History of Philosophy of Science as Philosophy of Science by Other Means?
Comment on Thomas Uebel

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1. Introduction. Adorno once remarked that the history of philosophy is the history of forgetting. Problems and ideas once examined fall out of sight and out of mind only to resurface later as novel and new. On the other hand, understanding history as historiography, the history of philosophy may be described as the organized and institutionalized attempt of overcoming the ever-growing threat of historical amnesia.

Even if „history of science is not just like other history" (Uebel), Adorno's dictum may well be true also for the (still rather brief) history of philosophy of science: problems and ideas once examined fall out of sight only to resurface later as novel and new. Examples are easily found: historical and sociological aspects of science that were treated by authors such as Duhem, Bachelard, Cassirer, Neurath or Fleck, fell into oblivion and were ignored by mainstream philosophy of science, only to be rediscovered decades later. In a different, perhaps even worse manner, the leading ideas of some historical currents, for instance classical logical empiricism of the Vienna Circle or Neokantian philosophy of science, suffered from partial historical amnesia: seriously distorted by later authors they sometimes served as their own caricatures. Thereby, to borrow Nestroy's well-known *bon mot* the progress in philosophy of science often tended to look bigger than it really was.

Adorno might have exaggerated the danger of historical amnesia that threatens philosophy, and certainly he did not spend a thought on the situation in philosophy of science. But his claim might serve as an antidote against the naive idea that the history of philosophy of science has been a history of permanent and unilateral progress in the sense that today we obviously possess a better philosophical understanding of the sciences than our forefathers. Rather, the more progress history of philosophy of science made in recent years the more it became clear how much remains to be done for achieving an adequate understanding of the still young discipline called philosophy of science.

This note will not deal with history of philosophy of science in general, it only has the modest aim to make some comments on Uebel's survey article on the history of *analytical* philosophy of science, in particular on his proposal to understand the task of history of philosophy of science as doing „philosophy of science by other means" conceiving it as an arsenal of

abandoned or forgotten conceptual possibilities, or, as he put it, as the „exploration of paths not taken".

The outline of this paper is as follows: In the next section I'd like to point out in what sense Uebel's topic – the history of *analytical* philosophy of science – may be considered as a particularly difficult but also particularly interesting topic of history of philosophy of science. The aim of section 2 is to show that the understanding of „history of philosophy of science as philosophy of science by other means" may be conceived as an expedient strategy of forestall a profusion of undesired meta(meta)disciplines which threaten the conceptual unity of an interdisciplinary research dealing with the history and philosophy of scientific culture. In section 3 I deal with some problems that may arise for Uebel's favorite example of a „path not taken", to wit, what he calls the „bipartite metatheory" of Carnap, Neurath, and Frank. In section 4 I argue that Uebel's proposal of a combined metatheory may be compared with Morris's earlier proposal of a synthesis of logical empiricism and American pragmatism in the 1930s. Morris's attempt failed, and it may be useful to inquire into the reasons of this failure to be in a better position to assess the prospects of Uebel's proposal.

1. The Many Issues on the Agenda of History of Philosophy of Science. Let me start with an observation concerning the programme of „History of Philosophy of Science" of this congress. From a logical point of view the collections of topics

- History of **Analytical** Philosophy of Science
- History of Philosophy of Science **in the French Tradition**
- History of the **19th Century** Philosophy of Science

may appear a bit mysterious. Comparing it with Borges's famous classification of „animals in a certain Chinese encyclopedia" intended to defy every attempt of explaining it rationally, may be exaggerated, but I think history of philosophy of science should make some efforts to explicate as clearly as possible the domain of possible issues that it wants to deal with. At least for some of the above mentioned items this seems easy enough. „History of Philosophy of Science in the French Tradition" and „History of the **19th Century** Philosophy of Science" may be characterized as „local" subdisciplines in a geographical and in temporal sense, respectively. Other examples for local subdisciplines in this sense easily come to mind:

„History of Philosophy of Science in the Polish Tradition“ or „in the German Tradition“, or „History of the **18th or 17th Century** Philosophy of Science“ and so on.

