What's in it for the historian of science? Reflections on the value of philosophy of science for history of science

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1. Introduction

This year's prize question is a timely but much neglected one. In the wake of Thomas Kuhn's *The Structure of Scientific Revolutions* many philosophers of science have engaged philosophically with the history of science. Historians of science, on the other hand, have remained largely indifferent to philosophy of science and looked to other fields (anthropology, psychology, and sociology of science) for methodological guidance.

In thinking about the potential value of philosophy of science for history of science it would be helpful to distinguish between two ways of integrating history and philosophy of science. The first way, historical philosophy of science (HPS), is a familiar one and investigates epistemological and metaphysical issues, such as the rationality of scientific revolutions or the reality of unobservable entities, in light of the historical development of science. Influential works in this category include Philip Kitcher's *The Advancement of Science* (1993) and Michael Friedman's *Dynamics of Reason* (2001). The second way, what I call philosophical history of science (PHS), has a rather different aim: to reconstruct particular historical episodes or to address historiographical questions by engaging with philosophical issues about, e.g., experimentation or conceptual change. Works in this category include Hasok Chang's *Inventing Temperature* (2004), my own *Representing Electrons* (2006a), Friedrich Steinle's *Exploratory Experiments* (2016), and Jutta Schickore's *About Method* (2017). Thus, HPS and PHS have different goals: the former aims at philosophical enlightenment whereas the latter is motivated by historiographical concerns.

Despite the recent interest in PHS, however, the discussion on the relationship between history and philosophy of science has been skewed towards HPS. The focus of the discussion has been the relevance of historical case studies to the generation and appraisal of philosophical claims (see, e.g., Donovan et al 1988, Pitt 2001, Burian 2001, and Sauer & Scholl 2016). Remarkably, historians of science have been conspicuously absent from that debate (cf. Zammito 2004, chapter 4). As a result, the prospects and problems of PHS have received insufficient attention and the historiographical potential of philosophical reflection on scientific practice has remained largely unexplored (cf. Arabatzis 2016a).

The aim of this paper is to begin redressing this imbalance towards HPS and to spell out the added historiographical value of engaging with philosophy of science. My point of departure will be an observation that we owe to Norwood Russell Hanson: historical accounts of past scientific practice involve metascientific concepts (e.g., "discovery" or "experiment"), which are not philosophically innocent and require philosophical scrutiny (Hanson 1962; cf. Chang 2011 and Schickore 2011). I will suggest that philosophical reflection on these concepts can be historiographically fruitful: it can elucidate historiographical categories, justify historiographical choices and, thereby, enrich and improve the stories that historians tell about past science as a knowledge-producing enterprise.

The concepts / topics I will focus on are "epistemic values", "experimentation", "scientific discovery", and "conceptual change". Before I discuss these topics, however, I will digress briefly by commenting on an earlier attempt to enlist philosophy of science in the service of historical interpretation, that of Imre Lakatos. The purpose of examining Lakatos' historiography is to indicate what went wrong with philosophical history of science in the past. His philosophical-cum-historical project offended the sensibilities of historians of science and, thus, ended up alienating them. By distancing philosophical history of science from the Lakatosian (and related) project(s), I hope to remedy the indifference or outright hostility of many historians of science towards philosophy of science. Space may then open up for a philosophical history of science that obeys the standard canons of historiographical practice and is, thus, unobjectionable from a historian's point of view.

2. How not to do philosophical history of science

In "History of science and its rational reconstructions" Lakatos addressed the very question we are considering, namely "*how* the historiography of science should learn from the philosophy of science" (Lakatos 1976, 1). The historian's philosophical predilections, according to Lakatos, color his or her selection and interpretation of historical material. In particular, they circumscribe the domain of "internal" history, that is, the domain of beliefs and judgments that the historian considers rational. This domain is self-explanatory in the sense that it conforms to the historian's expectations for rational behavior. On the other hand, "everything that is irrational in the light of his [the historian's] rationality theory" is relegated to "external" history, which plays a secondary role, if any, in understanding the development of science (*ibid.*, 18).

Philosophy of science has the historiographical responsibility to suggest to the historian an adequate theory of scientific rationality. The adequacy of a rationality theory is judged by its ability to maximize the domain of internal history, that is, to justify as rational the largest possible number of the beliefs and judgments of the "scientific élite" (Lakatos 1976, 35).

The historian is then obliged to adopt the best theory of rationality and use it as a tool for "rationally reconstructing" past scientific episodes. When an episode appears rational in the light of that theory, it is considered fully explained. In that case the historian's explanatory task is complete. When, however, an episode cannot fit within the philosophical framework adopted by the historian, and in the absence of a better framework, he or she has to account for the "irrational" aspects of the episode in question by invoking "external" (social or cultural) factors.

