Philosophy and science, as well as their respective histories, are not recognized as distinct genres until relatively late in Western philosophy. Even when they are thought to be distinct genres, neither can be written independently of the other, occasional protestations to the contrary notwithstanding. Philosophy and science were seen as almost one and the same activity for most of Western intellectual history, and the description of the relations between the history of philosophy and the philosophy of science not only forms a very large part of any account of philosophy and its history, but must include discussion of the history of science as well. Still, the terms “philosophy,” “history of philosophy,” “history of science,” and “philosophy of science” are not interchangeable because the networks of associated concepts and practices constituting each activity change over the long history of their relations.

One could argue that Aristotle’s criticism of the pre-Socratics in *Metaphysics* is at one and the same time the first history of philosophy, the first history of science, and the first attempt at a philosophy of science. Aristotle does not distinguish *philosophia* from *episteme*, that is, scientific knowledge; indeed, these terms appear side by side in *Metaphysics* at 993b20: “It is right also that philosophy should be called knowledge of the truth.” This knowledge of the truth comes from studying *sophia*, or first philosophy, together with physics and mathematics, but not only from the study of these theoretical sciences. *Philosophia* includes also the pursuit of *phronesis*, or practical wisdom, as well as the knowledge of the “productive sciences” such as poetics and rhetoric. For Aristotle, *episteme* encompasses all of what now goes under the name “philosophy” but it is not the same as what contemporary philosophers of science would count as science. There is, however, at least one respect in which Aristotle’s *Metaphysics* indulges in a practice that seems to be characteristic of the history of philosophy as written by philosophers: Aristotle criticizes his predecessors for not.
grasping the nature of philosophy and science, that is, *episteme*, but in doing so he fails to characterize their work accurately.

The tradition of identifying science with *episteme* in its ancient sense, and *episteme* with philosophy, as encompassing all of what Aristotle would call the theoretical, practical, and productive sciences, persists well into the early modern period. René Descartes’s *Principia Philosophiae* progresses from Part I: The principles of human knowledge, and II: The principles of material things, to Part III: The visible world, and IV: The earth. Descartes had envisaged a Part V, on living things, that is, on animals and plants, and VI, on man. Indeed, he extends this broad scope for philosophy even further when, in the Preface to the French translation of the work, he talks about philosophy being “like a tree whose roots are metaphysics, whose trunk is physics, and whose branches, which issue from this trunk, are all the other sciences. These reduce themselves to three principal ones, namely, medicine, mechanics, and morals.”

In the same work, Descartes, who does not typically indulge in history, engages in some reconstructive history of philosophy in the service of his philosophy of science. In this instance, however, he both attenuates the contrast between his philosophy and that of Aristotle, and accentuates his differences with atomists such as Democritus, presumably in the hope of bringing his Aristotelian readers into his camp. The title to Principles IV, article 200, announces that “there are no principles in this treatise that are not accepted by everyone, so that this philosophy is not new, but is the most ancient and most common of all.” As part of that argument, Descartes claims that he “made use of no principle which has not been approved by Aristotle and by all the other philosophers of every time.” Descartes asserts that he has considered only the figure, motion, and magnitude of each body, and what must follow from their collisions according to the laws of mechanics, as they are confirmed by certain and daily experience. He thus turns Aristotle into a fellow mechanist. Two articles later, he reinforces this revisionist history through a comparison of his principles and those of both Democritus and Aristotle: “That the philosophy of Democritus is not less different from ours than from the vulgar [or Aristotelian philosophy]” (IV, art. 202). Democritus’s atomism is for Descartes very distant from his own philosophy, since he rejects both atoms and the void as absurd or impossible. He shares with Democritus only the endorsement of mechanism, what he calls “the consideration of figure, magnitude and motion.” Therefore, he concludes,

inasmuch as because the consideration of figure, magnitude and motion has been admitted by Aristotle and all others, as well as by Democritus, and as I reject all that the latter has supposed with this one exception, while I reject practically all that has been supposed by the others, it is clear that this method of philosophizing has no more affinity with that of Democritus than with any of the other particular sects.