However, a „dimensional“ classification of this kind by no means exhausts the possible topics of our discipline: In a recent newsletter the editors of the Society for History of Philosophy of Science (HOPOS) call for a series of „state of the art“ essays dealing with „HOPOS figures“ (sic) including Aristotle, Descartes, Newton, Leibniz and many others. There is no reason to criticise such a personalized approach of history of philosophy, but it is certainly of a quite different kind than the one dealing with „local“ topics of various type. Still another possibility is exemplified by Uebel's choice of „Analytical Philosophy of Science“ as a topic of history of philosophy of science. Without arguing for it I think that Uebel's choice is „more philosophical“ and more interesting, at least if we conceive history of philosophy of science as another way of doing philosophy of science.

Nevertheless, the topic of analytical philosophy of science is a somewhat delicate choice. Some philosophers, rooted in the analytical tradition, still believe that analytical philosophy of science is the only philosophy of science that is to be taken seriously. All other efforts undertaken by philosophers in the course of history to come to terms with science may simply be disqualified as metaphysical rubbish. A similar, slightly less pretentious stance is to consider all non-analytical philosophy of science as nothing but a historical precursor of the real thing, i.e. analytical philosophy of science. This way of interpreting the place of analytical philosophy of science is not a mere remote possibility, it has been common usage in many quarters. For instance, Kitcher and Salmon, in their anthology *On Scientific Explanation*, classify Duhem's *La Theorie Physique* explicitly as belonging to the "Modern Prehistory" of the subject (cf. Kitcher and Salmon 1989, *Chronological Bibliography*, 196). Uebel does not subscribe to these radical ways of determing the place of analytical philosophy of science in the history of the subject. Rather, he is at pains to point out that the boundaries between the analytical and other currents of philosophy of science are by no means sharp. Nevertheless, the place of analytical philosophy of science remains remains difficult to determine, and in any case it is somewhat special compared with the apparently easily located local topics mentioned above and the more traditional way of doing things by dealing with the philosophy of science of the great dead philosophers of the past. This becomes evident when one attempts to imagine a coherent "history of non-analytical philosophy of science" or, perhaps better, a "history of continental philosophy of science". If such a history existed, it would be a rather mixed bag.

From an "American perspective" the attitude of conceiving analytical philosophy of science as the culminating point of the history of philosophy of science may be tempting and appears perhaps almost natural, but this perspective is certainly not without presuppositions, and one may ask whether from a "European perspective" these should be taken for granted. It leads to certain difficulties even if we restrict our attention to local topics of the history of philosophy of science as, say, philosophy of science in the German or the Austrian tradition. For instance, take Schlick's empiriocriticism of *Allgemeine Erkenntnislehre*. It can hardly be classified as "analytical", but I doubt, whether characterizing it as "pre-analytical" or "proto-analytical" is really fair. A similar, even more important problem arises for Neokantian philosophy of science, in particular Cassirer's as Uebel correctly remarks. Analogous remarks hold for conventionalist philosophy of science in the French tradition and other currents of European philosophy of science.

Willy-nilly, then, the analytical perspective often tends to play down the proper value of other currents of philosophy of science. It tends to ignore the losses philosophy of science has suffered on its way toward its analytical realization. I have no quick recipe how to overcome these difficulties, the only thing I want to say is that the choice of the topic „history of analytical philosophy of science" is not without presuppositions. Uebel seeks to avoid these difficulties. He rightly observes that Cassirer's Neokantian philosophy of science is important in its own right: it is problematic to conceive his account solely from the analytical perspective asking how it influenced Carnap on his way to analytical maturity.

Uebel attributes the growing importance of history of philosophy of science for philosophy of science to a naturalistic turn in philosophy that self-consciously rejects any a priori reflection about grand philosophical themes related to science. This is certainly correct. Moreover, I think Uebel is right in asserting that it is a trend to be heartily welcomed. A general naturalistic perspective seems to be less fond of producing intellectual fashions, necessarily accompanied by complementary blind spots, than aprioristic accounts. In this way naturalism may seem an antidote to the threat of amnesia mentioned in the beginning of this note.