There are at least two problems with Lakatos' approach to philosophical history of science. First, he invites the 'rational' historian to "radically improve" (Lakatos 1976, 18) the historical record when it does not conform to the best available theory of rationality. The gap between the historical record and its improvement is rendered conspicuous in the following way:

> One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and indicate *in the*

> *footnotes* how actual history 'misbehaved' in the light of its rational reconstruction. (*ibid*.)

Lakatos admits that this two-level narrative technique, applied to a reconstruction of Bohr's research program, may "strike the historian as more a caricature than a sketch" and he advises the reader to take some purportedly historical statements "not with a grain, but with tons, of salt" (Lakatos, 1980, 55)!

Second, and more important, Lakatos conflates the justificatory and the explanatory function of theories of rationality.[1] To explain a judgment of the "scientific elite" it is sufficient, according to Lakatos, to justify it on the basis of a particular rationality theory. Conversely, if no such justification is possible then the judgment in question requires an "external" explanation. However, the justification of a scientist's judgment and the explanation of why s/he made that judgment are distinct acts. The former does not imply the latter and vice versa. A judgement that is justified from the point of view of a contemporary theory of rationality may be unfathomable from the point of view of a scientist's beliefs and methodological preferences. Conversely, an unjustified judgment may be perfectly understandable in light of those beliefs and preferences.

The philosophical histories that have written by members of the Lakatos' school exhibit the same conflation. If a past scientific episode appears justified from the point of view of his methodology, there is nothing left to explain. Whereas most historians would consider "what actually attracted scientists to or repelled them from the various research programmes under study", Lakatos' followers focus exclusively on the "philosophically relevant difference[s] between research programmes" and assume that those differences carry all the explanatory weight (Kuhn 1980, 188).

Thus, Lakatosian "explanations" do not qualify as historical explanations. They do not (cannot) explain why a scientist held certain beliefs, pursued certain lines of inquiry, or made certain decisions (e.g., to accept or reject a theory). To explain such facts a

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[1] The confusion between historical explanation and post-hoc rational justification permeates Lakatos' "History of science and its rational reconstructions". See (Lakatos 1976, 13, 14, 15, 17, 26, 33). Cf. (Arabatzis 1994).

historian would have to recover, to the extent possible, the scientists' own point of view and the reasons for their beliefs and judgments. Lakatos' exclusive preoccupation, on the other hand, is with justifying those beliefs and judgments on the basis of his methodology. I see nothing wrong with this ex post facto justificatory game, which serve the purpose of developing an adequate theory of scientific rationality. I object, however, to its being disguised as historical interpretation.

The above criticism of Lakatos' historiography is not meant to show that there are no insights in his philosophy of science that could be incorporated into a philosophically informed history of science. Such an insight, for instance, is his suggestion that scientists do not exhibit the same commitment to all the constituents of a research program. Those insights, however, should be employed in historiography in a manner that is far removed from the one envisaged by Lakatos. Rather than forcing "history of science into … [a] Procrustean bed …, thus creating fancy histories, which hinge on mythical [methodologies],"[2] a philosophical perspective, if worth its mettle, should assist the historians to write better history. In other words, it should enable them to tell richer, more plausible, and more coherent stories about past scientific practice. In the remaining parts of this essay I will discuss four topics where insights from philosophy of science can be (and, indeed, have been) brought to bear on and enrich the historiography of science.

3. Epistemic values

In 1968 Kuhn gave a lecture on "The relations between the history and the philosophy of science," where he argued for one way traffic between the two fields (Kuhn 1977a): whereas history of science is relevant, indeed indispensable, to philosophical reflection on science, the converse is not the case. Kuhn thought that analytic philosophy of science had little, if anything, to offer to the historian of science. Curiously though, Kuhn did not discuss the potential relevance of his own philosophy

[2] Lakatos 1976, 28. Lakatos used these words to castigate the rational reconstructions of other "historiographical research programs", such as inductivism, conventionalism, and falsificationism. They apply equally well, however, to his own rational reconstructions.

of science, or that of his fellow historicist philosophers of science, to historiographical practice. As a matter of fact, there are philosophical insights in Kuhn's work that have significant historiographical value. I do not have in mind the usual suspects, paradigms and scientific revolutions. Rather, I want to focus on a lessdiscussed topic, epistemic values, and argue that Kuhn's reflections on that topic are not only philosophically illuminating but also historiographically fecund.

In a seminal article (Kuhn 1977b) Kuhn set the problem of theory-choice on a new basis. Rather than conceiving theory-choice as a rule-governed process, as was common at the time, he emphasized the role of epistemic values in the appraisal and selection of scientific theories. Kuhn's values were not original. They included empirical adequacy (and the cognate values of explanatory and predictive power), wide scope, simplicity, internal consistency, compatibility with other established theories, and fertility. Kuhn's originality consisted in his suggestion that, in practice, the application of those values to the appraisal of scientific theories is extremely complicated. This is due to three reasons.

