The Past, Present, and Future of Integrated History and Philosophy of Science

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Matteo Vagelli

Introduction

The coming together of history of science and philosophy of science is a far larger and more complex phenomenon than that which, almost exclusively referencing the Anglophone world and the work of Thomas S. Kuhn, goes by the name of the 'historical turn in philosophy of science'.[1] The term 'historical epistemology' instead has come to refer to a wider array of programmes for the combination of history and philosophy of science, ranging from the end of the nineteenth century to the present day.[2] But the heterogeneity of such programmes as well as the relatively recent proliferation of empirical studies grouped under the umbrella of historical epistemology have given rise to a complex and fragmented panorama constituted by what has been seen as a lack of coherence.[3] Indeed, philosophers and historians with very different backgrounds and interests have appealed to 'historical epistemology', especially from the 1990s onwards, and their work has been flanked by questions about the nature, objects and methods implied by historical epistemology.[4] What is remarkable is that the field of historical epistemology, despite its current proliferation, seems permanently haunted by questions relative to its nature, limits and ultimate tasks. Yves Gingras has critically remarked that the current heated discussion about the meaning and use of the expression 'historical epistemology' is a will-o'-the-wisp, a transitory 'brand into the market of ideas'.[5] In his view, 'historical epistemology' is the wrong name for an old programme: the sociology of knowledge, a longstanding historicist programme increasingly taken up since the 1970s by social studies of science proposing what Gingras calls a 'sociological theory of scientific knowledge'.[6]

I believe, in contrast to Gingras, that the discussion of historical epistemology in fact revolves around enduring difficulties in conceptualising the correct or most fruitful interaction between history and philosophy of science. In the next section of this chapter, I trace the recent questioning of historical epistemology back to the enduring and prevalently Anglophone debate over the 'marriage' between history and philosophy of science. I argue in the third section that this renewed interest can in turn be fruitfully situated in a French philosophical context. My main point is that French historical epistemology

(*épistémologie historique*) provides an example of a fully integrated approach between history and philosophy of science – one which, if carefully studied, could bring light to current Anglophone debates. In the fourth and last section of this chapter, I illustrate the distance of *épistémologie historique* from the contextualism and historicism, which, according to Gingras, characterise social studies of knowledge.

5.1 The 'marriage' debate: how can History of Science and Philosophy of Science be combined?

The so-called debate over the 'marriage' between history and philosophy of science arose in the 1960s, picked up momentum at the beginning of the 1990s, and, thanks to renewed interest, has continued on into the twenty-first century.[7] The general problem of whether and how philosophy of science and history of science might be combined contains different sub-problems, ranging from the causes of scientific change and the best way to assess its rationality to the theory-ladenness of historical data and the very nature of philosophical analysis itself. In this section, I limit myself to what I take to be the main difficulty at the core of this debate. In a 2011 article, Jutta Schickore argues that the fatal flaw condemning philosophy and history to be endlessly mismatched is the 'confrontation model', in which the two disciplines are not meshed to form a new approach but assembled as pre-given building blocks.[8] In my view, Schickore's assessment illuminates the two main tendencies within this debate: one assigning philosophy a guiding, normative role over history, the other giving predominance to history and fostering a more descriptive, empirical union between the two domains. The combination of history and philosophy of science has mainly taken place in a twofold manner, which, for the sake of brevity I will name respectively the *a priori-normative* and the *empirical* approaches.[9]

In the first case, a normative role is assigned to philosophy, which is understood as an abstract reflection upon the criteria of inference-drawing, theory formation or of rational theory change. Philosophy of science in this sense does not search for an understanding of what science is (even less for what science has been in the past) but for an explanation of what science ought to be in principle.[10] Hence, philosophy is *a priori* normative and therefore aims to develop its own 'theory of theories' regardless of actual scientific theories. This position is held by Norwood Russell Hanson (1962) and Roland Giere (1973) among others. Finding no strong conceptual rationale for history and philosophy of science, Hanson concludes that history of science is 'irrelevant' to philosophy, whereas Giere believes the relationship between the two disciplines to be a mere 'marriage of convenience', rather than an 'intimate relationship'.[11] Nevertheless, a philosopher can 'take a look' outside of philosophy for instructive examples of inferences actually drawn by scientists in order to adjust his or her generalisations.[12] According to this view, therefore, it is not a history of science that is needed by philosophers,

but a closer look at the actual practices of contemporary science. Accordingly, Giere believes it was not history that made Norwood R. Hanson's, Thomas Kuhn's or Stephen Toulmin's criticisms of, and alternative proposals to, logical empiricism effective, but rather their appeals to real science. The fact that they referred to the science of 'Kepler and Darwin, rather than R. P. Feynman and J. D. Watson may have been incidental.'[13] In Giere's view, the flaw undermining logical empiricists' attempts at rationally explaining science was self-referentiality. This is why he concludes that a philosophy of science that referred to actual scientific practice would be at best 'convenient', because it would be less self-referential.

