Kuhn and the genesis of the "new historiography" of science

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Abstract

In this paper I identify a tension between the two sets of works by Kuhn regarding the genesis of the "new historiography" of science. In the first, it could be said that the change from the traditional to the new historiography is strictly endogenous (referring to internal causes or reasons). In the second, the change is predominantly exogenous. To address this question, I draw on a text that is considered to be less important among Kuhn's works, but which, as shall be argued, allows some contact between Kuhn's two approaches *via* Koyré. I seek to point out and differentiate the roles of Koyré and Kuhn – from Kuhn's point of view – in the development of the historiography of science and, as a complement, present some reflections regarding the justification of the new historiography.

Keywords: Kuhn, Koyré, new historiography, history of science, genesis, justification

1. Introduction

It has been said that Thomas Kuhn`s first sentence in *The structure of scientific revolutions* is "perhaps the most famous sentence in the philosophy of science of the second half of the twentieth century" (Richardson 2003, p. vii).[1] The sentence, it is worth noting, does not refer to the theory of paradigms, the main element in *Structure* that had such widespread repercussions, but rather to history; and it is a methodological observation, in two senses: it refers to the method of the history of science (proposes a change in methods, a new historiography of science) and, at the same time, to the method of the philosophy of science (proposes a role for the new historiography of science in the philosophy of science).

[1] The sentence is the following: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed" (Kuhn 1970a, p. 1).

Kuhn said that *Structure* depends on the new historiography of science, and I believe I can justify my investigation into the theme also by the importance it attributes to the history of the historiography of science (Kuhn, 1977, p. xv). Kuhn demonstrates this through his frequent autobiographical references to the episode that led him from science to its history, as well as writings devoted directly to the genesis of the new historiography of science.[2]

Speaking of genesis, the idea of the present essay emerged from a comparison of these writings by Kuhn regarding the NHS in which two well-demarcated sets can be observed. The first includes Sections I and VI of *The structure of scientific revolutions*, and an article published the same year (1962), "The historical structure of scientific discovery". On the other side is the article "History of science", originally published in 1968 and included in the 1977 collection of articles *The Essential Tension*. What called my attention, and suggested this demarcation, was that the two sets of writings offer entirely different explanations for the genesis (and justification) of the NHS. In the first set, we could say that the change in the historiography of science is understood as strictly endogenous (related to internal causes or reasons). In the article from 1968, the prevalent approach is clearly exogenous.[3]

One way to deal with this difficulty would be to assume that the more recent text offers the "up-dated" explanation, and that Kuhn abandons the earlier one. The fact is that he does not even refer to the earlier version, nor does he explicitly discard it, which leaves a margin of doubt. In addition, he republishes the article from 1962, as well as the one from 1968, in his 1977 selection of writings.

Another way to understand this question would be to juxtapose the two explanations, which are not actually incompatible. This is the approach of Hoyningen-Huene, who sees no tension between the texts, and only comments in a note that while *Structure* "lists only troubles internal to the historiography of science", the text in *Essential tension* "treats the broader complex of factors involved in a more balanced manner" (Hoyningen-Huene 2003, p. 16).

However, as we saw, the fact that Kuhn does not even refer to the endogenous explanation in the more recent text makes it difficult to accept this approach. Kuhn appears to be simply abandoning the first explanation. And even though he reproduces the 1962 text in *Essential tension*, as though he were endorsing it in 1977, in his preface to the book, Kuhn refers only to the 1968 text when discussing the genesis of the NHS (classifying it among the

[2] Abbreviated from here on as NHS.

[3] Published as an entry in *International Encyclopedia of the Social Sciences* (where Kuhn refers to himself in the third person), the 1968 article is the most specific and comprehensive text by Kuhn on the history of science.

"historiographic studies" that compose the first half of the book). The 1962 text, on the other hand, is classified among the "metahistorical studies" of the other half of the book, and is not even mentioned with respect to the genesis of the NHS.