First, those values do not wear their interpretation on their sleeves. Different scientists may interpret them differently. This is most evident in the case of simplicity, which lacks a universally accepted definition. But even in the case of *prima facie* straightforward values, such as empirical adequacy, disagreements may arise about the degree of accuracy to which a theory should aspire.

Second, and more important, the hierarchy of these values is not unanimously agreed upon. Some scientists may attach more weight to, say, simplicity than to empirical adequacy and vice versa. For instance, when Walter Kaufmann's experiments with fast-moving electrons indicated that the variation of their mass with velocity did not conform to Lorentz's and Einstein's predictions, Lorentz thought that his theory had been refuted.[3] Einstein, on the other hand, dismissed Kaufmann's results as indecisive. Stressing the intuitive character and the simplicity of his theory,

[3] See Lorentz's 1906 letter to Poincare, quoted and discussed in Miller 1985, 85.

he considered its "probability … so high that Dr Kaufmann's results have every chance of being flawed."[4]

Third, in actual scientific life these values are rarely satisfied unequivocally. Consider, for instance, explanatory power. A theory may do a better explanatory job than its competitor(s) in a certain domain of phenomena and a worse explanatory job in other domains. According to Kuhn, this is precisely what happened in the chemical revolution:

> The oxygen theory … was universally acknowledged to account for observed weight relations in chemical reactions, something the phlogiston theory had previously scarcely attempted to do. But the phlogiston theory, unlike its rival, could account for the metals' being much more alike than the ores from which they were formed. One theory thus matched experience better in one area, the other in another (Kuhn 1977b, 323).

From a Kuhnian point of view, one may easily understand the protracted disagreements and controversies that are often found in scientific practice. By taking into account the complex interplay of epistemic values in theory appraisal, those features of scientific life can be plausibly understood as consequences of the epistemic uncertainties that are endemic in science.

Kuhn himself relied on epistemic values to account for the reception of Copernican astronomy and its battles with the Ptolemaic system (Kuhn 1957). More recently, Hasok Chang deployed Kuhn's framework to shed new light on the chemical revolution. Chang argued that the two opposed sides in the chemical revolution, "phlogistonists" and "oxygenists", were concerned with different problems and favored different methods and epistemic values. It is small wonder that the conflict between phlogiston-based chemistry and oxygen-based chemistry was so difficult to resolve and that the former was kept alive till the early 19th century. Thus, Chang's philosophical history of the chemical revolution highlights the historiographical

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[4] This is how Olivier Darrigol presents Einstein's reaction to Kaufmann's results in a dialogue between Einstein and other early 20th century physicists that is fictitious, but grounded in historical sources. See Darrigol 2000, 386. Cf. also Topper 2007, 8.

fruitfulness of the notion of epistemic values. On the basis of this and other philosophical tools he has been able to reconstruct the complexity of the epistemic landscape of late 18th century chemistry, to offer a balanced account of the conflicting approaches in that landscape, to present fairly the losing side, and to capture its rational merits (Chang 2012a, b).

Kuhn's list was not (and not meant to be) exhaustive. Philosophically inclined historians may want to add more epistemic values, such as unification or completeness, to the Kuhnian repertoire and employ them in investigating episodes from the history of science. In any case, Kuhn's analysis of the interplay of epistemic values in scientific practice suggests a capacious framework for understanding past scientific life. In contrast with Lakatos' methodology, which aimed at justifying retrospectively the judgments of the scientific élite, the philosophical resources provided by Kuhn do not necessarily vindicate the winners of past scientific controversies.

Kuhn's work indicates that philosophy of science can be historiographically employed in an interpretive rather than a vindicatory mode. Philosophical concepts, such as the concept of epistemic values, can function as interpretive resources for the historian and help him or her to illuminate the epistemic complexity of the problem situations faced by past scientists. Philosophically equipped historians may, thus, develop a sympathetic perspective on the "losers" of scientific controversies, and come to appreciate their reasons for resisting theoretical novelty and their being left behind by mainstream science. Philosophy of science in the interpretive mode is thus fully compatible with the historical imperative to maintain a critical distance from the scientists' perspective on the history of their fields (Forman 1991).

So far I have focused on the relevance of philosophy of science to the historical interpretation of theoretical activity. For a long time philosophy of science, including Kuhn's, was fixated on the theoretical aspects of the scientific enterprise. Happily, the philosophical landscape has now expanded so as to include experimental practice. As I will suggest in the next section, the insights of experimentalist philosophy of science are indispensable for understanding historically how experimental knowledge is generated, validated, and developed.