Aristotle and Descartes are not atypical in their “rational reconstructions” of the philosophical tenets of their predecessors; this activity is repeated many times in the history of Western philosophy. From such philosophers of nature as G. W.
Leibniz and Isaac Newton in the seventeenth century to the nineteenth century scientists–philosophers of science William Whewell and Pierre Duhem, one finds not only remnants of the identification of philosophy and science, but also histories of philosophy constructed to support or reject some particular philosophy. Certainly much more can be said about the views of these and other thinkers forming the background that shapes our present views on the relations between the philosophy of science and history of philosophy. In particular, the debate between the neo-Kantians and the positivists seems to loom large. Immanuel Kant’s Copernican turn, coupled with his division of philosophy into different spheres in accordance with the mental activities involved, preserved the identification of philosophy with science, but only with respect to the grounds of empirical knowledge. The history of science and the history of philosophy were irrelevant to transcendental philosophy and the scientific knowledge it made possible.

An alternative to the ahistoricity of Kant’s transcendental philosophy was provided by the historicism of G. W. F. Hegel and Karl Marx. In both cases, the study of the history of philosophy – and of the history of science – was necessary in order to understand either or both activities. The point of difference was whether ultimately the history of philosophy should be seen as comprised of episodes in the history of mind or the history of matter. The neo-Kantians attempted to capture those aspects of Kant’s philosophy that provided a non-empirical ground for empirical knowledge, by positing a set of logically coherent structures that must govern scientific knowledge, and between which sense experience provided no basis for choice. An alternative conception of history was offered by the positivists, notably Auguste Comte, in which scientific philosophy was the end result of philosophy’s being purified of metaphysics. On the positivist view of history, however, studying the history of philosophy and the history of science was no longer necessary once scientific philosophy emerged.

The end of history

What is, perhaps, most distinctive about the project of modernism in the early part of the twentieth century, at least initially, is its desire not to re-write history, but to repudiate it altogether. The dominant philosophical presence in early twentieth-century philosophy of science – logical positivism – is in its initial formulation explicitly aligned with the modernist project in rejecting the past, reconstructing society, and transforming not just science, art, and philosophy, but culture in all of its manifestations, including education, and architecture. Thus Rudolf Carnap writes in the Preface to his Aufbau (1967 [1928]: xvii–xviii) that he and his comrades feel an inner kinship between the attitude on which our philosophical work is founded and the intellectual attitude which presently manifests itself in entirely different walks of life; we feel this orientation in artistic movements, especially in architecture, in movements that strive for meaningful forms of personal and collective life, of education and of external organization in general. We feel all around us the same basic orientation, the same style of
thinking and doing... Our work is carried on by the faith that this attitude will win the future.

The explicit goal of the logical positivists is to make philosophy rigorous and scientific in a way that it had never been, not even in the neo-Kantianism in which they were educated, a philosophical movement itself dedicated to rescuing science from the excesses of German Idealism. They announce their arrival at “an altogether decisive turning point in philosophy,” from which point onward there would be “no questions which are in principle unanswerable, no problems which are in principle insoluble” (Schlick, in Ayer 1959: 56). This “new, scientific method of philosophizing” consists in the “logical analysis of the statements and concepts of empirical science” (Carnap, in Ayer 1959: 133); hence, the name logical positivism. During the same period that the Vienna Circle (Der Wiener Kreis, another name given to the group) met, Hans Reichenbach led a group of philosophers in Berlin who subscribed to the same ideas, the Berlin Society for Empirical Philosophy. The Berlin group apparently preferred to be known as the “logical empiricists,” but Reichenbach’s name appears among the list of members and sympathizers in an Appendix to Wissenschaftliche Weltauffassung, the manifesto published in 1929 by the Vienna Circle.