As a way of outlining an explanation for this strange situation, or pointing out a path for clarifying what Kuhn thinks about the genesis of the NHS, I call the reader's attention to another article by Kuhn which is considered to be less important (in that Kuhn chose not to include it in the 1977 collection of writings).[4] For our purposes, it is a strategic text, among other reasons, because it came after the others and allows some contact between Kuhn's two approaches. The text I refer to was published in 1970 as a review article in the journal *Encounter*, entitled: "Alexandre Koyré & the history of science – On an intellectual revolution".[5]

In all of Kuhn´s writings cited thus far, as well as others, he affirms the importance of Koyré for the NHS. However the text I refer to, which is the only one specifically about Koyré (with the exception of two brief reviews in the 1950s, Kuhn 1957 and Kuhn 1958), contributes to a clearer and more critical understanding of the role of Koyré in the NHS, from Kuhn's perspective, and from that, I believe, to a better understanding of Kuhn's conception regarding the genesis of the NHS.

I will thus organize the present paper as follows: in section 2, below, I present the endogenous approach of Kuhn to the emergence of the NHS (referring to internal causes or reasons) and the predominantly exogenous approach, or that in which there is, in any case, no reference to the reasons alluded to previously. In the third section, I present the 1970 article and distinguish the role of Koyré from that of Kuhn in the development of the NHS - in Kuhn's point of view. I complement the work with a brief reflection regarding the justification of the new historiography.

2. The nature of the historiographic change

2.1. An endogenous change

In well-known passages of Section I of *Structure*, Kuhn briefly describes the traditional historiography of science in these terms:

[4] It should be taken into account, however, that the text was not disqualified by Kuhn, as he does refer to it in the 1977 book (p. 150, note 15). Hanne Andersen also calls attention to this text in Andersen 2001.

[5] Review article by Koyré 1968. All six articles, among others, were published in French in Koyré 1973.

> 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. (…) And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation. Concerned with scientific development, the historian then appears to have two main tasks. On the one hand, he must determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented. On the other, he must 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. Much research has been directed to these ends, and some still is (Kuhn 1970a, pp. 1-2).

However, Kuhn emphasizes the need for a reaction to this practice, and suggests that some historians of science have already responded to it:

> In recent years, however, a few historians of science have been finding it more and more difficult to fulfill the functions that the concept of development-by-accumulation assigns to them. As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are simply the wrong sorts of questions to ask. (…) The same historical research that displays the difficulties in isolating individual inventions and discoveries gives ground for profound doubts about the cumulative process through which these individual contributions to science were thought to have been compounded (Kuhn 1970a, pp. 2-3).

Thus, for Kuhn, the result of this reaction was "a historiographic revolution in the study of science". According to him:

> Gradually, and often without entirely realizing they are doing so, historians of science 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 (Kuhn, 1970a, p. 3).

This endogenous explanation is complemented by "The historical structure of scientific discovery" and Section VI of *Structure*, from which Kuhn said he extracted the main ideas for the former. I give special attention to the article here because it was endorsed by Kuhn by republishing it in *Essential tension*. In it, Kuhn establishes a distinction between two types of discoveries, seeking to point out more precisely how difficulties in traditional historiography, to which he refers in Section I of *Structure*, emerge.

Kuhn saw no difficulty with the type of discovery, for example, of the neutrino, radio waves, or the missing elements in Mendeleiev's periodic table. According to him, the existence of these objects "had been predicted from theory before they were discovered, and the men who made the discoveries therefore knew from the start what to look for" (Kuhn 1977, p. 167). In such cases, therefore, the practice of the "old" traditional historiography would be perfectly admissible and feasible.

Many scientific discoveries, however, "particularly the most interesting and important", Kuhn stresses, are not of this type, and it would not be appropriate to ask when and where they occurred and who was responsible for them. Even if all the relevant information were available, he says, "those questions would not regularly possess answers". More complex discoveries of this type include oxygen, the electric current, X-rays, and the electron, which according to Kuhn "could not be predicted from accepted theory in advance and which therefore caught the assembled profession by surprise" (Kuhn 1977, p. 166). And further on he adds:

> there is no single moment or day which the historian, however complete his data, can identify as the point at which the discovery was made. Often, when several individuals are involved, it is even *impossible* unequivocally to identify any one of them as the discoverer (Kuhn 1977, p. 174, my emphasis).