4. **Experiment**

The philosophy of experiment is a subject with a great past. Its origins go back to the 17th century when experimentation emerged as a new epistemic activity. The lack of legitimacy of experimentation forced its proponents to articulate its methods and to defend the authority of its results. Both of them were controversial for two reasons. First, the very idea of obtaining knowledge via experimental intervention violated a central methodological tenet of Aristotelian natural philosophy, namely to ground knowledge on the unobtrusive observation of natural processes. Second, the authentication of experimental results, especially when produced in artificial settings that were not publicly accessible, was problematic. The more so because of the poorly functioning instruments often involved in experimentation, instruments such as Boyle's air-pumps or Newton's prisms (see Arabatzis 2005). Those issues were gradually resolved and experimentation ceased to be subject to philosophical scrutiny. By the mid-20th century it had come to be considered entirely unproblematic and philosophically uninteresting. Philosophers of science bothered to consider experiment only to the extent that it was involved in the generation and testing of theoretical knowledge (see Arabatzis 2014).

This unhappy situation continued till the early 1980s, when the pioneering work of Ian Hacking reinstated experimentation as a central topic in the philosophy of science (Hacking 1983). His innovative philosophical perspective has been articulated and "applied" to history of science by other historians and philosophers of science, including Allan Franklin (1986), Peter Galison (1987, 1997), and David Gooding (1990). Several philosophical insights have come out of that work and proved historiographically fruitful. Three of the most salient are the following:

First, and in contrast to Lakatos, who stressed "*the high degree of autonomy of theoretical science*" (Lakatos 1976, 10), historians and philosophers of experiment have emphasized the partial autonomy of experimental science and explored its historiographical ramifications. Experiment is partly independent from theory in the following respect: the authentication of experimental results involves very different "strategies" from those involved in the authentication of theoretical claims (Franklin

1989). Thus, the epistemology of experimental knowledge is rather different from the epistemology of theoretical knowledge.

Second, historians and philosophers of experiment have argued that the role of experiment goes well beyond theory-testing. Experiments often have an exploratory character and aim at imposing order on a *prima facie* chaotic domain of phenomena. Exploratory experimentation goes hand in hand with the formation of new concepts that are essential for expressing regularities in the domain in question. In that respect, it involves theorizing, albeit of a rather different sort than formulating hypotheses and testing their predictions. Again, this insight has stimulated much excellent historical work (see, e.g., Steinle 2016)

A third insight that has come out of the philosophy of experiment concerns “theory”. This accordion term has been applied to very different things: phenomenological laws, models of the instruments employed in experimentation, models of the entities investigated or manipulated, deep unifying principles, and abstract mathematical constructs. This confusion has obscured significantly the relationship between theory and experiment. For instance, it has created the false impression that an experiment that is designed to test a theory is necessarily informed by that very theory (Duhem 1954, 182). The differentiation between various kinds of theoretical knowledge, on the other hand, has shown that a theory under test and the “theories” informing the experimenters’ work need not overlap.

These insights have been particularly significant for my own historical work on the discovery of the Zeeman effect. As I have shown in (Arabatzis 1992), Zeeman’s original aim was to perform an exploratory experiment for investigating the influence of magnetism on light and not to test a theoretical prediction. Furthermore, the authentication of his experimental results had nothing to do with high-level electromagnetic theory, that is, Lorentz’s theory of electrons. Rather it relied on low-level experimental knowledge about the instruments employed and the phenomena investigated. On the other hand, the subsequent refinement of Zeeman’s results went hand in hand with the articulation of a central concept of Lorentz’s theory, the concept of “ion” (later renamed “electron). Thus, the philosophy of experiment has offered me an invaluable guide to understanding Zeeman's experimental practice and its complex interplay with theory.

The relative autonomy and exploratory character of much experimentation indicate the need for a multi-level periodization of past science. Theory and experiment may have different temporalities. A rupture in theoretical science need not be accompanied by a corresponding break in experimental science. Conversely, the evolution of laboratory life need not follow the developments in theoretical practice. These historiographical implications of the philosophy of experimentation are amply borne out by the historical record. For instance, the famous revolutions in early 20th century theoretical physics (relativity and quantum physics) had no significant impact on the practice of experimental physicists. Conversely, important developments in experimental physics, such as the emergence of large-scale collective research in the mid-20th century, had no immediate effect on theoretical work (Galison 1997).

In all, philosophical reflection on experimentation has been historiographically productive. It has shed new light on past experimental practice and generated novel historiographical insights. A similarly fruitful embrace between historical study and philosophical theorizing can been seen in our next topic, scientific discovery.

5. **Discovery**

The concept of discovery occupies a central place in the scientists' self-consciousness. It has also been extensively analyzed and used by historians and philosophers of science. The philosophical discussion on discovery can be traced back to the 17th century (Laudan 1980) and there are many recent historical studies of particular scientific discoveries (see, e.g., Nickles 1980). The very domain of philosophy of science in the 20th century has been demarcated from that of history and sociology of science via the well-known distinction between the context of discovery and the context of justification. This distinction has been repeatedly challenged and is now considered passé. The two contexts are inextricably linked: the generation and validation of new knowledge are two aspects of a single discovery process (Arabatzis 2006b).