Like their philosophical predecessors, the logical positivists see themselves as outstripping previous philosophy in being rigorous and scientific. And like the positivists after whom they take part of their name, the logical positivists do not regard studying the history of philosophy (or the history of science) as necessary for progress in science or philosophy, not even in the interest of showing how logical positivism is superior to previous philosophy (of science), or in locating the origins of their opposition to history. Schlick explicitly contrasts the historian’s and philosopher’s ways of studying the history of philosophy (in Ayer 1959: 43), and Reichenbach states that those “who work in the new philosophy [scientific philosophy] do not look back; their work would not profit for historical considerations” (1951: 325). Not wishing to “belittle the history of philosophy,” he insists, nonetheless, “it is history, and not philosophy” (ibid.). Scientific philosophy “attempts to get away from historicism and to arrive by logical analysis at truths as precise, as elaborate, and as reliable as the results of the science of our time” (ibid.). Its practitioners are “a new class of philosophers” who are “trained in the techniques of the sciences, including mathematics” and are able to concentrate on philosophical analysis (123).

Much of what past philosophers have deemed philosophical – metaphysics, ethics, aesthetics – is, in Carnap’s words, only an “expression of the general attitude of a person towards life (Lebensbewegung, Lebensgefühl)” (in Ayer 1959: 78). Metaphysics, ethics, and aesthetics appear to make meaningful assertions, but these are, in truth, meaningless, for they either cannot be translated into a logically correct form or there are no empirical conditions by which one could determine their truth or falsity. Carnap also lodges this charge at contemporaries in the German philosophical landscape, notably Martin Heidegger. Heidegger, too, saw himself as revolutionary, engaged also in an aufbau of society, but one opposed to the socialist, internationalist, technological, and scientific project of modernism. It is Heidegger’s metaphysical philosophy that is
specifically cited as “eliminable through the logical use of language,” although Carnap sometimes speaks also of the “meaninglessness of all metaphysics” (73). He finds the origins of metaphysics in mythology that bequeaths its heritage partly to poetry, and partly to “theology, which develops mythology into a system” (78). Metaphysics substitutes for theology on the level of systematic conceptual thinking, but further investigation reveals that metaphysics has the same content as mythology, and arises from the need to give expression to a man’s attitude to life, to the environment, to society, to the tasks that he must undertake and to the misfortunes which befall him. Art is an adequate means of expression for such an attitude, but metaphysics is not: “the form of its works it pretends to be something that it is not … a system of statements which are apparently related as premises and conclusion … of a theory” (79). The metaphysician deludes himself not because he “selects language as the medium of expression and declarative sentences as the form of expression; for lyrical poets do the same without succumbing to self-delusion” (ibid.). But lyrical poets know their domain is art and not theory, and the metaphysician thinks he has asserted something when he has “only expressed something, like an artist” (ibid.).

Carnap’s criticisms of the traditional conceptions of the history of philosophy, metaphysics, aesthetics, and ethics, and of the phenomenological tradition of continental European philosophy became standard in the Anglicized, de-politicized, and de-historicized version of logical positivism that emerged after Carnap and other logical positivists left continental Europe for Britain and the United States in the face of impending war. The successful repatriation of logical positivism entailed a deracinating of sorts; the Anglicized version of logical positivism embraced the technological and scientific successes of modernism and disowned its socialist and internationalist ambitions, the meaning of which had changed in the post-war political atmosphere. Post-war logical empiricism neither required nor encouraged any study of the history of philosophy or the history of science; what was required was a sharp distinction between studying philosophy and studying the history of philosophy, including, if not especially, the history of logical positivism. When genealogies of logical positivism do appear, they do not include the philosophy of Kant and the post-Kantians of continental Europe, nor the political and cultural context of German-speaking Europe in which logical positivism was initially formulated. The standard view of logical positivism in the English-language countries is epitomized by A. J. Ayer’s remarks in the editor’s Introduction to *Logical Positivism*. “It is indeed remarkable,” Ayer wrote, “how much of the doctrine that is now thought to be characteristic of logical positivism was already stated, or at least foreshadowed, by Hume” (1959: 4). It is significant that Reichenbach wrote *The Rise of Scientific Philosophy* in English, in 1951 after he and many other Viennese or Berlin positivists achieved a high profile in the philosophical landscape of the English-speaking countries. Reichenbach’s words meant something different in the American philosophical landscape of the 1950s from what they would have meant in Vienna during the days of the Vienna Circle. Ayer’s view was more or less the standard and largely undisputed view of logical positivism until the closing decade or two of the twentieth century when a different and far more interesting story has emerged.
Reichenbach insists that philosophy (of science) be distinguished not only from the history of philosophy but also from science itself. The “professional philosopher of science,” to use Reichenbach’s phrase, is the product of a new and indispensable distribution of work between scientific research and logical analysis. Indeed, logical analysis aims at “clarification rather than discovery” and may even “impede scientific productivity” (1951: 123). Thus does Reichenbach distinguish between the context of discovery and the context of justification, which, in turn, allows for a clear demarcation to be drawn between philosophers, who are concerned with justification, and historians, who, in one way or another, are concerned with discovery. Philosophers and historians can then go on their separate ways without having to consider the other – which they did until the 1960s. Before 1960, there are at least three recognizable and distinct domains – history of science, history of philosophy, and philosophy of science – each with its own perspectives, but in relative harmony with one another. Historians of science and historians of philosophy, although separated by training and professional societies, could still subscribe to a similar intellectualist historiography; Alexandre Koyré, for example, was one of the dominant post-war historians of science who espoused a methodology for the history of science that looked very much like the one practiced by historians of philosophy. At the time a rather unproductive debate was being waged between internal and external history of science.