A passage in *Structure* allows us to compare the two types of discoveries considered by Kuhn directly. He asks: "Why could not X-rays have been accepted as just one more form of a well-known class of natural phenomena? Why were they not, for example, received in the same way as the discovery of an additional chemical element?" His answer:

> New elements to fill empty places in the periodic table were still being sought and found in Roentgen's day. Their pursuit was a standard project for normal science, and success was an occasion only for congratulations, not for surprise.
>
> X-rays, however, were greeted not only with surprise but with shock. Lord Kelvin at first pronounced them an elaborate hoax. Others, though they could not doubt the evidence, were clearly staggered by it. Though X-rays were not prohibited by established theory, they violated deeply entrenched expectations (Kuhn 1970a, pp. 58-59).

The discovery of a new element in the periodic table, for example, corresponded to a "standard project for normal science". Whereas for the second, more complex type, like the discovery of x-rays, despite being an accidental discovery, it could, in principle, induce a subversion of normal scientific practice, in the same way the discovery of a chemical element with unexpected characteristics could lead to an alteration in the periodic table. As Kuhn wrote:

> Previously completed work on normal projects would now have to be done again because earlier scientists had failed to recognize and control a relevant variable. X-rays, to be sure, opened up a new field and thus added to the potential domain of normal science. But they also, and this is now the more important point, changed fields that had already existed. In the process they denied previously paradigmatic types of instrumentation their right to that title (Kuhn 1970a, p. 59).

And it is worth emphasizing, with Kuhn, to complete the comparison between the two types of discoveries, that

> discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that* something is and *what* it is (...) But if both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery, then discovery is a process and must take time. Only when all the relevant conceptual categories are prepared in advance, in which case the phenomenon would not be of a new sort, can discovering *that* and discovering *what* occur effortlessly, together, and in an instant (Kuhn 1970a, pp. 55-56).

Thus, according to this first perspective of Kuhn regarding the historiographical change, traditional historiography of science was in no condition to respond to the difficulties presented by the second type of discovery in its context, or to be practiced according to the cumulativistic proposals that defined it. It sought to respond to two distinct types of discoveries in the same way, as though there were only one type. The change to the NHS would therefore signify necessarily overcoming these difficulties or anomalies which emerged in the effective practice of the 'old historiography'.

2.2 An exogenous change

However, in the 1968 article "The history of science", republished in 1977 in *Essential tension*, Kuhn offers another explanation for the genesis of the NHS; a predominantly exogenous explanation in which there is no reference to previous writings or to causes and reasons evoked by them. He writes;

> Only in this century have historians of science gradually learned to see their subject matter as something different from a chronology of accumulating positive achievement in a technical specialty defined by hindsight. A number of factors contributed to this change (Kuhn 1977, p. 107).

And Kuhn quickly lists four factors:

The first factor, which he highlights as "probably the most important", was the influence of the history of philosophy which began at the end of the 19th Century. The attitude of "hypothetical sympathy", as he said, curiously referring to Russell (who is no model of historian of philosophy), or of methodological sympathy in relation to past thinkers, emerged in the history of science via philosophy. It was learned from men such as Lange, Cassirer, Burtt, and Lovejoy, "who dealt historically with people or ideas that were also important for scientific development", as well as "neo-Kantian epistemologists" like Brunschvicg and Meyerson. Kuhn does not cite Koyré here, although he can certainly be included in this tradition, having been an historian of philosophy before becoming an historian of science (Cf. Kuhn 1977, p. 107-108. Compare Kuhn 1970b, pp. 67-68, quoted below).

This influence of the history of philosophy, according to Kuhn, was reinforced by "another decisive event in the emergence of the contemporary profession": the recognition, with the work of Pierre Duhem, of the importance of the Middle Ages for the history of science, albeit almost a century later in relation to what occurred in general history. Despite the continuist positions that are often attributed to Duhem, as opposed to the discontinuist theory associated with the NHS, Kuhn believed Duhem had a positive influence in the advent of the historiographic revolution. It was a lesson learned from Duhem, according o Kuhn, that the science of the 17th Century could only be understood "if medieval science were explored first on its own terms and then as the base from which the 'new science' sprang". And in this way, "more than any other", emphasizes Kuhn, "that challenge has shaped the modern historiography of science", and the work of historians like Koyré became models of the new historiographical practice (Cf. Kuhn 1977, p. 108).