However, there is another philosophical misconception about scientific discovery that has been more resistant to criticism, namely that discoveries are events that can be attributed to specific scientists, unambiguously identified, precisely dated, and localized. This stereotype about discovery permeates scientific popularizations and science textbooks. Unfortunately, it has also had a pernicious influence on some historical work (see Arabatzis 1996).

The stereotypical view of discovery gives rise to the following W-questions:

- Who made the discovery?
- What was it about?
- Where was it made?
- When did it take place?

Philosophical reflection on scientific discovery, on the other hand, has shown that often these questions cannot be answered because they are based on a conflation of discovery with a flash of insight or the observation of a novel phenomenon. This conflation was exposed and criticized by Kuhn, in a pioneering article and his classic book (Kuhn 1962, 1970). Kuhn distinguished two kinds of discovery: of expected and unexpected phenomena (or entities). The former kind fits the stereotype: it is easy to identify the discoverer, the object of the discovery, and its location in space and time. The latter kind, however, is incompatible with the stereotypical conception of discovery. Discoveries of unexpected phenomena (or entities) are extended processes involving several scientists and impossible to precisely date or localize. These discoveries comprise the observation **and** conceptual assimilation of a novel phenomenon (or entity). The latter involves the overthrow of the existing conceptual framework and its replacement by a new one. This conceptual rearrangement is rarely the work of a single scientist and it inevitably extends through time and space. That is why the above W-questions do not admit of unambiguous answers.

Kuhn’s novel philosophical perspective shed new light on the discovery of oxygen. Older accounts of the chemical revolution had given conflicting answers to the W-questions concerning that discovery. Some had given credit for that discovery to Joseph Priestley, despite Priestley’s life-long rejection of the oxygen theory of combustion. Other accounts had identified Antoine Lavoisier as the undisputed discoverer of oxygen, despite the large conceptual distance separating Lavoisier's "principle of acidity" from later conceptualizations of oxygen as an element that had nothing to do with acidification. From Kuhn's perspective, on the other hand, those questions are misleading and unanswerable since they presuppose a false conception of discovery as an event.

Subsequent scholarship has demonstrated the historiographical fertility of Kuhn's account of scientific discovery. Recent historical studies have fleshed out and refined Kuhn's insights (see, e.g., Arabatzis 1996, and Caneva 2005, and Dick 2013). In (Arabatzis 1996), for instance, I argued against the widespread view that J. J. Thomson singlehandedly discovered the electron in 1897. Instead, I stressed the collective, distributed, and multi-dimensional character of the discovery of the electron. Furthermore, new arguments have been adduced for understanding discovery as an extended process, even in the case of expected discoveries. Given that discovery involves justification, the mere observation of a phenomenon, expected and unexpected alike, does not amount to its discovery. Rather, in order for a discovery to take place, an argument needs to be made to the effect that the phenomenon in question is genuine and not an artefact of the experimental setting. Making this argument involves substantial experimental and theoretical work that may span many individuals and laboratories. At any rate, it is work that extends through time (Arabatzis 2006b, Frercks et al 2009).

Thus, an unreflective use of the stereotypical notion of discovery can lead historians of science astray by giving rise to unanswerable questions. Even worse, historians who espouse an inadequate philosophy of discovery may distort the historical record in order to fit it within their Procrustean image of discovery. This is the case, I think, with many historical studies of the discovery of the electron (Arabatzis 1996).

Conversely, the philosophical explication of the notion of scientific discovery can be historiographically fruitful. The philosophical insight that discovery is an extended process offers a fresh perspective on past scientific discoveries, including some that have been meticulously studied. In a recent paper, Kostas Gavroglu and I have revisited the discovery of argon by taking into account that very insight. We argued that a philosophically naïve notion of discovery as an event permeates the historical literature and blocks the *historical* understanding of the discovery of argon. We then attempted to understand the discovery of argon as an extended process; a process that began with the detection of a discrepancy between atmospheric and chemically produced nitrogen, continued with the isolation of the gas that gave rise to that discrepancy, and ended with the conceptualization of that gas as a new element. Our philosophically informed narrative steered free from unproductive questions about the particular timing of the discovery of argon or the identity of its discoverer(s). Rather our narrative highlighted the strenuous experimental work and ingenious theorizing that went into that discovery, work that extended over a period of several years and involved the purported discoverers and their critics alike (Arabatzis & Gavroglu 2016).

As I mentioned above, one of the reasons that discovery has to be understood as an extended process is that it involves concept formation and conceptual change. This is another philosophical issue with significant historiographical implications.

6. Conceptual change

The formation and evolution of concepts are at the core of philosophical history of science. Philosophical accounts of conceptual change are relevant to the historiography of science for at least two reasons. The first pertains to the object of history of science. The processes and mechanisms of conceptual innovation are crucial for understanding how new theoretical and experimental knowledge is generated, articulated, and communicated. The second reason concerns historical writing and, in particular, a presupposition of historical narratives. Such narratives have "central subjects" (e.g., an idea, a person, an institution) that, although evolving, are assumed to retain their identity over time (cf. Hull 1975). Let me elaborate.