In the second approach to history of science, history is taken as the *laboratory* where philosophical claims can be tested. Kuhn has for example suggested that the aim of the history of science is to provide examples and evidence for philosophers' generalizations.[14] More particularly, Kuhn hints at a conception of philosophy of science aimed at providing a *theory of science*, one which must be subjected 'to the same scrutiny regularly applied to theories in other fields' and whose method of data collection is borrowed from the sciences themselves.[15] As a consequence, Kuhn could not propose any fully integrative approach between history and philosophy of science; on the contrary, he urged 'history and philosophy of science [to] continue as separate disciplines. What is needed is less likely to be produced by marriage than by active discourse'.[16] Such dialogue between the two disciplines, he argued, should be inter-disciplinary rather than intra-disciplinary. The differences between the history and the philosophy of science, Kuhn maintained, should not be subverted, since no one can practise history and philosophy of science at the same time, but only alternately.[17]

A much more prominent advocate of this methodological naturalisation is Larry Laudan, who advanced the idea that the philosopher of science should use historical cases as data to produce an empirical theory of theory choice.[18] Through a collective effort carried out at the Virginia Polytechnic Institute, Laudan promoted a philosophy of science founded upon the scientific practice of constructing and testing empirical theories. In 2011, well into the debate about the nature and meaning of historical epistemology as a new programme for integrating history and philosophy of science, we find a similar proposal by Philip Kitcher. In line with his advocacy for methodological naturalism, Kitcher deployed the metaphor of the laboratory of the history of science to emphasise the need for philosophy to avoid becoming mere armchair reflection.[19]

Over the years it is the empirical stance that has seemed to prevail, leading to a naturalisation not only of methods but also of its very object of inquiry. Giere, for instance, has recently revised his position by abandoning normativity and adopting a fully naturalised approach. He has come to conceive of philosophy as an empirical and fully naturalised enterprise, advancing theories about science that are liable to be true or false depending on whether they are in accordance or discordance with historical records.[20] In particular, Giere claims philosophy should be naturalised: reduced on the one hand to the cognitive sciences and to

the sociology of science on the other hand.[21] It should be remarked, however, that in both the *a priori* normative and the empirical, descriptive approaches the aim of philosophy is to produce a '*theory* of science', a '*theory* of scientific theories' or a '*theory* of conceptual change'.

One cannot fail to notice that the contemporary use of the term 'historical epistemology' has been accompanied by the same kind of questions that characterised the 'marriage' debate. Lorenz Krüger, the principle inspiration for the creation of the Max Planck Institute in Berlin, was deeply involved in the marriage debate, at least from the end of the 1970s on. In his methodological writings, he fostered a hermeneutic-historicist approach whose main tenet was the idea that the relationship between history and philosophy of science was a 'marriage for the sake of reason'.[22] This continuity between problems and the terms deployed to articulate them is what makes it possible for Gingras to claim that 'recent discussions of the term "historical epistemology"' provide us with 'an interesting example of branding in the field of (Anglo-Saxon) history and philosophy of science'.[23] Feest and Sturm, puzzled by the term 'historical epistemology', moreover have raised the following questions: 'is history necessary for epistemology? Is it useful? If so, in what ways and with what consequences? … How should the relation between philosophy of science and history of science be understood? Is it an intimate relationship, or a marriage of either convenience or reason?'.[24] The persistence of the terms of the 'marriage' debate in the discussion of historical epistemology means that, despite the 'historical drive' of the 1960s, there is still need for integration between history and philosophy of science.

5.2 Épistémologie historique: an *a posteriori* normative approach to the History of Science

It is surprising that contemporary debates about historical epistemology rarely refer to the French tradition of *épistémologie historique* and that, when such references are made, they are brief and unquestioning. In this section, I would like to begin to remedy this neglect by showing, on the one hand, how a comparative study (in this case between the French and Anglophone discussions) is a useful move toward the integration of history and philosophy of science. On the other hand, I will show that the historicisation of epistemology was not a given in the French philosophical context, where the problem of how to combine epistemology and history of science was seriously taken up and actively discussed.