The third factor highlighted by Kuhn is "a repeated insistence that the student of scientific development concern himself with positive knowledge as a whole and that general histories of science replace histories of special sciences". According to him, the name of Auguste Comte was historically associated with this project, and recently and more effectively, the works of Tannery and Sarton. It was not a successful experience, Kuhn says, but "the attempt has been crucial, for it has highlighted the impossibility of attributing to the past the divisions of knowledge embodied in contemporary science curricula", revealing ruptures (Kuhn 1977, p. 109).

The fourth and final factor, which is more recent, is the so-called external history of science. This refers to "an increased concern, deriving partly from general history and partly from German sociology and Marxist historiography, with the role of nonintellectual, particularly institutional and socioeconomic, factors in scientific development". According to Kuhn, at the time he was writing, this influence had not yet become well-defined due to the resistance of followers and practitioners of the internal history, such as Koyré himself, who saw it as a threat to the objectivity of science. However Kuhn had already called attention to the fact that joining internal history and external history could be the greatest challenge facing the profession. And he noted, optimistically, that "there are increasing signs of a response" (Kuhn 1977, p. 109-110).

As can be seen from this brief presentation of the influential factors in the genesis of the NHS, which Kuhn himself does not expand on much, the explanation he provides in his 1968 text (and reiterates in 1977) is essentially an exogenous explanation. In fact, of the four sets of factors, three are external or have origins which are external to the traditional practice of the history of science. Only the third factor can be said to be endogenous, as Hoyningen-Huene appears to admit, without revealing, however, as we saw, the tension between the perspectives of Kuhn in *Structure* and *Essential tension* (Cf. Hoyningen-Huene 1993, p. 16).

It is worth remembering, moreover, that even the endogenous factor is not explicitly present in the text of *Structure*.[6] On the other hand, there is no mention in the 1968 text of the difficulties pointed out so emphatically by Kuhn in the previous approach, particularly with respect to the two types of discoveries. Nevertheless, this distinction is important for the description of the practice of science (in its normal and extraordinary phases), as well as for the characterization of intertheoretical incommensurability.

3. The new historiography of science: Koyré and Kuhn

Seeking to clarify these issues, I turn to the text in the journal *Encounter*. Published in 1970, "Alexandre Koyré & the History of Science: On an Intellectual Revolution" is a review of *Metaphysics and Measurement – Essays in the Scientific Revolution*, published by Koyré in 1968. Placing the text in context, Kuhn refers to what he considers to be an intellectual revolution that began in the 1940s – a revolution in the historiography of science still in progress in 1970.

[6] It is the theme of the article "Mathematical versus experimental traditions in the development of physical science", published in 1976 and re-edited in Kuhn 1977.

Kuhn identifies two stages in this transformation. The first, which he considered to be consolidated already at the time of his writing, was associated with those historians who saw the history of science as essentially the history of ideas (the so-called “internal history” mentioned above). The second stage, still being developed at that point in time according to Kuhn, referred to those historians of science whose model was social and cultural history (the so-called “external history”).

As a result, Kuhn writes, it is possible

> for the first time to see science as having a history, or at least one capable of interesting a contemporary historian. During the long years when scientific development was viewed as the routine result of applying "the scientific method," most history of science inevitably looked like mere chronology. Its concern – what else was there for its practitioners to do? – was to date and describe the emergence of the main components of objective method and to chronicle their triumph over superstition and error (Kuhn 1970b, p. 67).