Ever since Kuhn and Feyerabend invented the notion of incommensurability for describing conceptual ruptures in scientific development, the philosophical implications of that notion have been discussed *ad nauseam*. Notwithstanding the well-documented philosophical difficulties associated with conceptual incommensurability, some historians have found it historiographically useful. Here are three examples: First, Jed Buchwald has drawn upon Kuhn's explication of incommensurability in terms of taxonomic structures, to shed light on the development of early 19th century optics (Buchwald 1992). Second, Hanne Andersen, Peter Barker, and Xiang Chen have articulated incommensurability using ideas from cognitive science and then deployed it to reconstruct several historical episodes, ranging from the Copernican Revolution to 20th century nuclear physics (Andersen, Barker, and Chen 2006). Third, Andrew Pickering has used Kuhn's notion to frame his sociological history of high-energy physics (Pickering 1984).

These examples indicate the historiographical merits of incommensurability. However, Kuhn had very little to say about the fine structure of the processes that lead from the initial to the final stage of a scientific revolution. Understanding these processes historically requires philosophical tools that Kuhn did not supply. This gap has been admirably filled by Nancy Nersessian, who has argued that conceptual change is the outcome of problem-solving activities that involve a variety of forms of reasoning. These forms include the drawing of analogies, the construction of visual representations, the formulation of abstractions and idealizations, and the performance of thought experiments. According to Nersessian, these forms are different aspects of "model-based reasoning", which is prominent in the work of scientific giants, such as James Clerk Maxwell, and ordinary mortals alike (Nersessian 2008).

All these works, from Buchwald's to Nersessian's, indicate that philosophical considerations about conceptual change are essential for understanding historically how new scientific knowledge emerges and supplants older knowledge. The philosophy of conceptual change is also relevant to meta-historical issues, such as narrative construction. I realized this when I was writing *Representing Electrons* (Arabatzis 2006a). In that book I had to come to terms with a presupposition of telling the story of the concept of the electron. In order for that project to get off the ground I had to assume that, despite the radical transformation of the concept of the electron from J. J. Thomson to, say, Paul Dirac, diachronic uses of the term "electron" hang together and belong to a single history. But if the concept of the electron changed dramatically from the late 19th century till the late 1920s how can a historian construct a coherent narrative around it? More generally, if a concept does not retain its identity over time, what is its history the history of (cf. Dear 2005; Kuukkanen 2008)? As I argued in *Representing Electrons*, to answer this question one has to engage with the extensive philosophical literature on conceptual change and come to terms with the well-known conundrums about meaning and reference change.

This is not a mere philosophical exercise but has direct historiographical consequences. Depending on whether a historian accepts or rejects the identity of concepts over time, the choice of the concept of the electron (or any other concept for that matter) as the subject of a historical narrative may look justified or regrettable. For someone like Quentin Skinner, for example, who thinks that "there is no determinate idea to which various writers contributed, but only a variety of statements made with the words by a variety of different agents with a variety of intentions", there is "no history of the idea to be written" (Skinner 1969, 38).

In *Representing Electrons* and subsequent work (Arabatzis 2012) I suggested a way out of the historiographical nominalism advocated by Skinner: the diachronic uses of the term electron were associated with an expanding set of experimentally produced phenomena that were considered manifestations of the single same thing. This is one of the reasons that justify their inclusion in a single historical account. Regardless of the viability of this suggestion, however, one thing is clear: an engagement with the philosophy of conceptual change is historiographically necessary. The very possibility of conceptual history hangs on it.

7. Coda

What about the question we started with: what can philosophy of science offer to the historian of science? I have indicated four topics of philosophical discussion (epistemic values, experimentation, scientific discovery, and conceptual change) that can be fruitfully brought to bear on the history of science. In each of those areas I focused on philosophical insights or issues that are directly relevant to historiographical practice: First, the insight that theory-choice is underdetermined by epistemic values, either because different scientists interpret the same value differently or because they attach different weights to different values, can enable the philosophical historian to gain a deeper understanding of scientific controversies. Second, the insight that experimentation has a life of its own opens up space for a philosophical history of experimental science as a relatively autonomous activity with its own temporal rhythm. Third, the insight that discovery is an extended process invites philosophical historians to understand its historical structure and prevents them from entering the blind alley of W-questions. Fourth, an engagement with the intricacies of conceptual change can help the philosophical historian to address an important historiographical question: what is a historical narrative a narrative of?