An anecdote that may provide insight into this debate comes from the 1999 History of Science Society meetings in Pittsburgh. I. B. Cohen gave a paper there entitled, “Context and Construction: Allies of the History of Science Old and New,” in which he related the excitement created by Koyré’s work in the late 1950s and early 1960s, work whose liberating influence was characterized by Cohen as Koyré’s externalism, although Koyré was widely considered to be the arch internalist. However, Cohen’s perspective is informed by the work that preceded Koyré, that is, an inductivism in which philosophical world-views, such as the purported Platonism of Archimedes, are regarded as metaphysical programs external to science and therefore can play no role. From this perspective, what Koyré was advocating was external history. But Koyré, in contrast to historians who would make use of social factors, restricted his historical accounts to intellectual factors, and thus could be seen as advocating only internal history.

Koyré’s approach complemented that of the dominant sociology of science, of Robert Merton and others, which was institutional and large in scale, that is, externalist. While historians of philosophy, like historians of science, usually treated their subject as an intellectual matter divorced from social and cultural considerations – philosophy or science sub specie aeternitatis – historians of philosophy also thought it advisable, if not mandatory, to proceed in a reconstructivist mode. For example, John Austin and Gilbert Ryle argued that the history of philosophy would be of greater use philosophically if it were divorced from its historical contingencies, or detours, a claim Edwin Curley (1986) easily and justly criticizes. As late as 1984, at the founding of the new History of Philosophy Quarterly, the editorial statement could request essays that “cultivate philosophical history in the spirit of philosophia perennis,” historical material that “should be exploited to deal with matters on the agenda of current discussion.”
Such “history” has closer filiations with pre-Koyréan history of science than with the history of science being done at the time of the founding of that journal.

**History recalled**

In the 1960s and 1970s the notion that the history of science, the history of philosophy, and the philosophy of science occupied distinct and independent intellectual realms was subject to a serious challenge, instigated by the publication, in 1962, of Thomas Kuhn’s *The Structure of Scientific Revolutions* (SSR). In its very first sentence Kuhn questions the assumption that the history of philosophy and the history of science are an expendable part of philosophy and science: “History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed.” After Kuhn, philosophers are required once again to study the history of philosophy and the history of science, but the point is not to show that a particular philosophy (of science) is superior to previous ones. Rather, philosophers are required to study the history of philosophy and science in order to understand the very concept of philosophy (of science). History of science, it seems, could be seen as evidence for philosophy of science. In his Preface, Kuhn in fact apologizes for his inability to produce sufficiently broad evidence or suitably wide-ranging historical accounts: “Far more historical evidence is available than I have had space to exploit below... In addition, the view of science to be developed here suggests the potential fruitfulness of a number of new sorts of research, both historical and sociological” (1962: ix).