However, more recent work in the history of science, said Kuhn, obeys a much more diverse model, in which the role of Alexandre Koyré stands out:

> Trained as a philosopher and historian of philosophy, Koyré’s transition to the history of science was marked by the publication in 1939 of his three brilliant *Études galiléennes*. Within a decade of their appearance, they and his subsequent work provided the models which historians of science increasingly aimed to emulate. More than any other single scholar, Koyré was responsible for the first stage of the historiographical revolution mentioned above. (…) Koyré showed how sympathetic and extended *explications de textes* could transform our image of the Scientific Revolution of the seventeenth century and of the men who made it (Kuhn 1970b, pp. 67-68).

Thus, it was Koyré, among others, who brought to the historiography of science the approach which Kuhn, in the preface to *Essential tension*, referred to as hermeneutic; and which, according to him, was a habitual part of the process of educating historians in other fields, with the significant exception of historians of science. [7]

The importance of Koyré for the NHS is affirmed by Kuhn in many of his writings, generally in passing. However, in the text we refer to in *Encounter*, unlike the others, there are also criticisms and reservations regarding Koyré, such as his disagreement with Koyré’s

[7] Having had access to Koyré shortly thereafter (Cf. Kuhn 2000, p. 285), Kuhn said that, “as a physicist”, he had to discover “the hermeneutic method” on his own. This is the personal experience he tells about in the preface to *Essential tension*, and various other places, of a re-interpretation of the physics of Aristotle. He felt the episode led to a decisive change in his view of science: “While discovering history, I had discovered my first scientific revolution” (Cf. Kuhn 1977, p. xiii).

statement that “good physics is made *a priori*” (p. 68). Kuhn questions 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” (Kuhn 1970b, p. 69).

But there are two more criticisms which I consider to be particularly relevant for my purposes here. Kuhn said that most traditional historians of science, knowing beforehand what constitutes scientific knowledge, felt authorized to select the works of those whom they had studied and to pick out the passages which they believed contained lasting contributions to science. “The discovery of such contributions was their ultimate goal” (Kuhn 1970b, p. 68).

And Kuhn compares:

> *Koyré's aim was very similar, occasionally too much so.* But for him the undertaking was far more problematic. To find out what, say, Galileo had contributed to the development of science, he had first to set Galileo in his own time, to discover what Galileo had taken science to be, what problems had seemed to him central, where his view of science and its problems had come from, and what alterations he had imposed upon that heritage. That task, Koyré felt, could not be done without immersion in an entire *corpus*, that of Galileo and those of his immediate predecessors, contemporaries, and successors (Kuhn 1970b, p. 68, my emphasis. Compare Kuhn 1970a, p. 3).

It can be said, then, that for Kuhn, Koyré is not yet fully a new historian of science. I think that, in the above passage, Kuhn affirms that Koyré has the same intentions and thus faces some of the same difficulties in practicing the ‘old historiography’ of science as the historians Kuhn refers to generically in *Structure* (in his strictly endogenous explanation). However, Koyré was able to begin to resolve these problems, perhaps due, essentially, to his background as a historian of philosophy (according to Kuhn’s exogenous explanation).

With respect to the second criticism of Koyré’s historiographical practice, Kuhn writes:

> I began by crediting Alexandre Koyré with a dominant role in the first stage of an historiographical revolution. Reading these essays should give substance to that attribution, but it will also illustrate how little place he left for the second stage. Partly because of his philosophical concern with ideas and partly because he dealt with men whose *work* was comparatively little affected by the new socio-economic climate of

> post-Renaissance Europe, he had little sympathy for those scholars who aimed to explain scientific development in social terms (Kuhn 1970b, p. 69).[8]

However, Kuhn emphasizes, Koyré knew there were problems in this regard. At the end of his life (he died in 1964), he spoke of how pleased he was with a book that seemed to "fill the hiatus between the history of science as such and social history", which until then "were miles apart" from each other (Kuhn 1970b, p. 69).

