showing" Koyré "that he had made a factual error in one of his publications." Koyré would have been "chagrined and sad, terribly annoyed with himself". But, after a moment's pause, would have replied: "If Duhem had never made any mistakes, we would have had no great jobs to do. We have lived on his mistakes". Such a statement

²⁷ It is important to remind that Butterfield's *The origins of modern science* was considered "admirable and influential" and a "pioneering synthesis" by Kuhn (1977, 35 and 109).

²⁸ Metzger (1930, 11) states that she avoided "especially any anecdotal or picturesque details" concerning the work of chemists in her historical investigation. She demonstrates disagreement with Sarton about the importance of providing biographies.

²⁹ See Metzger (1935, 9). Dumas (1951, 1) also finds "irritant" the problem stemming from historical discussions about the priority of scientific discoveries. Kuhn indicates his work (1996 [1962], 53) as an "indispensable recent review [about oxygen's discovery], including an account of the priority controversy".

³⁰ Metzger (1969 [1923], 155) cites section II, Chapter VII of *La théorie physique*. Besides it and Duhem's works about Chemistry, she cites his *Le système du monde*.

³¹ Kuhn, in 1952, had already mentioned Metzger's (1930 and 1923) and Meyerson's (1951) analysis as "more acute", which showed the "chemical revolution as proceeding not from a sudden break [...] but through an almost continuous extension and elaboration of the peripatetic and iatrochemical concepts" (Kuhn 1952, 14-15).

³² See Metzger (1969 [1923], 81, 342-343; 1935, 22), and specifically on conceptual change, see Metzger (1969 [1923], 61, 205; 1935, 13 and 19).

³³ Kuhn (1996 [1962], 100, note 3) considered in SSR that "[t]he fullest and most sympathetic account of the phlogiston theory's achievements" is provided by Metzger in his *Newton, Stahl, Boerhaave et la doctrine chimique* (1930).

³⁴ Marshall Clagett and Alistair Crombie are certainly other examples.

highlights the Duhemian legacy for the promotion of historiographic change, a change that took place in different degrees among the makers of the revolution that Kuhn identified in its early stages in the early 1960s.

Final Considerations

It is important to remember that when Kuhn (1996 [1962], viii) wrote about the historians that were important in shaping his “conception of what the history of scientific ideas can be”, he noted that he was “increasingly” questioning “a few of their particular historical interpretations”. It is thus his article on Boyle that led Kuhn (1952) not only to analyze different historical views, but also to try to provide a completely new view about the subject. This is clear when, some decades later, Kuhn (2000, 291) wrote about his first article: “It is, I think, a very good article – it’s totally unreadable because I thought I had to persuade a very learned group of historians of chemistry out there. And what I gradually discovered was that nobody knew nearly as much this problem [the notion of element in Boyle’s work] as I did”.

If it is true that Duhem’s followers disputed many of their historical interpretations, it is also true that Kuhn made reservations to their works. Koyré’s work is a good example. In a text published in 1970,³⁵ Kuhn writes again about the historiographic revolution, now discussing its stages, and for him, in that context, as notes Pinto de Oliveira (2012, 119), Koyré was not “fully a new historian of science” yet.

Thus, if on the one hand, Koyré criticized Duhem for committing exaggerations in his studies on medieval science, on the other hand Kuhn questioned “how Koyré could have failed to discuss the role played by the observation of pendulums in Galileo’s argument, commenting ‘That is no trivial slip, and it illustrates something else about Koyré. He did exaggerate the universality of his insights, and he did make mistakes, very occasionally egregious ones’” (Pinto de Oliveira 2012, 118)³⁶.

For many historians, Duhem’s work is mentioned because of his researches on medieval science and this is not different with Kuhn’s work. In an interview in 1995, when asked which authors would have played a role in shaping his thinking, in addition to mentioning the customary names of Alexandre Koyré, Arthur Lovejoy, Émile Meyerson, Hélène Metzger, Kuhn (1995, 13) states: “In relation to Duhem, I have maintained principally his idea that, to understand the transition from ancient physics to modern physics, one cannot economize the medieval physics”.

We know that the discovery of scientific medieval contributions has significantly altered the historical narrative of scientific development. History, as wrote Harcourt Brown, “is the product of historians; its categories remain fluid as new outlooks and emphases produce new evaluations. [...] As the work of, for instance, Pierre Duhem has progressed and been absorbed, the perspective has changed, and much of sixteenth-century science has lost its glamor” (Brown 1960, 42).

Brown’s analysis, which is focused on the change of vision in relation to the Renaissance, can, by extension, be applied to the scientific revolution. If it is true that the Duhemian view runs counter to the Renaissance conception as a period of sparse productivity, after the darkness of the Middle Ages, for many historians, it also diminished the merit of seventeenth-century scientists and, therefore, diminished the grandeur of scientific revolution. This conception is clear in Koyré’s thought, according to which Duhem denied the occurrence of revolutions in science³⁷.