3. History of Philosophy of Science as Philosophy of Science by Other Means. In the ongoing process of naturalization, which, of course, not only comprises the historical dimension, but also the sociological, the psychological and other ones, a lot of new meta-disciplines pop up:

- History of philosophy of science
- Psychology of philosophy of science
- Sociology of history of philosophy
- History of history of science

and so on. This may lead us to conceive „History of ...“, „Psychology of ...“ as sort of operators analogous to the modal operators such as „possibly“, „necessarily“, „obligatory“, and so on that are used in alethic or deontic modal logics. Iterations of such operators make sense formally, but become more and more opaque conceptually. Given a proposition p one may form expressions such as

$$\Box\Diamond\Diamond p \quad \text{or} \quad \Diamond\Box\Box\Diamond p$$

but probably very few people have an intuitive idea what these might mean. Analogously, the meanings of new metadisciplines such as „history of philosophy of sociology“, „philosophy of history of sociology“ tend to become obscure.[1] Uebel puts forward an important thesis that can be used to cut off this undesired multiplication of possible meta-disciplines:

> History of philosophy of science ... is ... predominantly *philosophy* of science. ...
> History of philosophy of science is philosophy of science by other means ....

This thesis is, of course, not new. In various forms, it has been brought forward by many authors, usually not restricted to philosophy of science, but claimed to hold for philosophy in general. One might recall that already Windelband, more than one hundred years ago, in his influential *Lehrbuch der Geschichte der Philosophie* (Windelband 1889) pondered on the relation between philosophy and history of philosophy. According to him, history of philosophy should be considered as an integral part of philosophy, namely as its „organon“ (ibid., 567).[2] Nevertheless, he insisted that both disciplines should not be mixed up, philosophers should not forget the philosophical over the historical (ibid., iii).

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[1] Actually, things are more complicated than the operator analogy suggests: for instance, it is far from clear how the relation between the “history of science” and “philosophy of science” is to be conceived. One, not overly convincing, option is to understand the first as “descriptive” and the second as “normative”. Another option is to claim an unspecified “complementarity” and “collaboration” of some kind between them. Further, as is exemplified by the logical empiricism and other currents of “scientific” philosophy, there are interesting relations between “history of philosophy of science” and “history of scientific philosophy” that render the agenda of “history of (philosophy of) science” rather complicated (cf. Richardson 2008).

[2] Regrettably, Windelband nowhere explained exactly what he understood by “organon” here. There is no reason to expect that the role that he had in mind for history of philosophy resembled very much to that which Uebel is thinking of.

It is a common place that traditionally the historical was a stronghold of continental philosophy, while the interest of analytical philosophy in history of philosophy was less fully developed. Recently, this state of affairs is changing as is evidenced, for instance, by the meanwhile well-developed history of the logical empiricism of the Vienna Circle and similar currents that are usually pursued from an analytical perspective.

Even if there is a rather general consensus among philosophers that history of philosophy should play a certain role for philosophy, it is far from clear what precisely this role is to be.[3] For instance, some contend that we have to know the history of philosophy (of science) so as not to avoid important alternatives to comtemporary proposals (cf. Curley 1986). This seems to be the stance of Uebel, Hardcastle, Richardson, and other philosophers of science. In the case of philosophy in general, some authors want to go further, claiming that philosophy is essentially an historicist endeavor (cf. Cohen 1986). How far this attitude may be applicable also to history of philosophy of science, remains to be investigated.

In any case, if we take into account something like Uebel's thesis the profusion of meta-disciplines becomes less disturbing. Doing history of philosophy of science, or sociology of philosophy of science, just means doing philosophy of science in specific ways. Conceiving history of philosophy of science as one of the ways of doing philosophy of science, it is natural to ask why we should pursue the historical way of doing philosophy of science, and what achievements we can expect from this endeavor.