In shifting our attention to the French tradition of historical epistemology, it is important to make a preliminary point concerning the aforementioned 'theoretical attitude' of philosophy of science. The term '*épistémologie historique*' has a natural sense in French, and its English version, 'historical epistemology', does not resonate to the Anglo-Saxon world. This is because the English word 'epistemology' indexes what French philosophy refers to as theory of knowledge, or gnoseology. The French word '*épistémologie*' is instead closer to '*philosophie des*

sciences', the critical study of the principles, hypotheses and results of the diverse sciences, which is interested in determining their logical (and not psychological) origin.[25] Since Auguste Comte, this study has been conceived in France as *necessarily* or *intrinsically* historical, rather than theoretical or methodological.[26]

Qualifying epistemology with 'historical' would therefore be almost redundant within a French philosophical context. Nevertheless, this does not mean, as commentators often surmise, that the implications of the different ways to historicise epistemology were not an issue in this context.[27] On the contrary, as Cristina Chimisso has shown, the question of whether and how to combine philosophy and history – at first meant as the history of philosophy, then also as the history of science and general history – was a major preoccupation for a generation of French intellectuals who gained academic recognition at the turn of the twentieth century.[28] In this period, a naturalising trend with two poles can be detected among the widespread efforts to combine history and philosophy: one side aiming to frame philosophy through all-embracing historical syntheses, the other trying to neutrally draw on historical records to sustain philosophical claims. The former position was predominantly promoted by Henri Berr and his *Centre de synthèse historique*, whereas the latter was held by Émile Bréhier and Leon Brunschvicg. On the one hand, Berr's idea was that every particular history was part of a 'total history', or rather, an historical narrative that would not exclude any kind of material or disciplinary approach. Berr aimed to extend the methods of the natural sciences to the human sciences, thus making history a scientific and objective discipline. In particular, Berr favoured an inductive method that could generalise the results of large collections of data, like bibliographic entries.[29] If in Bréhier's view, on the other hand, history is to the philosopher what the experimental method is to the scientist, Brunschvicg's *a posteriori* study of the mind likewise needed the laboratory of 'history' to observe reason at work.[30]

Chimisso rightly presents Gaston Bachelard's and Georges Canguilhem's *épistémologies* as the height of such attempts to combine history and philosophy. In contrast with the empirical and naturalising trend just described, Bachelard and Canguilhem, while retaining an *a posteriori* approach, produced a *normative turn* in historiography that fundamentally opposed both the ideal of a total history and that of an objective, neutral observation and collection of historical facts and data.[31] In what follows, I will concentrate mainly on Canguilhem, whose historiographical reflection is particularly fitting to the 'marriage' debate – a fact which makes the failed dialogue between the Anglo-American and the Continental traditions all the more unfortunate.

Georges Canguilhem (1904–1995) is a key figure of French *épistémologie historique* – not only was he Michel Foucault's *maître*, but he himself was also the 'heir' of Gaston Bachelard: he took from Bachelard the Chair of History and Philosophy of Science at the Sorbonne and the directorship of the *Institut d'histoire et de philosophie des sciences et des techniques* (IHPST). In his 1966 talk titled 'The object of the history of sciences', presented at a conference in Montreal, Canguilhem challenged the historiography of science. He argued

that by reassessing the relation of philosophy to science, the philosopher could attain privileged access to the history of science, more so than scientists and historians themselves. Canguilhem's paper aimed to better understand the nature and targets of history of science through a sharper understanding of its object. In order to make this point, Canguilhem identified a 'properly *philosophical*' reason to engage the history of science:

> The strictly philosophical reason [to do the history of science] comes from this: without reference to an epistemology, a theory of knowledge would be a meditation on the void, and without relation to a history of sciences an epistemology would be a less important labor which was completely superfluous to the science of which it pretends to speak.[32]

On the one hand, without reference to the history of science, philosophy would be a mere repetition of the sciences, but, on the other hand, without reference to epistemology, history of science would be reduced to mere chronicle. Epistemology and the history of science from this perspective must collaborate and integrate one another's perspectives: epistemology reclaims history in order to sort out the dialectic of conceptual rectification that constitutes science; history of science borrows from epistemology the knowledge-values currently ordering the chronological succession of theories according to intellectual growth.[33] Rather than independent and endlessly recombining, from this perspective history and philosophy of science are internally related. This passage by Canguilhem should therefore be read as the refusal of any confrontational model between history and philosophy of science.