As far as I can see in his work, Kuhn most likely conceived the Duhemian work in a distinct way from his *maître*.³⁸ Incidentally, the content of one of the bibliographical notes on a text by Koyré, “Le vide et l’espace infini au XIVe siècle”, is noteworthy here:

An attack on Duhem’s absurd statement that modern science begins with edicts of Bishop of Paris in 1277 against impossibility of void, etc. Documented by detailed study of some 14th century writings about the void showing clearly that they don’t take a modern position, etc. Useful, but considerably

³⁵ Review of *Metaphysics and measurement – Essays in the scientific revolution*, published by Koyré in 1968.

³⁶ Kuhn (1977, 35, note 3) presents some caveats to Butterfield’s *The origins of modern science*.

³⁷ See Koyré (1973, 172; 1966, 15-16). I discuss this subject in another work. See Oliveira (2012).

³⁸ Kuhn (1977, 21) referred to Koyré in these words: “the man who, more than any other historian, has been my *maitre*”.

vitiated by straw-man it attacks. Really fails to see whether there is an effect of the edicts [...] (Kuhn *apud* Oliveira 2012, 239).

It could be suggested that Kuhn did not cite Duhem in his book about scientific revolutions because of Duhem's thesis of continuity. Under this assumption, it would make no sense that Kuhn quoted an author who would supposedly deny the occurrence of revolutions in science at a time when revolutions studies were the order of the day. *Prima facie*, this could be a good reason. But, when analyzing the context, I do not think this is a satisfactory reason, because of the opposition between continuity and discontinuity views, and that the notions of revolution and evolution have no place in the SSR, not even being mentioned by Kuhn in the terms proposed by some of his interpreters. Also, the transition from older to new historiography involves many nuances, and the causes by which historians would represent more or less the new or the old historical tradition depend on what would be considered relevant to the discussion.

I have tried to identify some features of new historiography according to Kuhn's writings and, based on them, show that Duhem was a real contributor to historiographic change. But, the question that can be asked is: when did the change to which Kuhn refers in SSR begin? Kuhn's review of Koyré's work allows us to temporarily locate what he called the "historiographical revolution": a movement that began with Koyré himself. Kuhn (1970a, 67) writes: "More than any other single scholar, Koyré was responsible for the first stage of the historiographical revolution". But, as we know, a revolution is not the work of a single man. Kuhn identified in SSR a group of historians. It was a group of younger scholars that was presenting a "philosophical and analytical approach to the history of science" (Crombie 1959, 164).

Mary Hesse's review of SSR corroborates Kuhn's identification with a group: "My own impression is that Kuhn's thesis is amply illustrated by recent historiography of science and will find easier accessibility among historians than among philosophers" (Hesse 1963, 286). In fact, when we observe the works Kuhn cites in SSR, we can see that most of them were published in the 50's. His more cited masters had works published since 1930. We may also recall that in 1957, Kuhn was among more than seventy participating historians of the University of Wisconsin Congress, critically discussing problems in the history of science. Among them, there were some of those cited in SSR, such as Mashall Clagett, Rupert Hall, Giorgio de Santillana, A. C. Crombie, Derek J. S. Price, Henry Guerlac, Charles C. Gillispie and Marie Boas.³⁹ In different degrees, among other historians, they were making the historiographic revolution. And Kuhn saw himself as a participant of it. Retreating to Duhem to seek the sources of modern historiography could leave the structure of the so-called historiographic revolution (as writes Kuhn about the scientific revolution) "unchanged but extending it greatly in time".

So, it is likely that Kuhn did not cite Duhem in SSR simply because Kuhn was announcing an ongoing change at a time when there was a group sharing a new perspective, which echoes "the heroic times of Pierre Duhem", a man of "astonishing energy and knowledge" (Koyré 1973 [1966], 61), but which was promoting a new sort of history in a pioneering attitude. So much so that Kuhn, when analyzing later on the development of the history of science, granted a substantial role in the formation of the new historiography to Duhem. Perhaps, in this moment he was in better conditions to analyze the change in the field, since the writings evoked by Duhem's work, such as those of Dijksterhuis,⁴⁰ Maier, and especially Alexandre Koyré, became models which many contemporaries of Kuhn aimed "to emulate".⁴¹

In any case, what matters is that since Kuhn did not link Duhem to the historiographical revolution in his most famous book, we can try to do so, both through his favorite historians' writings and through his reflections on history and philosophy of science. My attempt as a reader (perhaps less concerned with analyzing the monumental work of Duhem and more with investigating the relationship between his and Kuhn's works) has been to better understand such a stimulating part of the development of the history of science.

³⁹ See Clagett (1969 [1959]).

⁴⁰ Dijksterhuis, a historian not cited in SSR, has his *Mechanization of the world picture* (1961) considered "magistral" by Kuhn (1977, 132). As we can see, Kuhn was updating his references in 1968.

⁴¹ P. Omodeo (2016, 74-75), by mentioning Kuhn's recognition in relation to Duhem's work, writes: "The reference to the alleged success of Duhem's school is prescriptive. Kuhn counted himself as one of the 'contemporary emulators' of the medievalist".

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