

Carnap, Kuhn , and the history of science

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Abstract

The purpose of this article is to respond to Thomas Uebel's criticisms of my comments regarding the current revisionism of Carnap's work and its relations to Kuhn. I begin by pointing out some misunderstandings in the interpretation of my article. I then discuss some aspects related to Carnap's view of the history of science. First, I emphasize that it was not due to a supposed affinity between Kuhn's conceptions and those of logical positivism that Kuhn was invited to write the monograph on the history of science for the *Encyclopedia*. Three other authors had been invited first, including George Sarton whose conception was entirely different from Kuhn's. In addition, I try to show that Carnap attributes little importance to history of science. He seldom refers to it and, when he does, he clearly defends (like Sarton) a Whig or an "old" historiography of science, to which Kuhn opposes his "new historiography of science". It is argued that this raises serious difficulties for those, like Uebel, who hold the view that Carnap includes the historical or the social within the rational.

Keywords: Carnap, History of science, Kuhn, Logical positivism, New historiography of science, Philosophy of science, Revisionism, Sarton

1. Introduction

In his article "Carnap and Kuhn: On the Relation between the Logic of Science and the History of Science", published in 2011 in the *Journal for General Philosophy of Science*, Thomas Uebel responds to my criticism of what I call the revisionism of the logical positivist work, particularly its thesis regarding the compatibility or intimate relation between the philosophies of science of Carnap and Kuhn.

My article, which appeared in 2007 in the same Journal, appealed for a broadening of the discussion on the subject, and I am grateful to Professor Uebel for re-opening the case (considered closed by many), which gives me the opportunity to revisit and attempt to clarify my arguments in this new paper.

Uebel's article begins with the observation that my criticism "is of considerable interest". The reason he provides is that it "would raise a deep problem for the compatibility of Carnapian logic of science and Kuhnian philosophy-cum-history of science". But he adds a caveat: "if it were correct" (p. 129). And Uebel then sets out to demonstrate that this is not the case.

Toward this goal, he seeks to present a brief summary of my article in section 1. I thus begin my response with some preliminary questions regarding his interpretation of my text. According to the revisionists, Uebel writes,

the publication of Kuhn's *Structure of Scientific Revolutions* as a volume of the *International Encyclopedia of Unified Science* in 1962 did not amount to the placement of a "Trojan Horse" (Pinto de Oliveira 2007, 148 and 155) at the heart of the logical empiricist's unified science movement that it has long been taken for. Instead, that publication reflected a much deeper though widely overlooked agreement about how to think about science. The argument to this effect proceeds along *two routes which Pinto de Oliveira then criticises*. The first route builds on the two letters by Carnap to Kuhn (of 12 April 1960 and 28 April 1962) in which Carnap expressed his approval of Kuhn's project and of its realisation, first published and commented upon by George Reisch (1991). The second route employs the comparison of the ideas expressed by Kuhn and the philosophical tenets of the mature Carnap in order to unearth "deep affinities" (Friedman 2003, 20). (Uebel 2011, 130, my italics).

And Uebel adds:

According to Pinto de Oliveira, *neither argument is convincing*. The first route fails because the letters are "far too brief" (2007, 149) to support the interpretive weight put on them. And the second route fails to establish with sufficient depth "the compatibility between the two authors' respective philosophies of science" (ibid, my italics).

I was surprised by this interpretation of my writing. To begin with, the problem regarding the letters is not that they are too brief, but rather that they are less important than Carnap's work itself, although they should be considered historical documents. And in his work, Carnap never cites Kuhn. Nevertheless, the revisionists take the letters

into account (which are favorable to the revisionist interpretation) but not the fact that Carnap neglects to mention Kuhn anywhere in his work (which does not favor the revisionist interpretation). In addition, after identifying the two lines of argument used by the revisionists, as summarized above by Uebel, I was very explicit in saying that my article aimed to criticize only the first. In other words, all my criticisms are aimed *only at the revisionists' first line of argument*, as I stated in the last paragraph of the Introduction of my article (p. 149).

This is not to say that some of my criticisms (such as the difference between Carnap and Kuhn regarding the discovery-justification distinction, and the fact that Carnap never cites Kuhn in his work) are not relevant to the revisionists' second line of argument, as well. However, this misinterpretation of the purposes of my article unfortunately leads Uebel to have unrealistic expectations of me (because related to the revisionists' second argument). He says that

Reisch, Earman, Irzik and Grunberg, and Friedman all agree that Carnap and Kuhn share (1) similar ideas concerning the dependence of scientific knowledge claims on paradigms or lexical structures and logico-linguistic frameworks; (2) the view that radical theory change involves a change of language; (3) a conception of (possible) incommensurability as nontranslatability between scientific theories and, with it, the rejection of the idea that all facts are theory-neutral. Beyond that Earman and Irzik and Grunberg disagree on (4) whether Carnap and Kuhn share a holistic conception of meaning, and Irzik and Grunberg and Friedman seem to disagree on (5) whether Kuhn himself was right in seeing a major difference with Carnap in his own view that language change is "cognitively significant" (1993, 314). *Pinto de Oliveira offers no discussion of these theses whatsoever* (Uebel 2011, 131, my emphasis).