The core argument of Canguilhem's text concerns the very object of history of science. He argues that many of the questions that can be raised with respect to the nature and function of history of science (*who* does the history of science? *where*, in which institutions? *why*, for what reasons?) are all fundamentally linked to this: *what* is history of science the history of? *What* is history of science *about*? It is because this simple question ('what is the object of the history of science?') has been evaded that the philosopher's privileged access to the history of science gets overlooked. The reason that such a question has been evaded or unasked may seem obvious: the object of the history of science is science, just like the object of any science, say crystallography, is crystals, a particular object of inquiry. Yet, Canguilhem suggests that, in the expression 'science *of* crystals', the preposition 'of' does not play the role of a genitive expressing possession (like being the owner *of* a dog). Rather, according to Canguilhem, it indicates science or knowledge *on, upon, about*, crystals.[34] We should therefore ask what does it mean to have knowledge *upon* science? Are we supposed to build knowledge *upon* science in the same way we build knowledge *upon* crystals? A discussion of the *kinds* of objects at play is therefore in order.

In this context, Canguilhem distinguishes three types of objects: natural, scientific, and historical objects. The object of history of science belongs to the last category: it is an object that is intrinsically historical, whereas science is knowledge of a natural object – one that has no history. Crystals, the object of crystallography, might have a history only in the sense that the earth and its minerals have a history, but this history itself is something already there, pre-given to the historian of science.[35] Alternatively, the object of history of science is permanently unachieved, always in the making. Natural objects (e.g. crystals) do not correspond to scientific objects (e.g. to crystals defined in relation to the constancy of facet angles and the regularity of truncation according to systems of symmetries), as these are constituted by science itself and determined by scientific methods. Scientific objects can be said to be second to natural ones, but they do not derive from them. The same goes for the object of the history of science, which is second to the scientific object but is not derived from it. History of science constitutes and carves out its own object, which is, as Canguilhem says, the 'historicity of scientific discourse' itself, in so much as this historicity 'represents the carrying out of an internally law-governed project, but one which is traversed by accidents, retarded or deflected by obstacles, interrupted by crises, i.e. moments of judgment and of truth'. The history of crystallography is therefore a discourse on the dynamic of historical change of discourses on the nature of crystals.[36]

Canguilhem is here reminding us that, if we do not bear these distinctions in mind, there is a constant risk of conflating the object of history of science and the scientific object. This reminder has important consequences for how we conceive the relation between history and philosophy of science. According to Canguilhem there are two fundamentally distinct ways of carrying out the connection between the history and philosophy of science, i.e. two distinct ways in which epistemology and history of science can relate to one another (and this takes us back to the 'marriage' debate): history can be thought of as the *laboratory* of philosophy, or philosophy (and epistemology in particular) can be history's *tribunal*. Canguilhem argued that it is the laboratory-theory, usually associated with an 'experimental theory of the human mind', that has gained the favour of the majority of specialists.[37] But Canguilhem warned that such a conception 'turns back to copy the relation between the history of sciences and the sciences of which it is the history from the relation between that science and the objects of which it is the science'.[38] Extending the experimental relation from science to its history reveals a rather narrow understanding of the functioning of science, since the experimental relation is only one of the possible ways in which science relates to its objects. Behind the extension of the laboratory model from science to the history of science there is the enduring idea that science is reducible to a unique and eternal scientific method 'slumbering at times, vigilant and active in others'.[39] The image of the history of science as a 'mental microscope' that enlarges pre-existing objects is unable to get at the historicity of science, since it only presupposes the 'injection of duration into the exposition of scientific results'.[40] This attitude is only deceptively historical and in fact prevents any access to the historical dimension of science. This is because,

as Canguilhem wrote in 1963, in a text dedicated to the role played by history of science in Bachelard's epistemological works, a 'mental microscope does not distinguish between a difficulty and an obstacle, between delay and wander … A microscope does not judge. A microscope might detect a movement, but it cannot reveal a dialectic'.[41] That is why, in his 1966 talk, Canguilhem argues that it is not a laboratory but a tribunal that is needed to capture and render visible the dialectic of conceptual rectification constituting science:

> In order to understand the function and significance of a history of sciences one can oppose to the model of the laboratory that of the school, or of the tribunal, of an institution and of a place where judgments are brought to bear on the past of knowledge, on the knowledge of the past. But here one needs a judge. It is the epistemologist who is called to furnish history with the principle of judgment by teaching it the most recent language spoken by some science, chemistry for example, and in thus permitting it to retreat into the past, back to the time when this language ceases to be intelligible or translatable into any more loose or more commonplace language which was spoken before.[42]

Canguilhem is here arguing that if one conflates the scientific object with the historical object, one is led to use the metaphor of the laboratory, and hence to construct history of science as itself a science. In order to do away with this wrongheaded association, one has to maintain the specificity of the object of the history of science through the adoption of the alternative historiographical model, that of the tribunal.