Let me close with two caveats: First, I have touched upon just a few of the topics where an engagement with philosophy of science can pay off historiographically. These topics do not exhaust the possibilities for a philosophically informed history of science. Other topics (e.g., scientific representation, scientific objects, or the contingency of scientific development) would have been equally fitting for exploring the historiographical uses of philosophy of science. Second, the above philosophical insights may not be universally applicable: it may turn out that not all cases of theory-choice are underdetermined by epistemic values; that not all experiments have a life of their own; that not all discoveries are extended processes; and that not all concepts retain their identity over time. The philosophical historian should be open to these possibilities. After all, the historiographical value of any philosophical insight has to be shown on a case-by-case basis, by illuminating the epistemic situation as it was experienced by the historical actors. In any case, meeting these caveats will have to wait for another occasion.

References

Andersen, H., P. Barker, X. Chen (2006). *The Cognitive Structure of Scientific Revolutions,* Cambridge: Cambridge University Press.

Arabatzis, T. (1992). "The Discovery of the Zeeman Effect: A Case Study of the Interplay between Theory and Experiment," *Studies in History and Philosophy of Science* 23: 365-388.

Arabatzis, T. (1994). "Rational versus sociological reductionism: Imre Lakatos and the Edinburgh school," in K. Gavroglu et al. (eds.), *Trends in the Historiography of Science,* Dordrecht: Kluwer, pp. 177-192.

Arabatzis, T. (1996). "Rethinking the 'discovery' of the electron," *Studies in History and Philosophy of Modern Physics* 27, 405-435.

Arabatzis, T. (2005). "Experiment," in M. Horowitz (ed.), *New Dictionary of the History of Ideas,* Vol. 2, Detroit: Charles Scribner's Sons, pp. 765-769.

Arabatzis, T. (2006a). *Representing Electrons: A Biographical Approach to Theoretical Entities*, Chicago: The University of Chicago Press.

Arabatzis, T. (2006b). "On the Inextricability of the Context of Discovery and the Context of Justification," in J. Schickore and F. Steinle (eds.), *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction*, Dordrecht: Springer, pp. 215–30.

Arabatzis, T. (2012), "Experimentation and the meaning of scientific concepts," in U. Feest and F. Steinle (eds.), *Scientific Concepts and Investigative Practice*, Berlin: De Gruyter, pp. 149-166.

Arabatzis, T. (2014), "Experiment," in M. Curd and S. Psillos (eds.), *The Routledge Companion to the Philosophy of Science*, 2nd ed., London: Routledge, pp. 191-202.

Arabatzis, T. (2016), "The Structure of Scientific Revolutions and history and philosophy of science in historical perspective," in A. Blum, K. Gavroglu, C. Joas, and J. Renn (eds.), *Shifting Paradigms: Thomas S. Kuhn and the History of Science*, Berlin: Max Planck Institute for the History of Science, Edition Open Access, pp. 191-201.

Arabatzis, T. and Gavroglu, K. (2016). "From discrepancy to discovery: How argon became an element," in T. Sauer & R. Scholl (eds.), *The Philosophy of Historical Case Studies*, Switzerland: Springer, pp. 203-222.

Buchwald, J. Z. (1992). "Kinds and the Wave Theory of Light," *Studies in History and Philosophy of Science* 23:1, 39-74.

Burian, R. M. (2001). “The Dilemma of Case Studies Resolved: The Virtues of Using Case Studies in the History and Philosophy of Science,” *Perspectives on Science* 9:4, 383-404.

Caneva, K. L. (2005). “‘Discovery’ as a Site for the Collective Construction of Scientific Knowledge,” *Historical Studies in the Physical Science* 35:2, 175-291.

Chang, H. (2004). *Inventing Temperature: Measurement and Scientific Progress*, Oxford: Oxford University Press.

Chang, H. (2011). “Beyond Case-Studies: History as Philosophy,” in S. Mauskopf and T. Schmaltz (eds.), *Integrating History and Philosophy of Science: Problems and Prospects*, Dordrecht: Springer, pp. 109-124.

Chang, H. (2012a), *Is Water H_2O? Evidence, Realism and Pluralism*, Dordrecht, Springer.

Chang, H. (2012b). “Incommensurability: Revisiting the Chemical Revolution,” in V. Kindi and T. Arabatzis (eds.), *Kuhn’s The Structure of Scientific Revolutions Revisited*, New York: Routledge, pp. 153-176.

Darrigol, O. (2000). *Electrodynamics from Ampère to Einstein*, Oxford: Oxford University Press.

Dear, P. (2005). “What Is the History of Science the History Of?” *Isis* 96, 390 – 406.

Dick, S. J. (2013). *Discovery and Classification in Astronomy: Controversy and Consensus*, Cambridge: Cambridge University Press.

Donovan, A., Laudan, L, and Laudan, R. (1988), Scrutinizing Science: Empirical Studies of Scientific Change, Dordrecht: Springer.

Duhem, P. (1954). *The Aim and Structure of Physical Theory*, Princeton, N.J.: Princeton University Press.