It is my understanding that Uebel identified the discovery-justification distinction as being the central theme of my article, as this question is also related to the revisionists' second argument. However, this leads him to neglect my criticism of the historical work (or historiography) of the revisionists. I criticize how they address the historical question of the publication of *Structure* in the *Encyclopedia* (and the letters). The excessive importance they attribute to the letters is only one of my criticisms, along with the fact that they fail to take into account the absence of any reference to Kuhn in Carnap's work.

Added to this is the circumstance, also not taken into consideration in Reisch's text (subsequently endorsed by other revisionists), that the publication of *Structure* resulted from a request made to Kuhn to write a monograph in *the history of science* for the *Encyclopedia*; and that this request was not made originally to Kuhn but had been made first to other historians who had declined. When Reisch affirms that Kuhn's work was carried out in response to an invitation from the positivists (1991, 265), but fails to point out these historical circumstances, he inadvertently suggests that the positivists' invitation to Kuhn was to produce a text in *the philosophy of science*, and so would be almost trivial to conclude that there is an affinity between *the philosophies of science* of positivism and that of Kuhn.

It is in the context of these criticisms that I point to the revisionists' neglect of the discovery-justification distinction, which Uebel recognizes "was not addressed in any of the discussions of the Carnap-Kuhn relation (...) criticised by Pinto de Oliveira" (p. 131). The distinction appears to be a fundamental element of the historical context in which the publication of *Structure* in the *Encyclopedia* occurred. As I sought to show in my article, the positivists asked Kuhn to produce a work in the *history of science* and published it as such. This fact reflects a difference between positivism and Kuhn, since Kuhn certainly did not see *Structure* as a *history of science* work (nor do we). It was a curious difference and important to explain, for which I turned to the discovery-justification distinction, which was generally accepted by the positivists and denied by Kuhn.¹

This idea seemed promising to me because, in addition to shedding light on the difference between positivists and Kuhn with respect to the nature of *Structure*, it contributed to understanding about another important aspect of the historical process: the (slow) change in the meaning of "philosophy of science", which to this day does not appear to be entirely consolidated.² I should add that a change in the meaning of "history of science" was also underway, with the shift from the old to the "new historiography of science", a question we will return to in the sections that follow.

Because Uebel does not situate my reference to the discovery-justification distinction within the broader context of my criticism of the historical work of the

¹ See Hoyningen-Huene 2006, 124-128, and Pinto de Oliveira 2012, 120-121.

² As is evident in the resistance one still observes to recognizing Kuhn as a philosopher of science. See, for example, the quote of Agassi in my 2007 article (p. 152, note 4).

revisionists regarding the publication of *Structure*, I believe he has not fully evaluated it. He goes on to ask in his article, “How convincing is Pinto de Oliveira’s criticism?” and responds:

Once his caveat is noted one may of course grant his (now limited) claim (that the letters alone prove very little) and turn to the analyses given by Reisch, Earman, Irzik and Grunberg, and Friedman to argue that they have shown, on independent grounds, that there do obtain deep affinities between (selected) aspects of Carnap’s and Kuhn’s philosophies of science. Carnap’s letters are but the icing on the cake (p. 131).

This supposedly limited reach of my article, to merely restricting the importance attributed to the letters taken in isolation (the only point highlighted in Uebel’s abstract), I have already denied above. In truth, as I have tried to show, I contest Reisch’s interpretation (endorsed by the other revisionists) of the letters, and the publication of *Structure* in the *Encyclopedia* itself, in a broader sense, and I offer an alternative interpretation that avoids these criticisms.

As I indicated in the first article, Michael Friedman considers two types of arguments in favor of the revisionist thesis. With respect to the first argument, as I highlighted in my article, he writes:

These expressions of approval [in the letters] by Rudolf Carnap – the generally acknowledged leading representative of logical empiricism – are certainly striking, and they must give serious pause to expositors of the conventional wisdom (Friedman 2003, 20).

Thus, despite my interest in it as an historical episode, it is Michael Friedman who attributes undue importance to what, according to Uebel, is merely “the icing on the cake”. But I believe it is necessary to note that Friedman captures a relevant historical aspect associated with this argument. It is considerably simpler and more immediate than the broad comparison of the works of Carnap and Kuhn required in the second argument of the revisionists, and has therefore, in practice, been equally effective in winning people over to the revisionist thesis. It was for this reason that I cited, at the end of my earlier article, texts written by some authors (Salmon and Andersen) that present only this argument to justify or explain their endorsement of the revisionist thesis regarding the relation between Carnap and Kuhn. And it was for the same reason that I also wrote:

In short, regardless of whether the general revisionists' thesis about the close relationship "between Kuhn's theory of scientific revolutions and logical empiricist philosophy of science" is true or not, it seems to me that they are mistaken in supporting it (partly) on the fact that *Structure* was published in *Encyclopedia* (argument 1). If this is not the only or the best argument in favor of the revisionist thesis, it is certainly the most emblematic and deserves further discussion (p. 155).