Both the metaphor of the tribunal and of the school can be traced back to Bachelard, who taught secondary school classes (*lycée*) before taking his position at the Sorbonne. Bachelard claimed that philosophers should instruct themselves at the school of the sciences and produce a history of science which follows accordingly.[43] In continuity with Bachelard, Canguilhem held that epistemology helps discriminate between lapsed and sanctioned history (*histoire périmée ou sanctionnée*), or rather, between those theories that are recognised as being still part of an actual science and those that are relegated to the repertoire of imaginary, abandoned beliefs.[44] But Canguilhem also advanced an important additional warning concerning the meaning and function of the judgments the epistemologist is demanded to produce:

> A judgment on this matter is neither a purge nor an execution. The history of sciences is not the progress of sciences in reverse, i.e. the putting into perspective of outmoded stages whose truth is today on the point of disappearing. It is an effort to enquire into and give an understanding of the extent to which outmoded notions or attitudes or methods were, in their time, successful; and consequently of the respect in which the outmoded past remains the past of an activity for which it is necessary to retain the term 'scientific'.[45]

Here Canguilhem means that history of science is not written once and for all, but is, on the contrary, always unstable, always in need of rectification. As a result, our scientific past can not only change, but in fact necessarily and continuously changes through the advancement of both scientific production and historiographical reflection.

Before coming back to the 'marriage' debate, I should dwell for a moment on the consequences that this normative stance had on the histories of science that Canguilhem produced. While Bachelard dealt mostly with geometry, physics and chemistry, Canguilhem focused on the medical and the life sciences.[46] The physiological concept of reflex, for instance, was the object of his secondary doctoral thesis, directed by Bachelard. Standard historiography traces the concept of reflex action back to a mechanistic framework and credits Descartes with its discovery. This conclusion shows, according to Canguilhem, a poor understanding of physiology by historians of science. A neural reflex is currently defined in physiology as a spontaneous movement caused by the peripheral neural system, without the involvement of the central system. This distinctive feature cannot be found anywhere in Descartes' texts, and Canguilhem shows that it in fact belongs to the physician Thomas Willis (*De motu musculari*, 1670) and the vitalist and animist framework inspiring his work.[47] Since Descartes and Willis, this concept has undergone three relevant revisions – in clinical medicine, in physiology and in psychology – and any historical account that does not observe these revisions has not been sufficiently instructed by epistemology. A triumphalist history of physiology ascribes the invention of reflex to the mechanist framework, because mechanistic philosophy was, from the nineteenth century onwards, dominant, while the vitalist one was considered lapsed.[48] In this 'objective' account, the element of judgment is disguised or 'sanitised', so to speak.

However brief, this glimpse at Canguilhem's historiographical production shows that his 'normative turn' consisted in the recursive use of an actual norm or value established in current science on the history of that science itself. This particular type of history thus takes the name 'recurrent history'.[49] Bachelard developed the idea of a recurrent history in order to break with teleological narratives – theories growing spontaneously out of everyday experience and flowing spontaneously into another one. For both Bachelard and Canguilhem, the history of science cannot be 'objective' or 'neutral': *épistémologie* is needed to discriminate between what is *périmé* (lapsed) and what is *sanctionné* (approved) in the history of thought.

5.3 Normative, Whig and presentist history

I now turn to show how this 'French' discussion can provide resources for the Anglophone context of the 'marriage debate'. One of the major terms of this debate, as mentioned above, is its sub-problem of the theory-ladenness of historical data. This preoccupation, which cast a negative light over all 'philosophical' uses of history, emerged in particular among advocates of

the empirical approach to bringing together history and philosophy. It concerns the risk of a biased use of history by philosophers seeking raw material as external evidence to prove their theories. This worry is multi-faceted, and it may appear in the literature under the name of 'dilemma of case studies'.[50] Indeed, of the many ways in which philosophy can be seen to be 'altering' history, the risk of anachronism is the most feared – at least since Herbert Butterfield's *The Whig Interpretation of History* (1931). This fear of *biased* history often turns into striving for objectivity. The ideal of a 'sanitised' history is claimed as the only one providing some support to philosophical argumentations. But this ideal, urging philosophers to appeal to history only as an external source to be treated as independent seems to reinforce the 'confrontational model' and hinders real integration between historical and philosophical perspectives.