Kuhn also overtly rejects the distinction between the context of justification and the context of discovery, making room for closer integration – again – between philosophy of science and history of science:

Undoubtedly, some readers will already have wondered whether historical study can possibly effect the sort of conceptual transformation aimed at here. An entire arsenal of dichotomies is available to suggest that it cannot properly do so. History, we too often say, is a purely descriptive discipline. The theses suggested above are, however, often interpretive and sometimes normative... I may even seem to have violated the very influential contemporary distinction between “the context of discovery” and the “context of justification.” (*Ibid.*: 8–9).

But these distinctions, he asserts, are neither elementary logical nor methodological dicta that are prior to the analysis of scientific knowledge. Rather, they seem to Kuhn to be integral parts of a traditional set of substantive answers to the very questions on which they have been deployed:

If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate. How could the history of science fail to be a source...
of phenomena to which theories about knowledge may legitimately be asked to apply?

Kuhn also lays the seeds of a larger debate about the desirability, if not necessity, of an external and social history of science in contrast to an internal and intellectual one. Kuhn sees SSR as extending the positions he wrote about in 1957 in *The Copernican Revolution* (CR), a study of the transformation of the Aristotelian geocentric image of the world to the heliocentric one in the style of Koyré. In SSR Kuhn writes:

Gradually, and often without entirely realizing that they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences... They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. (1962: 3)

The movement in SSR toward social history is accentuated in its 1969 Postscript in which Kuhn declares that a different kind of history might have been more appropriate for the work: “If this book were being rewritten, it would therefore open with a discussion of the community structure of science, a topic that has recently become a significant subject of sociological research and that historians of science are beginning to take seriously” (ibid.: 176). Indeed, he ends by repeating the call for a wider social history: “Having opened this postscript by emphasizing the need to study the community structure of science, I shall close by underscoring the need for similar, and above all, comparative study of the corresponding communities in other fields” (209).

Imre Lakatos puts Kuhn’s conclusions in SSR in stark perspective: “Kuhn’s position concerning the Copernican Revolution changed radically from the essentially internalist simplicism of his [CR] to his radically sociologistic [SSR]” (Lakatos and Zahar, in Lakatos 1978: 177). While Lakatos endorses neither of these historiographical positions, the latter to his mind is clearly the worse: he characterizes it as a view that sees only “irrational change” in the historical details (118, 133). For Lakatos, historical details are neither so simple nor immune from analysis; indeed, he is famous for a “problem shift” with regard to the internal–external distinction (102). The distinction changes depending on the particular relevant historiography: what is external for the inductivist may be internal for the conventionalist (for Lakatos “internalist simplicism” is a genre of conventionalism, in the mode of Pierre Duhem). What is external for the conventionalist may be internal for the methodological falsificationist, and so on. Doubtless, Lakatos is right about the degree of complexity involved, but for the purposes of the present discussion we can restrict the meaning of ‘internal’ and ‘external’ to those Kuhn uses in his 1968 article, cited by Lakatos, “Science: The History of Science.” For Kuhn, “internal history’ is usually defined as intellectual history; ‘external history’ as social history” (1978: 102).

Although there is merit in Lakatos’s criticism, things are even more complex than he allowed. Kuhn’s historiographical stance is not one-dimensional in either of his primary
works, and thus neither of Lakatos’s descriptions fit just right. There are sufficient non-internalist–simplicist accounts in CR for Kuhn to be able to refer back to them in SSR. For example, in its Preface Kuhn apologizes also for having said “nothing about the role of technological advances or of external social, economic, and intellectual conditions in the development of the sciences,” adding that, “one needs, however, to look no further than Copernicus and the calendar to discover that external conditions may help transform a mere anomaly into a source of acute crisis” (1962: x). The footnote to this statement states, “these factors are discussed in [CR], 122–32, 270–1.” Indeed, Kuhn proceeds to use CR as a source for non-internalist historical detail in the body of SSR: when he refers to Copernicus’s Preface to De Revolutionibus as “one of the classic descriptions of a crisis state,” Kuhn cites CR, pp. 135–43 (1962: 69; see also 83). Even when Kuhn argues that Copernicus achieved a scientific revolution in substituting for the old paradigm a new and incommensurable one, he refers to his previous work. In SSR Kuhn claims: “Copernicus’ innovation was not simply to move the earth. Rather, it was a whole new way of regarding the problems of physics and astronomy, one that necessarily changed the meaning of both ‘earth’ and ‘motion.’” The footnote to that statement refers to CR, Chapters 3, 4, and 7, and states that “the extent to which heliocentrism was more than a strictly astronomical issue is a major theme of the entire book” (ibid.: 149–50).