Uebel's article provides the opportunity to re-open this discussion. As his conclusion, he writes:

Despite his own concentration on the formalist logic of science, Carnap recognised the legitimacy and usefulness of the empirical sciences of science and the incompleteness of a philosophy of science that only concentrates on the former. Whatever else the differences between Carnap and Kuhn may be, the recognition of the deep significance of history for a philosophical understanding of science is not what divides them (p. 139).

Aiming to participate in the discussion, I present some arguments below which I believe can clarify and reinforce the questions raised in my previous article. They refer particularly to the history of science and its historiography, and this time directly address the revisionists' second line of argumentation, as well. First, I emphasize that it was not due to a supposed affinity between Kuhn's conceptions and those of logical positivism that Kuhn was invited to write the monograph on the history of science for the *Encyclopedia*. Three other authors had been invited first, including George Sarton whose conception was entirely different from Kuhn's. Furthermore, I try to show that Carnap attributes little importance to history of science. He seldom refers to it and, when he does, he clearly defends (like Sarton) a Whig or the "old" historiography of science, to which Kuhn opposes his "new historiography of science". In the final section, it is argued that this raises serious difficulties for those, like Uebel, who hold the view that Carnap includes the historical or the social within the rational.

2. Enriquez, Sarton, Cohen, and Kuhn

As mentioned above, I sought in my 2007 article to offer a more nuanced alternative to the interpretation of Reisch and other revisionists regarding the

publication of *Structure* in the *Encyclopedia*. I tried to show, among other things, that a place for a work in the history of science had already been reserved in the *Encyclopedia* since the 1930s, and I cited various passages containing references to invitations made by the positivists in this regard to historians like Enriques, Bernard Cohen, and Kuhn.

But the list of authors invited by logical positivism to write the monograph of the history of science was not complete. One name need to be added to the list. Paul Galison revealed that another author who was considered for publication of a work on the history of science in the *Encyclopedia* was George Sarton. Galison says:

History does take unexpected turns; here, I will argue, it did not. On many grounds the Kuhnian antipositivism is only awkwardly extricable from positivism, especially Carnap's, from which it emerged. But let us step back to understand in somewhat more detail the links between the Unity of Science movement and the history of science.

Already in 1936, Charles Morris, the most active advocate of the Vienna Circle in America and an editor (with Carnap and Neurath) of the *Encyclopedia*, was keen to get George Sarton (one of the earliest and most vigorous boosters of the history of science) on the permanent committee of the movement. (...) To Morris, Sarton offered the possibility of grounding the Unity of Science movement with a sympathetic history of science. It was an eminently sensible choice: Sarton was a positivist (though more Comtean than logical). But in the end, Sarton declined for lack of time, disappointing Morris, who very much wanted the history of science in the second volume of the *Encyclopedia*: "you would have seen the whole larger significance of the history of science for a comprehensive science of science program." In his place, Sarton offered the services of his assistant, I. Bernard Cohen. Cohen, too, eventually withdrew, and it was then that the task fell to a third Harvard historian of science, Thomas Kuhn. (Galison 1995, 29-30)

Thus Galison himself assumes a revisionist position, and appears to have done so even before the publication of Reisch's article.³ Galison does not refer to Enriques, however, but Reisch says in his 2005 book that the invitation to write the monograph was passed from the Italian historian Federigo Enriques to Sarton, then to Bernard Cohen, and finally to Kuhn (Reisch 2005, 9).

³ Galison writes in a note: "Since the time that this paper was presented (March 1990), a fine article by Reisch has appeared that also discusses the Kuhn-Carnap interaction" (p 29, note 9). And he mentions Reisch 1991.

And Kuhn himself mentions Sarton in an interview conducted by John Horgan in 1991. Kuhn quipped: “Before I was doing the history volume, Federigo Enriques was doing it then, and George Sarton was doing it. Neither of them survived. I barely survived but I did it”.⁴

The list of invited authors thus appears to be: Enriques, Sarton, Cohen, and Kuhn. But it is not my purpose here to claim that the list is now complete. What I would like to do, essentially, is show that this new or ‘lost’ link, the historian George Sarton, is particularly significant for the historical analysis outlined in my earlier article.

Sarton was a prominent name in the institutionalization of the history of science as a discipline beginning in the 1920s. But he also championed the idea of cumulative progress and Whig historiography. Kuhn says about him in an unequivocally unfavorable manner:

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 and he certainly saw science as the greatest human achievement and the model for everything else. And it wasn't that I thought that it was *not* a great human achievement, but I saw it as one among several. 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 (Kuhn 2000, 282).