The book to which Koyré referred, Kuhn later recounted, was *Structure* itself. Very modestly, Kuhn omits this from the text published in *Encounter* in 1970 [9], but he reveals the fact informally in an interview in 1995, published in *The Road since Structure*:

> Another story out of sequence I don't want to forget: shortly before Alexandre Koyré died -- which is now a good many years later, he died shortly after *Structure* came out -- I had a last letter from him. (…) He said, "I've been reading your book," and I don't know what adjective he used, but it was a thoroughly agreeable one. He said, and again I had not seen this coming - when I thought about it, I thought he was right - he said, "you have brought the internal and external histories of science, which in the past have been very far apart, together." Now, I hadn't thought of that at all as what I was doing. I saw what he meant, and coming from him it was particularly agreeable because he had been so anti-external history; his gifts were as an analyst of ideas. And that made an impression, or at least it pleased me tremendously (Kuhn 2000, p. 286).

In this context, given the assessment of Koyré's role, we can also understand Kuhn's role in the NHS, from his own perspective. In the first place, he deliberately avoids posing certain questions and seeking certain answers, as traditional historians did, including Koyré. As he writes in *Structure*:

> Was it Priestley or Lavoisier, if either, who first discovered oxygen? In any case, when was oxygen discovered? In that form the question could be asked even if only one claimant had existed. As a ruling about priority and date, an answer does not at all

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[8] This does not mean that Koyré fails to attribute "a significant role in scientific development to extrascientific *ideas*". What Kuhn emphasizes is that, like other internalists, Koyré resisted giving attention to socioeconomic and institutional factors, unlike authors like Robert Merton, for example. This is what Kuhn said in a note, which was also important for an assessment he makes regarding the distinction between internal and external histories (Kuhn 1977, p. 32).

[9] Perhaps it is not only a question of modesty, but also of initial surprise or disagreement with respect to Koyré's observation. After all, in a text from 1979, Kuhn still writes of the intrinsic impossibility of an integration between internal and external history. In this text, he explicitly considers himself an internalist (Cf. Kuhn 1979, pp. 123 and 125). And he makes the same affirmation retrospectively about *Structure* in *The road since Structure*: "I thought of it as pretty straight internalist. It constantly surprises people in England that I'm an internalist. They cannot get their heads around it" (Kuhn 2000, p. 287). And perhaps an intermediary position can be seen in the 1968 text. As we saw, he writes there optimistically that "there are increasing signs of a response" regarding integration, although he also points out that "any survey of the field's present state must unfortunately still treat the two as virtually separate enterprises" (Kuhn 1977, pp. 109-110).

> concern us. Nevertheless, an attempt to produce one will illuminate the nature of discovery, because there is no answer of the kind that is sought. Discovery is not the sort of process about which the question is appropriately asked (Kuhn 1970a, p. 54).

In the second place, Kuhn admits, with Koyré, and finally acknowledges in the 1995 interview mentioned above, that his theory comes to fill the void between internal and external histories. It is not fitting here to specify what this fusion or this bridge would be, nor is it so important at this moment in time, when a clear distinction between the genesis and justification of knowledge is no longer prescribed.

I limit myself to remembering that, in an interview published in Borradori 1994 (p. 157), Kuhn goes so far as to say that he would perhaps classify *Structure* as a work in the sociology of knowledge, which certainly emphasizes the importance of studying scientific communities as producers and legitimaters of knowledge, with their psychological, sociological, and historical differences. For him, scientific knowledge "is intrinsically *a group* product" and "neither its peculiar efficacy nor the manner in which it develops will be understood without reference to the special nature of the groups that produce it". In this sense, says Kuhn his work "has been deeply sociological, but not in a way that permits that subject to be separated from epistemology (Kuhn 1977, p. xx).

By the way, Hoyningen-Huene said in a note that "Kuhn's *theory* can play a potentially important role in the integration of internal and external factors within *historiography*. For Kuhn's theory identifies a central point of contact between science and society: scientific values" (Hoyningen-Huene 2003, p. 19, note 70. See also pp. 147-154). And Philip Kitcher may summerize most effectively what is in question when he states that, for Kuhn, "justification is always justification in a particular historical context" (Callebaut 1993, p. 45).

4. Final considerations

I would like to conclude with a word about the strange, somewhat schizophrenic manner in which Kuhn deals with the genesis of the NHS through two sets of writings which are not integrated and which do not appear to acknowledge one another. My hypothesis is that the difficulty is related not to the genesis directly but to the justification of the NHS.