Hasok Chang has interestingly challenged this view and claimed that, in reality, 'we have never been Whiggish'.[51] Chang distinguishes Whiggism from presentism and triumphalism, three different historiographical attitudes that are usually conflated. He regards the second, presentism, as unavoidable given the fact that the historian has no choice but to be in the present. Whiggism, on the contrary, is a form of presentism underwritten by progressiveness, the idea that the present is superior to the past. Historians' attempts to shun all epistemic judgments in the pursuit of an ideal objective and neutral history is only, according to Chang, 'a judgmental stance disguised as non-judgment'. Indeed, it ends being a form of 'triumphalism', an 'uncritical celebration of anybody who won at the time regardless of whether he was right by today's standards'.[52] For Chang, both internalist historians and sociologists working within the Strong Programme tend to uphold the same ideal of historiographical neutrality. Indeed, the principle of symmetry calls for the value of truth itself to be bracketed, so to speak, so that scientific 'winners' and 'losers' are treated in the same manner.[53]

The normative turn characterising Bachelard's and Canguilhem's approaches could appear to Larry Laudan, but also to Bernard Cohen and many other 'anti-presentist' philosophers and historians of science, as a kind of Whiggish, naively outdated history – a biased reconstruction that relies on current values and organises the past accordingly.[54] As I have just shown, their historiographical approach took the form of a recurrent history in which the epistemologist, through a regulated use of anachronism, imposes on the history of science the norms and values characterising the science contemporary with his or her analysis. But it is in fact this standpoint, I argue, that grants epistemology the role, in Canguilhem's own words, of teaching the historian 'the most recent language spoken by some science'.[55] In this respect, Canguilhem maintained that

> there is a clear difference between retrospective critical evaluation of the scientific past in the light of a present state of knowledge (which is certain, precisely because it is scientific, to be surpassed or rectified in the future) and a systematic, quasi-automatic application to the past of some standard model of scientific theory.[56]

Various attempts at 'naturalising' the history of science, that is, at making history of science as scientific and objective as science itself by using its very methods, would only amount, in Canguilhem's eyes, to slightly different versions of a 'scientific inquisition' of past theories. These histories of science would be 'ideological', in that, as Canguilhem claimed in his 1969 talk titled 'What is a scientific ideology?', history of science can be ideological if it has a false consciousness (*fausse conscience*) of its object: the closer it thinks to be to its object, the more it misses it.[57]

As we have seen, especially in 5.1, the general tendency towards a progressive naturalisation of philosophy of science is propelled by the ambition of 'being closer' to the sciences, both their objects and their methods. In 5.2, we saw how Canguilhem's historical epistemology instead introduces a distance from its object – a distance that is opened by acknowledgement of the 'artificiality' of the object of history of science, which is constructed by the historian. Bachelard and Canguilhem seem therefore to endorse a particular kind of perspectival history, whose distinguishing feature is the establishment of a dynamic relation between the past and the present. In this respect, Canguilhem's and Bachelard's recurrent histories, seen from a larger perspective, instantiate a particular form of presentism, one which avoids triumphalism and above all objective or neutral narratives.[58] As a consequence, what becomes clear is the incompatibility of this historiographical approach with both the principle of context-dependence and that of symmetry between the 'winning' and the 'losing' sides of a scientific dispute, two of the main tenets of both historicism and the sociology of knowledge.

Conclusion

I started with the remark that philosophy was not suddenly historicised at the start of the 1960s. Rather, the oscillation between a normative and a descriptive relation between history and philosophy of science created the situation in which the relationship between the two became unstable. History and philosophy did not merge into a single discipline, like a chemical compound, but remained independent entities that gave birth to a new umbrella term: the hyphenated history-and-philosophy of science. Turning to 'French' epistemology, however, one finds an example of the integration of history-and-philosophy of science in the resolutely normative (but *a posteriori*) approach. Analysis of French historical epistemology enables us to single out three methodological points important for an integrated approach in history and philosophy of science: first, it is not enough for the philosopher of science to get closer to scientific practice but it is the historicity of science itself that he or she should address. As a consequence, the confrontational model dominating the 'marriage' debate is inadequate: history and philosophy of science should be intrinsically integrated with one another from the start. This also means that the laboratory model prevalent in naturalised approaches to history and philosophy of science is wrongheaded: epistemology is normative, and the philosopher plays the role of a courtroom

judge with respect to history of science. These insights, formulated by Canguilhem mainly in the 1960s, seem all the more fitting today in a landscape of increasingly naturalised historiographical approaches and an expanding field of digital humanities promoting quantified, data-driven and distant reading methods. This is why integrated history and philosophy of science cannot do without an integrated history of philosophy of science – one which is comparative and takes into account both Anglo-American and so-called Continental histories and philosophies of science. In this sense, historical epistemology, in both its French and its contemporary Anglophone iterations, should be understood as a dynamic conceptual arena for continued discussion of how we might effectively integrate history and philosophy of science.