In *Essential Tension*, Kuhn cites again Sarton as a reference among defenders of the Whig history or the idea of cumulative progress in science, which *Structure* directly argues against. He speaks of “an almost continuous tradition from Condorcet and Comte to Dampier and Sarton” which “viewed scientific advance as the triumph of reason over primitive superstition, the unique example of humanity operating in its highest mode” (Kuhn 1977, 148).

It is not my intent to argue here that traditional historiography of science is, for Kuhn, unhistorical (Cf. Pinto de Oliveira 2012, section 4). However, the discrepancy

⁴ The interview (February 1991) served as a basis for a book by Horgan, although the passage was not published. The complete interview was found online at: <http://www.stevens.edu/csw/cgi-bin/shapers/kuhn/>. Accessed in January, 2009.

between the historiographies of Kuhn and Sarton (and even their antipodal character) appears to illustrate an essential aspect only hinted at in my previous article. I believe that the fact that Sarton as well as Kuhn were invited (one after the other) to write the monograph on the history of science for the *Encyclopedia* shows that the positivist editors were unconcerned about the nature of the historiography or the philosophical aspect underlying the work. They did not take into account the large difference between the authors, which ranges considerably from the explicit cumulativism of Sarton to the explicit anti-cumulativism of Kuhn; from the old historiography of science to the new.⁵

Unless revisionists aim to argue that the substitutions of the author throughout the process (which lasted from the 1930s until its publication in 1962, and passed through a list of names including Enriques, Sarton, Cohen, and Kuhn) precisely describe, through the different conceptions of the history of science, the evolutionary path of the logical positivist philosophy of science... Obviously this would make no sense, given the fact that the reasons were circumstantial and the changes did not originate from the positivist editors themselves.⁶

Nevertheless, it should be noted that, curiously, Galison and Richardson seek to mark the possible similarities among the historians invited to write the work about the history of science in the *Encyclopedia*. Galison says that, after Sarton and Cohen declined, “the task fell to *a third Harvard historian of science, Thomas Kuhn*” (Galison 1995, 30, my italics). Richardson, in turn, writes:

...in the 1950s, the relations between Kuhn and his fellow Harvard historians of science and the logical empiricists and their students are instances of cross-fertilization and, at times, collaboration; certainly the logical empiricists seemed to think that the new historians who worked with James B. Conant were giving accounts of the history of science *that illustrated their philosophical points* (Richardson 2007, 354, my italics).

⁵ See, for instance, Sarton 1937, especially chap.1. For a comparative analysis, see my forthcoming paper (with Amelia Oliveira) “Kuhn, Sarton, and the History of Science”.

⁶ And Kuhn was invited to write the monograph of the history of science nearly ten years before its publication, when he had only written a few articles specific to the subject, some book reviews, and had not yet published his first book (*The Copernican revolution*, 1957). Regarding this, see Kuhn 2000, 291-292, and the short list of his publications up until the beginning of the 1950s on pp. 325-326. Regardless, it seems probable that the invitation to Kuhn would have resulted from his work as a professor in the History of Science course for non-scientists directed by Conant. This already guaranteed his distinction, reinforced by Cohen’s recommendation, who had in turn been recommended by Sarton. See Merton 1979, 71-125.

Both Galison and Richardson refer to Conant as a point of unity or convergence among the other “Harvard historians”. However, it is well to remember here, as I did in a note in Pinto de Oliveira 2012, that

Kuhn acknowledges in *Structure* that it was James B. Conant “who first introduced me to the history of science and thus initiated the transformation in my conception of the nature of scientific advance” (Kuhn, 1970a, p. xi). Furthermore, the first edition of the book is dedicated to Conant, “who started it”. Nevertheless, Kuhn in all his work does not refer to Conant as a significant influence in the emergence of the ideas that characterize the NHS [new historiography of science] (note 6, p. 118).

As he did with Sarton, Kuhn also emphasizes his differences in relation to another Harvard historian of science who received an invitation from the positivists, Bernard Cohen. Kuhn says: “Bernard has done a lot of good for the history of science but he is not someone who thinks about development at all in the way I do. We've not seen eye to eye (Kuhn 2000, 283).

To conclude these crossed citations, Cohen himself places Conant alongside Sarton as a supporter of the idea of cumulative progress:

Sarton’s analysis led him to conceive that a primary aspect of science was its cumulative character; in fact, he declared (1936, 5), science is the only “truly cumulative and progressive” activity of mankind – a judgment in which J. B. Conant (1947, 20) and others have concurred (Cohen 1985, 22).⁷

In the section that follows, I seek to show that Carnap attributes very little importance to history of science. As a result, his manifestations on the subject are few. But when he does, as will be argued, it can be shown that he defends an historiography incompatible with that of Kuhn, and that it is in fact a characteristic example of the “old historiography” that Kuhn directly criticizes in *Structure* and elsewhere.