Philosophers of science have dealt with this question for some time now. Roughly in line with Uebel's naturalistic turn, some years ago Hardcastle and Richardson spoke of a „historicist turn" in philosophy of science that might help to overcome the crisis that plagues philosophy of science. By this they did not mean the turn inaugurated by Kuhn's *The Structure of Scientific Revolutions*:

> „We refer to a more recent development in which philosophers have begun to recover the problems, solutions and motivations of earlier projects in the philosophy of science, paying attention to how the historical figures engaged in these projects understood them. ... Adapting what is perhaps the most famous sentence in the philosophy of science of the second half of the twentieth century, we can assert that the history of the philosophy of science is coming to be viewed as more than a repository for anecdote and chronology, and can, if we allow it, produce a decisive transformation in the *philosophy of science* we now possess." (Hardcastle and Richardson 2003, vii).

[3] As it seems, there has not been made too much progress in this issue since the times of Windelband.

To be explicit, for Hardcastle and Richardson the „most famous sentence in the philosophy of science of the second half of the 20th century" is Kuhn's dictum: „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 now are possessed." (Kuhn 1962, 1) In agreement with Uebel, then, Hardcastle and Richardson contend that history of philosophy of science is philosophy of science by other means. Moreover, they claim that philosophy of science urgently needs this new means. According to them, contemporary philosophy of science is entangled in a deep conceptual, almost existential crisis, and history of philosophy of science might help overcome it. Indeed, they invite us to tap the spiritual sources of the past:

> „[I]t may well be time to return to the social spirit of philosophy of science of the 1930. Perhaps that is our best philosophical venture in a world of anxious social and technological Maybes." (ibid., xxvi)

Uebel does not speak of a crisis. Less alarmist he is to content to point out that the historical way of doing philosophy of science may help us to recover conceptual resources that we lost from sight. As he rightly contends, not „everything „logical positivist" or everything „transcendental idealist" belongs in the dustbin of history. In other words, he proposes to use history of philosophy of science as a source for exploring hitherto undeveloped or underestimated conceptual possibilities. In this respect I fully agree with him. History of philosophy of science might be a way of doing philosophy of science that overcomes the wide-spread *historical amnesia* reigning in many quarters of philosophy, as already Adorno lamented. On the other hand, history is certainly not a foolproof way of doing philosophy of science. It may lead us astray by inviting us to indulge in the idea of *an Golden Age of Philosophy of Science* when allegedly our discipline flourished in every respect. Due to historical contingencies, this narrative claims that later philosophy of science deviated from the right path and ended up in the morass where we presently find it. Uebel does *not* consider himself as an aficionado of the *Golden Age*.

4. The Bipartite Metatheory: A Path to be Taken? Even if the conception of history of philosophy of science as a special means for doing philosophy of science is not the only way of pursuing it, for philosophers of science it is perhaps the most natural attitude. Uebel, apparently in line with Richardson, Hardcastle, Howard, and many others conceive the reflexive dimension that is opened up for philosophy of science by paying attention to its

history as a means for making *proposals*. He invites us to employ the historicist option to do philosophy of science and to explore „paths not taken". More precisely, he proposes to reconsider a path that in the first half of the 20th century allegedly was persued by Carnap, Neurath, and Franks but later was abandoned by their successors. This path he dubs the path of a „bipartite metatheory of science". Uebel considers it as a promising strategy for contemporary philosophy of science. This „bipartite metatheory" has two components:

- a logical component in the sense of a Carnapian logic of science;
- an empirical part roughly in the sense of Neurath's „behavioristics of scholars" or Frank's „pragmatics of science".