Notes

1 See Bird 2008.
2 See Rheinberger 2010. In Rheinberger's account, the 'historicizing of epistemology' is a long-running process borrowing from both the positivism of Ernst Mach and the conventionalism of Henri-Poincaré, the phenomenology of Edmund Husserl and Martin Heidegger as well as the historical-philosophical works of Ian Hacking and Bruno Latour, among others.
3 The creation, under the flag of historical epistemology, of the Max Planck Institute for the History of Science (MPI) in 1994 catalysed this proliferation. Some examples of the diversity of themes involved in historical epistemology include analysis of the birth of the 'modern fact' by the sociologist and historian Mary Poovey (1998), the study of the origins of writing by Peter Damerow (2006), and the study of the emergence of sexuality by A. I. Davidson (2001). On the accusation of incoherence among historical epistemologies, see Gingras 2010, pp. 444–447 and Feest and Sturm 2011, p. 286.
4 In this respect, it is sufficient to look at the most recent international conferences on this subject: "What (good) is Historical Epistemology?" organized by the MPI in Berlin in 2008 (Feest and Sturm 2011) or "What is Historical Epistemology?" and "What Does Historical Epistemology Want?", the respective titles of two discussions held at Columbia University in 2008. For a discussion of the meaning of the term "historical epistemology" in relation to the work being done at the MPI, see Hacking (1999; 2002).
5 Gingras 2010, p. 441.
6 Ibid., p. 447.
7 See for instance Domski and Dickinson (2010) and Laudan and Laudan (2016). Schickore (2011) provides a good digest of publications and events concerning the relation between history and philosophy of science.
8 Schickore (2011). I find Schickore's overall reconstruction of the 'marriage' debate compelling but I tend to disagree on her reading of Canguilhem.
9 I am here partially reformulating Burian (2001), who distinguished between a 'top-down' and a 'bottom-up' kind of relation between history and philosophy of science: in the former, philosophical claims are meant to be tested against empirical evidence; in the latter, generalisations are drawn on the basis of the available historical records.
10 This is one formulation, given by R. Giere (1973), of what is known as the 'is-ought problem'.
11 The reference here is to the title of Giere's 1973 paper 'History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?' and to its conclusion:

'the primary relationships for philosophy of science are with philosophy and science. Likewise, the primary relationships for history of science are with history and science. What they have in common is science. But this common interest is not a sufficient basis for other than a marriage of convenience' (Giere 1973, p. 296).

12 Ibid., p. 290.

13 Ibid., p. 290. This position is not dissimilar from that held by Kuhn, who maintained that philosophy of science should be improved and made less abstract by resorting to history of science in order to bridge the gap between the sciences and provide data and examples. But present science for Kuhn is better than that of the past in terms of being a proximate source of first-hand information about scientific practice (see Kuhn 1977, pp. 5, 7).

14 Kuhn 1977, pp. 5, 7.

15 Kuhn 1962, pp. 8–9: 'How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?'

16 Kuhn 1977, p. 20.

17 Ibid., p. 4. Kuhn used to say that he wore two hats, one as a historian of science and another as a philosopher of science, but, as has been argued by many scholars (see for instance Galison 1981, p. 72), he did not wear them simultaneously.

18 Laudan 1987, p. 24: 'I am suggesting that we conceive rules or maxims as resting on claims about the empirical world [if I do this, I pursue that], claims to be assayed in precisely the same ways in which we test other empirical theories. Methodological rules […] are a part of empirical knowledge, not something wholly different from it'. Cf. Laudan 1989, p. 11.

19 See §3, 'History as the Methodologist's Laboratory', in Kitcher (2011). The paper by Kitcher is part of Feest and Sturm (2011), which illustrates the continuity between the 'marriage' debate and the debate over the sense and use of the term 'historical epistemology' in the Anglophone world.

20 See Giere in Mauskopf and Schmaltz 2012, p. 61: 'I came to the conclusion that philosophy of science should be transformed into something like the theory of science. That is, philosophers should be in the business of constructing a theoretical account of how science works. Philosophical claims about science would then have the status of empirical theories. In short, the philosophy of science should be naturalized. This means, among other things, giving up pretensions to finding autonomous standards for the practice of science'.

21 Giere 1988.

22 Krüger 1982. See also Krüger 2005[1978]. See Klodian-Coko's contribution to the present volume for an example of the hermeneutic-historicist approach in history and philosophy of science.

23 Gingras 2010, p. 441.

24 Feest and Sturm 2011, p. 287.

25 Lalande 1972, pp. 293–294.

26 This is the sense in which Rheinberger uses the term 'epistemology': to indicate a reflection 'on the historical conditions under which, and the means with which, things are made into objects of knowledge. It focuses thus on the process of generating scientific knowledge and the ways in which it is initiated and maintained' (Rheinberger 2010, p. 2).