3. Carnap and the history of science

In a paper published in 1974, Bernard Cohen describes a picturesque episode about Carnap in relation to the history of science:

⁷ Cohen cites Sarton 1936, 5, and Conant 1947, 20. Preston also includes Conant among the advocates of cumulative progress (Cf. Preston 2008, 53).

Some thirty years ago, when Carnap was spending a term in Cambridge, Massachusetts, I gathered up my courage and asked him whether he would speak to our graduate-student history of science club. He was polite enough not to refuse at once; before giving me his answer, he asked me to walk home with him and to tell him why I found the history of science interesting and worthy of study. We spent a good bit of the afternoon together; in the end, he thanked me, and remarked that he felt he could not speak to our group. I had convinced him, he said, that he really was as unhistorically minded a person as one could imagine. He had nothing whatever to say, he concluded, about the study of the history of scientific ideas that could possibly be of interest to historians! (p. 310, note 10).

This would have been an excellent opportunity for Carnap to explain the question of the division of labor and to cite, despite his own “specialization” in the logic of science, the important role that the logical positivism movement reserved for the history of science, if this were in fact his position. After all, at the beginning of the 1940s, he had already published at least a text (Carnap 1938) cited by Uebel in favor of the bipartite interpretation (see below). And Cohen and his colleagues would have been very pleased and proud to learn that Carnap, despite not having written on the subject himself, attributed considerable importance to the history of science in his philosophy of science, to the point that he recognized “the deep significance of history for a philosophical understanding of science” (Uebel 2011, 139) or included the historical element in the idea of rationality.⁸ Indeed, Cohen’s history of science club would have been the ideal audience for such a revelation by Carnap...

Consistent with the distance and lack of interest that he revealed to Cohen, Carnap rarely refers to the history of science. However, in his intellectual autobiography, published in 1963 in the volume edited by Schilpp, he explicitly mentions the history of science and relates it to his view of the history of philosophy. He refers directly to the Department of Philosophy at the University of Chicago, where he worked for many years and where great emphasis was given to the history of philosophy. He writes:

The methodological attitude toward the history of philosophy which the students learned was characterized by a thorough study of the sources and by emphasis on the requirement that the doctrine of a philosopher must be understood immanently, that is, from his own point of view, inasmuch as a criticism from outside would not do justice

⁸ Uebel says that Carnap “does not exclude the social from the rational” (2011, 134).

to the peculiarity of the philosopher in question and his place in the historical development. This education in historical carefulness and a neutral attitude seemed to me useful and proper for the purpose of historical studies, but not sufficient for training in philosophy itself. *The task of the history of philosophy is not essentially different from that of the history of science. The historian of science gives not only a description of the scientific theories, but also a critical judgment of them from the point of view of our present scientific knowledge. I think the same should be required in the history of philosophy. This view is based on the conviction that in philosophy, no less than in science, there is the possibility of cumulative insight and therefore of progress in knowledge. This view, of course, would be rejected by historicism in its pure form* (Carnap 1963, 41, my italics).

In this excerpt, Carnap refers not only to the history of science but to its historiography, as well. He highlights an aspect that he considers important in the historiography of science as well as in the historiography of philosophy, which is the critical judgment of past theories from the point of view of current knowledge. He sees this as being characteristically present in works on the history of science and absent in the works on the history of philosophy. This is why he uses the historiography of science as a model for the historiography of philosophy, proposing that the historian of philosophy critically judge past theories of philosophy from the point of view of contemporary philosophy, just as the historian of science judges, in what Carnap believes to be an exemplary manner, the scientific theories of the past.

It is worth adding that this Carnap's attitude regarding the history of science and the history of philosophy is not an isolated attitude. It is fully shared, for example, by Reichenbach. Reichenbach manifests similar thoughts in a more explicit (and radical) way in various texts on the history of philosophy and of science, particularly in *The Rise of Scientific Philosophy* (1951). In the preface to the work, Reichenbach writes:

...this book may be used as an introduction to philosophy, and in particular, to scientific philosophy. Yet it is not intended to give a so-called "objective" presentation of traditional philosophical material. *No attempt is made to expound philosophical systems with the attitude of the interpreter, who wishes to find some truth in every philosophy and hopes to make his readers believe that every philosophical doctrine can be understood. This way of teaching philosophy is none too successful. (...) If a presentation of philosophy is to be objective, it should therefore be objective in the standards of its critique* rather than in the sense of a philosophic relativism. The

investigations of this book are intended to be objective in this sense (Reichenbach 1951, viii-ix, my italics).