Uebel contends that this bipartite theory should be considered as the common legacy of three great figures of the Vienna Circle, to wit, Carnap, Neurath, and Frank. I must confess, that I am skeptical about the prospects of pursuing further the „bipartite meta-theory", even if the great figures of the Golden Age of philosophy of science could be read as having subscribed to it as a desideratum, although in their later careers they did not undertake serious efforts to realize it. It seems not totally unreasonable, as Uebel admits, to doubt that such a theory as a more or less coherent conceptual enterprise has ever actually existed. To me, the mere coexistence and alleged complementation of Carnapian logic of science and Frank-Neurathian pragmatics of science, do not justify the claim that there was a kind of theory that comprised the logic and pragmatics as two sub-theories. After all, a theory should have a certain amount of theoretical unity, which the bipartite metatheory is clearly lacks. Uebel himself admits that the two halves of his envisaged bipartite theory neither share a common methodology nor a common object. In line with Neurath, for him it suffices that the relation between the logic of science and the pragmatics of science was one - or could have been one - of „coexistence and complementation".[4] In my opinion, Uebel is rather indulgent with the often vague programmatical announcements of the founding fathers of classical logical empiricism.[5] For

[4] I consider this condition as too weak as though it could distinguish clearly the scientific complementation and collaboration of partisans of the project of a bipartite meta-theory from dubious endeavors such as the recent fashionable attempt of certain philosophers to construe "religion" and "reason" as "complementing and cooperating elements" of the modern condition.

[5] He shares this attitude with many scholars engaged in the history of early logical empiricism. This attitude might be understandable in view of the fact that the "true" history of logical empiricism and related currents has been unduly neglected for a long time, and is still neglected in some quarters of analytic philosophy perhaps even now. Nevertheless, I see certain dangers in this attitude.

instance, he emphasizes that it was an essential feature of the manifesto's programme that philosophy of science be a collective undertaking based on a well organized division of labour. By historical hindsight, however, the results of this envisaged „new way" of doing philosophy of science have been less than totally convincing. After all, up to now, the score of really collective work in philosophy of science has not been too impressive. In general, then, it is an important task for history of philosophy of science to identify the illusions and unfulfillable dreams that past philosophers cherished.

Let us come back to the issue of the bipartite metatheory. One may readily admit that it is, of course, always possible to weaken the requirements a theory has to satisfy in such a way that any iuxtaposition of more or less unrelated theoretical endeavors counts as a theory. The question is, whether it is useful to conceive such a thing as a theory. For the time being, the expression „bipartite metatheory" seems to be me more a name of a problem rather than a framework for a truly comprehensive philosophy of science. Be this as it may, my negative assessment should not be considered as an objection to Uebel's main thesis, namely, that history of philosophy of science is an expedient means for exploring conceptual possibilities for contemporary philosophy of science.

Having expressed my doubts about the feasibility of Uebel's proposal of exploring further the path the bipartite metatheory may be interpreted as sort of an obligation to make myself a proposal of an interesting path not taken that would be worth to be explored in the present situation. I think, however, that it would be more appropriate to the context of history of philosophy of science to point out that Uebel's proposal of a synthesis of logical and pragmatical currents of philosophy of science had an interesting precursor some sixty or seventy years ago. This attempt of synthesis failed for reasons that we do not fully understand up to now. Thus, it may be justified to rescue that unifying attempt from oblivion – not the least of these reasons the one that this example perhaps could shed some light on the feasibility of Uebel's proposal.

5. A Comprehensive Metatheory that Failed: Morris's Scientific Empiricism. Already in the thirties of the last century Morris had urged the logical empiricists of the Vienna Circle to think over their narrow concept of scientific philosophy as syntax of the language of science. Against this overly narrow conception of philosophy Morris argued for a pragmatist scientific philosophy that comprised four different stages: Painting with a broad brush Morris identified

four realms of scientific philosophizing labeling them with the names of Carnap, Peirce, Dewey, and Whitehead (cf. Morris 1937, 8ff.)[6]

- Philosophy as logic of science (Carnap)
- Philosophy as clarification of meaning (Peirce)
- Philosophy as empirical axiology (Dewey)
- Philosophy as empirical cosmology (Whitehead)