Forman, P. (1991). “Independence, Not Transcendence, for the Historian of Science,” *Isis* 82:1, 71-86.

Franklin, A. (1986). *The Neglect of Experiment*, Cambridge: Cambridge University Press.

Franklin, A. (1989). “The Epistemology of Experiment,” in Gooding, D., Pinch T. and S. Schaffer (eds.), *The Uses of Experiment: Studies in the Natural Sciences*, Cambridge: Cambridge University Press, pp. 437-460.

Frercks, J., Weber, H., Wiesenfeldt, G. (2009). “Reception and discovery: the nature of Johann Wilhelm Ritter’s invisible rays,” *Studies in History and Philosophy of Science* 40, 143-156.

Friedman, M. (2001). *Dynamics of Reason*, Stanford: CSLI Publications.

Galison, P. (1987). *How Experiments End*, Chicago: The University of Chicago Press.

Galison, P. (1997). *Image and Logic: A Material Culture of Microphysics*, Chicago: The University of Chicago Press.

Gooding, D. (1990). *Experiment and the Making of Meaning*, Dordrecht: Kluwer.

Hacking, I. (1983). *Representing and Intervening*, Cambridge: Cambridge University Press.

Hanson, N. R. (1962). "The Irrelevance of History of Science to Philosophy of Science to Philosophy of Science," *The Journal of Philosophy* 59:21, 574-586.

Hull, D. L. (1975). "Central Subjects and Historical Narratives," *History and Theory* 14:3, 253-274.

Kitcher, P. (1993). *The Advancement of Science*, Oxford: Oxford University Press.

Kuhn, T. S. (1957). *The Copernican Revolution*, Cambridge, MA: Harvard University Press.

Kuhn, T. S. (1962). "Historical Structure of Scientific Discovery," *Science* 136:3518, 760-764.

Kuhn, T. S. (1970). *The Structure of Scientific Revolutions*, Chicago: The University of Chicago Press, 2nd ed.

Kuhn, T. S. (1977a). "The Relations between the History and the Philosophy of Science," in Kuhn, *The Essential Tension*, Chicago: The University of Chicago Press, pp. 3-20.

Kuhn, T. S. (1977b). "Objectivity, Value Judgment, and Theory Choice," in Kuhn, *The Essential Tension*, Chicago: The University of Chicago Press, pp. 320-339.

Kuhn, T. S. (1980). "The Halt and the Blind: Philosophy and History of Science," *British Journal for Philosophy of Science* 31:2, 181-192.

Kuukkanen, J.-M. (2008). "Making Sense of Conceptual Change," *History and Theory* 47, 351 – 372.

Lakatos, I. (1976). "History of science and its rational reconstructions," in C. Howson (ed.), *Method and Appraisal in the Physical Sciences*, Cambridge: Cambridge University Press, 1-39.

Lakatos, I. (1980), "Falsification and the methodology of scientific research programmes," in Lakatos, *The methodology of scientific research programmes, Philosophical Papers, Volume I*, Cambridge, pp. 8-101.

Laudan, L. (1980). “Why was the logic of discovery abandoned?” in T. Nickles (ed.), *Scientific Discovery, Logic, and Rationality,* Dordrecht: Reidel, pp. 173-183.

Miller, A. I. (1985). *Frontiers of Physics: 1900-1911*, Boston: Birkhauser.

Nersessian, N. J. (2008). *Creating Scientific Concepts*, Cambridge, MA: The MIT Press.

Nickles, T. (ed.) (1980). *Scientific Discovery: Case Studies*, Dordrecht: Reidel.

Pickering, A. (1984). Constructing Quarks: A Sociological History of Particle Physics, Chicago: The University of Chicago Press.

Pitt, J. C. (2001). “The Dilemma of Case Studies: Toward a Heraclitian Philosophy of Science,” *Perspectives on Science* 9:4, 373-382.

Sauer, T. and Scholl, R. (eds.) (2016). *The Philosophy of Historical Case Studies*, Switzerland: Springer.

Schickore, J. (2011). “More Thoughts on HPS: Another 20 Years Later. Perspectives on Science,” 19:4, 453-481.

Schickore, J. (2017). *About Method: Experimenters, Snake Venom, and the History of Writing Scientifically,* Chicago: The University of Chicago Press.

Skinner, Q. (1969). “Meaning and Understanding in the History of Ideas,” *History and Theory* 8:1, 3-53.

Steinle, F. (2016). *Exploratory Experiments: Ampère, Faraday, and the Origins of Electrodynamics,* Pittsburgh: University of Pittsburgh Press.

Topper, D. R. (2007). *Quirky Sides of Scientists: True Tales of Ingenuity and Error From Physics and Astronomy*, New York: Springer.

Zammito, J. H. (2004). *A Nice Derangement of Epistemes: Post-positivism in the Study of Science from Quine to Latour*. Chicago: University of Chicago Press.