This idea of not intending, in an historical approach to philosophy, to explain philosophical systems with “the attitude of the interpreter” he supports based on the fact that, according to him, there exists a “philosophical truth”. And this truth should not be confused with “philosophical opinions” which the interpreter uncritically seeks to present as a “collection of truths”, as though there were a truth relative to each philosophy, and each philosophy could be pointed to as a different version of wisdom. Unlike the interpreter, understood in this sense, the “competent historian” for Reichenbach, as for Carnap, should be critical. Reichenbach writes:

I do not wish to belittle the history of philosophy; but one should always remember that it is history, and not philosophy. Like all historical research, it should be done with scientific methods and psychological and sociological explanations. But the history of philosophy must not be presented as a collection of truths. There is more error than truth in traditional philosophy; therefore, only the critically minded can be competent historians. The glorification of the philosophies of the past, the presentation of the various systems as so many versions of wisdom, each in its own right, has undermined the philosophic potency of the present generation. It has induced the student to adopt a philosophic relativism, to believe that there are only philosophical opinions, but that there is no philosophical truth (Reichenbach 1951, 325).

The history of philosophy is identified by Reichenbach as a “philosophical museum” (Cf. Reichenbach 1951, 123-124), in which philosophy is dead, while the philosophy of the “live present” is practiced in an unhistorical manner, disconnected from the past. Or it is practiced historically, but only to the extent to which “the creators”, or those who work in the field of philosophy “productively”, in the present as in the past, do not care about the opinions of their predecessors (Cf. Reichenbach 1931, 84). And Reichenbach writes in the same place, accentuating the importance of the contemporary perspective:

Compared to earlier times, the situation has changed so completely that the ancients can no longer be fruitful for us. (...) Let those for whom, as Kant says, 'history of philosophy is philosophy itself' spend their time in studying the writings of the great philosophers of the past. We would rather emulate these great men in another respect; all of them were ahistorical thinkers, and did not care about the opinions of their predecessors. (...) Our modern task can only be performed without consideration of

tradition, in close contact with the problems that scientific discoveries pose for the philosopher. (...) For the creators, logical connections are important; historical connections will be established as a matter of course (Reichenbach 1931, 84. See also Reichenbach 1951, 325, excerpt cited by Mormann and Ibarra 2010, 93-94).

The superiority of the contemporary moment, because of the peculiarity of its new problems or the theoretical (or technical) superiority for approaching old problems, is what would justify the critical point of view, “an objective” criticism of the theories of the past. For Reichenbach, as for Carnap, what can be verified regarding the history of science is what should count as well for the history of philosophy. Reichenbach writes in 1948:

Everyone who has taught the history of philosophy, whether he did it with more or less enthusiasm, is familiar with the feeling of dissatisfaction with which he often went home from his classes. (...) Why should we teach it, if there is no outcome, no recognized truth? (...) The philosopher of the twentieth century should have enough intellectual distance from the constructions of his predecessors to be capable of an objective critique, and he should have the courage to say what is wrong with philosophy since it is evident that philosophy has been unable to develop a common doctrine that could be taught to students with the general consent of all those who teach philosophy (Reichenbach 1948, 135-136).

And he continues:

Those among us who have taught one of the sciences will know what it means to teach on a common ground. The sciences have developed a general body of knowledge, carried by universal recognition, and he who teaches a science does so with the proud feeling of introducing his students into a realm of well-established truth. Why must the philosopher renounce the teaching of established truth? Why must he qualify all his teachings by the clause 'according to the view of philosopher X' and restrict his objectivity to the statement of what was the view of philosopher X? (...) Imagine a scientist who were to teach electronics in the form of a report on views of different physicists, never telling his students what *are* the laws governing electrons. The idea appears ridiculous. Though the physicist does mention the history of his field of study, the views of individual physicists appear as contributions to a common result established with a superpersonal validity and universally accepted. Why must the philosopher forgo a generally accepted philosophy? (Reichenbach 1948, 136).

It should be noted that this 1948 article by Reichenbach (like the 1931 article also cited above) was reprinted in *Modern Philosophy of Science*. The book was

published in 1959 and includes a preface written by Carnap where he refers to Reichenbach as someone who shares his own views. Carnap identifies scientific philosophy with logical empiricism and describes them like a logical analysis. He writes:

Hans Reichenbach (1891-1953) was one of the founders of the movement of scientific philosophy or logical empiricism, and one of its most vigorous and most productive representatives. In Essay IV [1931] of this book, Reichenbach has clearly outlined the aims and methods of the new way of philosophy which is characterized by its close relationship to scientific work. (...) It strives to reveal the main features of the scientific method by a logical analysis of the hypotheses, observations, and conventions which enter into the construction of a scientific theory (Reichenbach 1959, vii).

And Carnap continues:

The papers collected here illustrate well the method of philosophy just characterized. (...) The first papers were published in German in the period from 1921 to 1932; they let us feel the élan and the optimistic anticipations of a pioneer in the newly opened field. Reichenbach subsequently developed the conceptions formulated here and presented them in a more definitive form in later publications [*The rise of scientific philosophy (1951)*] (Idem, vii-viii).

And it is worth to emphasize that in 1951 Reichenbach writes that “scientific philosophy attempts to get away from historicism and to arrive by logical analysis at conclusions as precise, as elaborate, and as reliable as the results of the science of our time” (Reichenbach 1951, 325).