In this schema, Carnap's purely theoretical account of scientific philosophy as syntax of the language of science figured as the first and most restricted level of a comprehensive scientific philosophy which whould take into account not only the logical but other dimensions of of a scientific culture as well (cf. Morris 1937, 8ff). Morris readily admitted that moving from „Carnap" to „Whitehead" amounted to lowering the standards of exactness and certainty (ibid., 19). But he was convinced that scientific philosophy had to pay this price, if it wanted to be relevant for life in a comprehensive manner that took into account theory and practice of human existence. Moreover, he gave a compelling naturalist reason why it might be unscientific or even unreasonable to insist on Carnapian standards of exactness throughout:

> Science reveals no absolute break between theory and practice, and there is no clear reason why the situation should be different in philosophy. Meaning at the level of philosophical generality has its pragmatic dimension just a have the meanings at other levels. ... It would be a signal instance of ethical irresponsibility ... to turn the world over to the exclusive control of dreamers, adventurers, men of action, and technicians. (ibid., 20).

Carnap never showed much sympathy with the pragmatist unification programme even if he did not militate against it explicitly. Rather, he tried to eschew it in some way or other. In his reply to Morris's proposal (in the Schilpp volume) at the end of the day he had not more to offer than the bland assertion: „I am inclined to agree with Morris that the difference between my view and that of the pragmatists is not as large as it might appear at first glance" (Carnap 1963, 862). Notwithstanding this conciliary assessment he stuck to his ethical non-cognitivism clinging to the existence of „pure optatives" and refusing the the pragmatist „mean-end continuum".

[6] Uebel does not mention pragmatist philosophy of science in his list of „movements (within or fading into the analytical tradition)", probably because European philosophers have not contributed much to a philosophy of science from a pragmatist point of view. Be this as it may, history of pragmatist philosophy of science would certainly an important issue on the agenda of history of philosophy of science.

Despite Carnap's half-hearted conciliation the conceptual differences between Carnapian logical empiricism and American pragmatism of Dewey, Lewis, and Morris (to say nothing about Peirce and Whitehead) essentially remained as they were. Probably they can be attributed to the quite different conceptions of science underlying this currents of scientific philosophy. For Carnap, science was a system of theoretical knowledge – a set of consistent and rationally justifiable statements (Carnap 1935, 32). For Dewey, to take him as the most outspoken representative of a genuine pragmatist philosophy of science, science was rather a process or activity. Science, according to him, was not knowledge, but a process for solving problems. This entailed that Dewey and the other pragmatists contended that valuation was essential to the production of scientific knowledge, whereas Carnap insisted on a radical separation between knowledge and valuation. The gap between these fundamentally different philosophical perspectives on science was never really overcome as is shown by the difficult and finally rather unsatisfying coexistence between empiricist and pragmatist currents of scientific philosophy in the second half of the 20th century.

The failure of constructing a comprehensive „scientific empiricism" in Morris's sense should be taken into account when we seek to assess the chances of Uebel's „bipartite metatheory", even if the parallelism between Uebel's and Morris's proposals is limited. At first look, it might be tempting to associate Neurath's „behaviorism of scholars" and Frank's „pragmatic of science" at least *grosso modo* with the part that Morris reserved for the pragmatists in his sketchy program of a comprehensive philosophy of science. But I am not sure how far this goes. If Neurath's and Frank's accounts could serve as a pragmatic (or pragmatist?) complementation of Carnapian logic of science somehow analogous to classical American pragmatism that Morris had envisaged some decades ago then Uebel's bipartite metatheory would be confronted with similar difficulties that led to the abandonment of Morris's program.[7] On the other hand, if it would turn out that the complementation envisaged by Uebel were of a quite different kind than that which Morris envisaged, interesting problems concerning the relation between genuine American pragmatist philosophy of science and the Viennese ersatz pragmatism of Neurath and Frank would arise. In any case, there are still a lot of issues on the agenda of history of analytical philosophy of science that deserve to be studied in the future.

[7] Actually, I think that Neurath's and Frank's "pragmatism" or "pragmatic" would make a rather poor substitute of the real thing, but this is not an issue to be discussed here.

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