With respect to the genesis, the two explanations can be suitably integrated. As with the emergence of a new scientific theory, according to Kuhn, an internal anomaly would be the engine for change in the historiography of science, as well, complemented by external influences, such as, in this case, the historiography of philosophy. But the apparently natural transposition of Kuhn's model of science to historiography of science seems to present a problem. From the perspective of the justification, the two explanations appear to lead to very different results.

The justification of the NHS associated with the strictly endogenous explanation is much stronger than that which follows from the predominantly exogenous explanation. In the latter, the NHS would be merely an alternative to traditional historiography, and not the necessary way of practicing a truly historical historiography of science, as Kuhn would seem to demand. It happens that the proposal has an undesirable result, and could have led Kuhn to decrease his emphasis on the idea of endogenous change, as presented in *Structure*: in the case of science, the discarded theory is not a-scientific in itself, whereas in the case of the history of science, the traditional historiography of science, linear and cumulative, would be ahistorical . . .

Kuhn is unequivocal with respect to this in *Structure* as well as in other texts. In *Structure*, for example, he writes: "even from history, however, that new concept will not be forthcoming if historical data continue to be sought and scrutinized mainly to answer questions posed by *the unhistorical stereotype* drawn from science texts" (Kuhn 1970a, p.1, my emphasis). In fact, the ahistorical character extends beyond scientific textbooks. It includes, according to Kuhn, the texts which are disseminated and philosophical works based on the textbooks (Cf. Kuhn 1970a, p.136). "All three of these categories" are responsible for the problem Kuhn calls "the invisibility of revolutions", precisely because of their ahistorical approach. He addresses this issue in a separate chapter, from which I quote the following excerpt, which summarizes well what is of interest to us here:

> Textbooks thus begin by truncating the scientist's sense of his discipline's history and then proceed to supply a substitute for what they have eliminated. Characteristically, textbooks of science contain just a bit of history, either in an introductory chapter or, more often, in scattered references to the great heroes of an earlier age. From such references both students and professionals come to feel like participants in a long-standing historical tradition. Yet *the textbook-derived tradition in which scientists come to sense their participation is one that, in fact, never existed.* For reasons that are both obvious and highly functional, science textbooks (*and too many of the older histories of science*) refer only to that part of the work of past scientists that can easily be viewed as contributions to the statement and solution of the texts' paradigm problems. *Partly by selection and partly by distortion*, the scientists of earlier ages are implicitly represented as having worked upon *the same* set of fixed problems and in accordance with *the same* set of fixed canons that the most recent revolution in scientific theory

> and method has made seem scientific. No wonder that textbooks and the historical tradition they imply have to be rewritten after each scientific revolution. And no wonder that, as they are rewritten, science once again comes to seem largely cumulative (Kuhn 1970a, pp. 137-138, my emphasis).

One could add the word "ahistorical" to the end of this passage with no risk of rejection: upon being re-written, science comes to seem largely cumulative and ahistorical, the legitimate product of an ahistorical historiography. Elsewhere, Kuhn calls it *Whig* historiography (Cf. Butterfield 1931), as in this reference to Sarton in the 1995 interview:

> My notion was that there was a sort of history of science to do that Sarton wasn't doing. I mean, I would not have said then the sorts of things I would say now about him, and I recognize that in some very important sense he was a great man, but he certainly was a Whig historian (…) I could have learned a lot of data from Sarton but I wouldn't have learned any of the sorts of things I wanted to explore. (…) There were a number of other people who taught it within one or another of the science departments. But what they taught often *was not quite history in my terms, at least, not quite history*; *it was textbook history*. I have sometimes said that some of the greatest problems that I've had in my career are with scientists *who think they are interested in history* (Kuhn 2000, p. 282, my emphasis).[10]

One should not lose sight of a fact that John Preston calls attention to: that there are still those who defend the idea of a cumulative progress in the history of science, and mainly, that "[Kuhn's] accusation that cumulativists don't take the history of science seriously fails to register the interpretive latitude available when doing history of science. Neither continuity nor revolution is written on the face of science, and to suppose otherwise is to fail to take account of the fact (of which Kuhn was elsewhere well aware) that history is an interpretive (and therefore partly philosophical) discipline" (Preston 2008, pp. 19 and 54).