To situate the position of Reichenbach and Carnap in relation to the history of science (and the history of philosophy), one needs only to compare it precisely to the perspective of a philosopher of science like Kuhn. In addition to conferring a privileged role to history in the investigation of science, Kuhn is himself an historian. I believe the question should be put plainly: Are Reichenbach and Carnap not defending the theses of the “old historiography” of science, against which Kuhn contrasts his “new historiography” at the very beginning of *Structure*?

In texts that are widely known, Kuhn describes the traditional conception which values precisely the critical perspective regarding the past from a contemporary point of view. I cite only one of them here:

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

According to Kuhn, it is precisely in opposition to this way of conceptualizing science and its history, and to the work guided by it, that an “historiographic revolution” emerges:

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 1970, 3, my emphasis).

Concerning the history of philosophy, Kuhn, who is an historian of science, is understandably less explicit. Nevertheless, there is no shortage of texts on which he expresses himself with clarity in this regard. In “The Relations between History and the History of Science”, he writes:

The history of philosophy, as taught within philosophy departments, is often, for example, a parody of the historical. Reading a work of the past, the philosopher regularly seeks the author's positions on current problems, criticizes them with the aid of current apparatus, and interprets his text to maximize its coherence with modern doctrine. In that process the historic original is often lost (Kuhn 1977, 153).

He also commented informally, in the interview published at the end of his last book, *The Road since Structure*:

...I tried to talk a little bit about my experience of having philosophers and historians and scientists in the same classroom. The philosophers and the scientists are much

closer to one another, because they all come in being concerned about what's right and wrong -- not about what happened -- and therefore tending to look at a text and simply pick out the true and the false from a modern point of view, from what they already know (Kuhn 2000, 315).

To conclude this section, we could say that both Reichenbach and Carnap propose and defend the “old historiography” of science; an historiography of science which criticizes past theories from the vantage point of the contemporary scientific perspective. And the same should be true then for the history of philosophy. In this sense, according to Carnap, as we saw, the historian of philosophy should imitate the example of the historian of science who, in addition to describing scientific theories, is concerned with presenting a “critical judgment of them from the point of view of our present scientific knowledge” (as George Sarton did). This would make it possible to speak, in the case of philosophy as well, of “cumulative insight” and “progress in knowledge” (Cf. Carnap 1963, 41).

4. Conclusion

I believe I can conclude by maintaining that the above arguments reveal serious difficulties in the interpretation presented by Uebel in his 2011 article and which he refers to as “bipartite metatheory” (p. 136. See also Uebel 2010). As Mormann and Ibarra (2010, 74) summarize:

Recently Thomas Uebel dubbed Carnap’s envisaged bipartite approach, which, according to him, was also favoured by Frank and Neurath, the “bipartite metatheory” of the Vienna Circle’s logical empiricism. The bipartite metatheory was designed to be the successor discipline of traditional metaphysically contaminated epistemology and philosophy of science. This “bipartite metatheory” was said to have two components:

- (1) a logical component in the sense of a Carnapian logic of science;
- (2) an empirical part roughly in the sense of Neurath’s “behavioristics of scholars” or Frank’s “pragmatics of science”.

Within this framework, Mormann and Ibarra (2010, 76) imagine that Carnap

might have hoped to use Kuhn's work as a substitute for the underdeveloped non-logical half of the bipartite theory that he himself could not take care of, and whose realization had been neglected also by his fellow empiricists.

I reject this idea, which seems in a way to have also been embraced by Uebel and other revisionists. One of my reasons is pointed out by Mormann and Ibarra (2010, 77):

Even Carnap himself, in his later writings after the publication of *Structure* in 1962, never mentioned *Structure* – not even in his semipopular non-technical *Philosophical Foundations of Physics* (Carnap 1966) where mentioning *Structure* would have been quite appropriate - if one considers *Structure* as relevant for philosophy at all. Apparently, Carnap did not. For him, *Structure* remained a “nice piece” of history of science.

This fact has been seldom noticed in the growing literature on the alleged similarity or alliance between the views of Carnap and Kuhn. An exception is the recent paper of Pinto de Oliveira (Pinto de Oliveira 2007). He considers as the cause of this omission Carnap's strict separation between history of science and philosophy of science.⁹

In addition to this criticism of mine, I identify here three points of tension in Uebel's interpretation. The first is the fact that the editors of the *Encyclopedia* did not take into account the historiography or underlying philosophical aspect of the history of science work. It was not due to a supposed affinity between Kuhn's conceptions and those of logical positivism that Kuhn was invited to write the monograph on the history of science. Three other authors had been invited first, including Sarton whose conception was entirely different from Kuhn's.

The second inconsistency results from Carnap's lack of interest in the history of science, revealed, as we saw, by action and omission. I believe that this disinterest cannot be conveniently explained by the division of labor argument. The very idea of division of labor depends on the positive valuation of someone else's work in the complementary area. Carnap not only did not write any works on the history of science, he also showed no interest in the discipline compatible with the importance attributed to it in Uebel's interpretation.