27 See for example Gingras (2010, p. 442) and Feest and Sturm (2011, p. 288).

28 My main reference in this respect is Chimisso (2003; 2008), who provides the fundamental background needed to account for the emergence of Bachelard's and Canguilhem's reflections.

29 Chimisso 2008, pp. 91–93. Bréhier considered bibliographic entries an example of data that the historians should collect.

30 Bréhier's idea occurs in the unsigned editorial of the first issue of *Revue d'histoire de la philosophie* and is therefore attributable to its director Emile Bréhier (see

Anonymous (1927), quoted in Chimisso 2003, p. 303). For Brunschvicg's take on the issue see L. Brunschvicg *et al.* 'Histoire et Philosophie. Séance du 31 mai 1923', p. 162, quoted in Chimisso 2008, p. 73.

31 For another take on the normative turn of French historical epistemology see the chapter by Simons contained in this volume.

32 Canguilhem 2005, p. 200. This passage can be read as a reformulation of Abel Rey: 'la théorie de la connaissance n'est qu'une idéologie vague ou une dialectique verbale, sans l'histoire philosophique de la science' (Rey 1935, p. XVIII). With these words, Rey meant to convey an aversion for any philosophy of science intended as a theory of science (*Erkenntnistheorie*) or a general methodology of science. To some extent this is a modulation or echo of the Kantian motto ('Thoughts without content are empty, intuitions without concepts, blind'), which Hanson (1962) and Lakatos (1971) later rephrase ('Philosophy of Science without History of Science is empty, History of Science without Philosophy of Science is blind').

33 Canguilhem 1974, p. 66.

34 Canguilhem 2005, pp. 202–203.

35 Ibid.

36 Canguilhem 2005, p. 203. As an instance of such a discourse Canguilhem quotes Hélène Metzger's *La genèse de la science des cristaux* (1918).

37 Ibid., p. 200.

38 Ibid. Particularly striking here is the contrast with Laudan (cf. footnote 18 *infra*).

39 Ibid.

40 Ibid.

41 Canguilhem 1968, p. 180. This translation is mine.

42 Canguilhem 2005, p. 200. Rheinberger, in his historical survey on the primarily twentieth-century process that brought epistemology to hybridise with history, claims that 'Historical epistemology has its own permanent laboratory in the past and future history of the sciences' – thus obscuring the normative metaphor of the tribunal. Rheinberger (2010, p. 66) misinterprets Canguilhem's use of the metaphor of the laboratory, assuming he is subscribing to the position of Eduard J. Dijksterhuis, the Dutch historian of science whose version of the metaphor Canguilhem discusses in his 1966 talk. As we have just shown, Canguilhem reports this image as one of the two possible ways of doing history of science, the other being the tribunal, or court, where the historian plays the role of the judge. It is this latter image that Canguilhem explicitly endorses. Rheinberger's reading has produced some distortions, including Schickore's (Schickore 2011; Arabatzis and Schickore 2012) depiction of Canguilhem as an anti-normative historicist-hermeneutist. This is clearly not the case, since, as we shall see, Canguilhem's historical epistemology is thoroughly normative.

43 See Chimisso 2001, Ch. 3 in which Bachelard's rationalism is read through the teacher-pupil dialectic.

44 Bachelard 1951, p. 25.

45 Canguilhem 2005, p. 201.

46 Chimisso rightly points out that Bachelard's and Canguilhem's respective approaches to the normative turn show remarkable divergences, in part as a consequence of the specific sciences to which they are applied (Chimisso 2003, 2015). For the sake of the argument developed here, I am more interested in the continuities between them than in their particularities.

47 Canguilhem 1955, pp. 60–69.

48 I borrow the idea of a 'triumphalist' history of science from Chang (2009), about which I will say more later.

49 Canguilhem 1955, pp. 166–167.

50 See Hull 1993, Pitt 2001, and Burian 2001.

51 Chang exemplifies his claim with reference to the historiography of chemistry and in particular to the standard accounts of the abandonment of the phlogiston theory and the success of Lavoisier's chemical revolution.
52 Chang 2009, p. 251.
53 Chang 2009, p. 253.
54 For Cohen's anti-presentist worries, see Cohen (1974).
55 See footnote 42 above.
56 Canguilhem 1988, p. 12.
57 Canguilhem 1981, p. 24: 'An ideology is … a knowledge as far from its given object as it thinks itself bound to it. Here ideology would be the misunderstanding of the fact that any knowledge with a critical grasp of its project and its problem knows from the start that it is at some distance away from its operationally constituted object'.
58 On the necessity to reassess presentism in the light of French historical epistemology, see Braunstein 2008 and Loison 2016.

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