One of the "elsewhere's" Preston refers to is certainly the Section I of *Structure*. In the final pages, Kuhn writes about the genesis-justification distinction, understanding it as an

[10] Kuhn writes in *Encounter*, as we saw above, that with the emergence of the NHS , it is possible "*for the first time to see science as having a history*, or at least one capable of interesting a contemporary historian" (Kuhn 1970b, p. 67, my emphasis). He makes reference to the natural disinterest of the historian in an ahistorical discipline, as the history of science would have been prior to the NHS. The idea that the 'old historiography' of science was ahistorical is also present in the writings of other authors. See, for example, Kragh, who, echoing the Kuhnian critique, writes that Sarton's view "was, at least by modern standards, somewhat naive and surprisingly ahistorical". He also cites Rupert Hall who asks, "with all respect" regarding Sarton, "if he was ever a historian at all". And also Butterfield, who speaks of *Whig* historiography as "unhistorical history writing" (Kragh 1987, pp. 18, 93 and 198, note 43).

integral part of a particular conception of science. What he says there also gives substance to what we said earlier about his integration of internal and external histories (emphasized by Koyré):

> History, we too often say, is a purely descriptive discipline. The theses suggested above are, however, often interpretive and sometimes normative. Again, many of my generalizations are about the sociology or social psychology of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology. In the preceding paragraph I may even seem to have violated the very influential contemporary distinction between "the context of discovery" and "the context of justification" (…) Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge (…) Yet my attempts to apply them, even *grosso modo,* to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic. Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields (Kuhn 1970a, pp. 8-9).

Another pertinent passage is found in the text on the history of science published in 1979:

> What, then, has made fruitful interactions between history and philosophy of science so rare? Much of the answer is that they have, in fact, been infrequent only within the English-speaking or, more accurately, the logical-empiricist tradition. Most of the men, who provided internal historians with their primary models, were neo-Kantians of one sort or another. The philosophers who have recently found history of science relevant to philosophy are outside the logical empiricist tradition. What has seemed to separate history from philosophy of science is not, I think, intrinsic to either field but rather to a particular philosophical tradition, one largely confined in this century to the English-speaking world. No equivalent separation has been visible on the Continent (Kuhn 1979, p.125).

It is curious to observe Kuhn's difficulty in contrast with Koyré. Koyré seems to consider traditional, or *Whig*, historiography as merely another possible historiography, which for Kuhn, at least in the case of science, signifies an ahistorical activity, which is not even possible to practice effectively. Koyré writes:

> The disdain to which the eighteenth century has been subjected can only be explained by the fact that it was vanquished. It is the victors that write history, and it is the representatives of this victory, representatives, in particular, of the Romantic reaction, and especially of the German Romantic reaction, who have largely determined our historical judgments and our very conception of history. They are also the men who have convinced us that the eighteenth century disregarded these ideas of ours.
>
> Nothing seems more untrue to me than this assertion. It seems to me indefensible, unless we accept the Romantic conception of history. If, on the contrary, we do not share this idea, we should find that it is to the eighteenth century that we owe the discovery, or rather the rediscovery, of history (…) Quite true, the men of the eighteenth century did not have the regard, the respect and the reverence for history that the Romantics had. Nor is there any doubt that they did not have the religion of scholarship, and that they often disregarded the details (and even more than the details) of the past. They felt no nostalgia for the past - like the Romantics. On the contrary, they were concerned primarily with the future (Koyré 1948, pp. 132-133).

It is worth saying that these exemplarily tolerant words of Koyré come precisely from a text about Condorcet, an author who, for Kuhn, is one of the historic champions of the idea of cumulative progress . . . And I am reminded that Kuhn was once called a romantic . . . But that is another and longer story. [11]

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[11] See Kuhn 1977, pp.148 and 106-107; and Bloor 1991, p. 62. I try to address these questions in a forthcoming paper: *Kuhn and the justification of the "new historiography"*.