⁹ I am not referring to a “strict separation” between the disciplines, but as I argue throughout this article, that Carnap attributes only a traditional and very secondary role to the history of science.

The third point of tension is in Carnap's conception regarding the historiography of science. He seldom refers to the history of science, but when he does, he appears to clearly defend a Whig historiography or what I called the "old historiography of science", which is opposed to what Kuhn calls the "new historiography of science". It is strictly because of this new historiography that Kuhn attributes a distinct and important role to the history of science in the philosophy of science (See Hoyningen-Huene 1993, chap. 1, and Pinto de Oliveira 2012, 115).

I believe that Uebel is not entirely correct with respect to the "ideological" spectrum of the members of the Vienna Circle. In a classification based on the relative positions assumed in the debate regarding the protocol sentences, he admits that Carnap is part of the "left wing" of logical positivism, alongside Neurath. And so he extends that classification also to the debate which concerns us here, regarding the role of the history of science. On the contrary, as Hempel emphasizes,

Neurath's ideas differed fundamentally from those *mainstream logical empiricism as advocated by Carnap* (...) Carnap and Popper (but not Neurath, as will be seen below) were emphatic in rejecting such a "naturalistic" view. Accordingly, they held it to be *strictly irrelevant* for the logical analysis of science to study the biological, psychological, and sociological factors that can affect scientific inquiry as a concrete human activity (Hempel 2000, 300-301, my italics).

And he continues, aiming to emphasize the secondary nature of the "naturalist" approach to science for Carnap:

There was, to be sure, *a polite bow* in the direction of a pragmatic study of psychological, historical, political, and social facets of actual scientific research behavior: such study might shed light on the ways in which that behavior *deviates* from analytic-empiricist standards (Hempel 2000, 301, my italics).¹⁰

It is worth comparing the passage with a text by Feigl that I cited in my 2007 article (p. 153):

I do not for a moment deny the psychological, social, economic or political factors that have on many occasions had a powerful influence upon the thinking of scientists. But to become aware of these *distorting influences* is already the first step toward their successful *elimination* (Feigl 1961, 15, my italics).

¹⁰ It should be taken into account that Hempel calls Carnap's theoretical position "analytic empiricism".

In light of this, I think it is very difficult to support a bipartite interpretation in the strong sense intended by Uebel, in which, as he says, Carnap “does not exclude the social from the rational” (p. 134). What Hempel (and Feigl) said about Carnap is precisely that the social (like the historical) serves only to indicate and explain *the deviations* in relation to the previous pattern of rationality established by the logic of science. This becomes even clearer when compared to Kuhn’s perspective, as described by Hempel himself:

Yet despite his naturalistic, socio-psychological account of theory choice, Kuhn calls science a rational enterprise. Thus he declares: “scientific behavior, taken as a whole, is the best example we have of rationality,” and “if history or any other empirical discipline leads us to believe that the development of science depends essentially on behavior that we have previously thought to be irrational, then we should conclude not that science is irrational, but that our notion of rationality needs adjustment here and there.” (Hempel 2001, 359).¹¹

By the way, it seems strange that Hempel, whose early mentor was Carnap and who was later widely influenced by Kuhn, does not refer to the supposedly strong philosophical relations between the two authors. It is noteworthy, however, that Hempel, at the same time, points out the similarities between Kuhn and Neurath. He writes, for example, that “with respect to their pragmatic-sociological orientation, the ideas of Neurath share a clear affinity with the ideas of Kuhn” (Hempel 2000, 194).

In truth, Hempel pointed out an opposition in the Vienna Circle between “two quite different schools”: Schlick and Carnap’s program, on the one hand (what we could call the right wing), and the more pragmatic focus of Neurath, on the other (the left wing). According to Hempel, in fact, the latter program was not attacked by the critics of positivism and is as alive now as it was before (Cf. Wolters 2003, 117-118. See also Hoyningen-Huene 1992b, 89-94).

Uebel writes in his article:

So far my argument still leaves unaddressed the claim that Carnap did classify Kuhn’s work as history of science and he was wrong to do so. One correct point that we can take from Pinto de Oliveira’s critique is that Carnap did indeed understand and appreciate Kuhn’s work as history of science, not as philosophy of science. But this point only raises thorny questions, it does not answer them (p. 135).

¹¹ Hempel quotes from Kuhn 1971, 144, and Kuhn 1970b, 264.

In the historical context that I sought to present here, if I am in fact correct regarding the point that Uebel (like Wray¹²) kindly conceded to me, I think that it does answer the questions. And I would not say that Carnap was wrong in thinking what he thought about Kuhn's work. I believe that he merely saw it from his own perspective, and obviously, owes us no apologies for that. After all, it was necessary to spend a considerable amount of ink over the past 50 years – since Kuhn's *Structure* was published – to finally be able to say (some of us still apprehensively) that we changed perspective in the philosophy of science.

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¹² See Wray 2012, 4.

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