Although it is likely that Kuhn here is reading back his later views into his earlier work, there had to be enough materials in CR to allow him to read it in the fashion of SSR.

While CR is not the internalist–simplicist manifesto that Lakatos alleges, neither is SSR a radically sociologistic tract. What may be overlooked in Kuhn’s apology for not having said anything about the role of technological advances or external social, economic, and intellectual conditions in the development of the sciences is that he also asserts that “explicit consideration of effects like these would not,” he thinks, “modify the main theses developed in [SSR].” Later in SSR (ibid.: 69), when discussing the Copernican crisis, he repeats that

breakdown of the normal puzzle-solving activity is not, of course, the only ingredient of the astronomical crisis that faced Copernicus. An extended treatment would also discuss the social pressure for calendar reform, a pressure that made the puzzle of precession particularly urgent. In addition, a fuller account would consider the medieval criticism of Aristotle, the rise of Neo-Platonism, and other historical elements besides. But technical breakdown would still remain at the core of the crisis.

Thus, even in the seemingly most psychological–sociological element of SSR – that is, in crisis and the emergence of scientific theories – Kuhn is sure that external elements would not modify his conclusions and internal technical matters would be key to grasping the issues.

Yet the issue raised by Lakatos resonates, for Kuhn does seem to invite research in the social history of science and even sociology of science, research that includes traditional methods as well as more novel approaches such as qualitative or internal sociology. Social history of science develops, as does sociology of science; one can
find an excellent exposition of the historical stance of such work, in the Introduction to Steve Shapin and Simon Shaffer’s *Leviathan and the Air-Pump* (1985). There had been other significant developments, of course: Joseph Agassi (1963) argued that the accounts given by historians of science were influenced by their philosophies of science, with inductivists constructing inductivist history of science, conventionalists constructing conventionalist history, and Popperians, Popperian history. Lakatos extended Agassi’s point: “philosophy of science without history of science is empty; history of science without the philosophy of science is blind” (1978: 102). Thus the issue of the relation between history (of science) and philosophy (of science) is raised anew. This can be seen in Larry Laudan’s reflective equilibrium model of history of science with philosophy of science and his attempts at demarcating various kinds of histories (1978: Ch. 5), all of which he rejects in subsequent work. More importantly, history of philosophy finally learned from history of science. As Daniel Garber recounts:

What my generation of historians of philosophy was reacting against was a bundle of practices that characterized the writing of the history of philosophy in the period: the tendency to substitute rational reconstructions of a philosopher’s views for the views themselves … the tendency to treat the philosophical positions as if they were those presented by contemporaries.

The antidote was to adopt the stance previously accepted by history of science; Garber continues: “My own particular heresies in the history of philosophy derived from my acquaintance with the history of science… I began reading more and more in the history of science, trying to link the history of science to the history of philosophy.” And since “[o]ne of the important trends of history of science in the 1980s and 1990s was its interests in the social background to science,” he confesses, “I made some stabs at trying to integrate aspects of these more sociological approaches to my work in the history of philosophy” (2004: 2–4).

At this stage in the 1990s there might have been a different marriage envisioned between social history of science, contextualist history of philosophy, historicist philosophy of science, and internalist sociology of science. But the image of science painted by the sociologists was in the end unacceptable to Kuhn, who had brought history from the exile to which the logical positivists had condemned it. Kuhn’s strongly cognitivist, anti-relativist approach led him to disassociate himself from the conclusions advanced by social studies of science, which had the further consequence that Kuhn, in one stroke, had also distanced himself from much of recent history of science and history of philosophy (1992). Kuhn’s reinterpretation of himself has had defenders, such as Vasso Kindi (2005), who argue that Kuhn was consistent all along in seeking first principles of philosophy of science apart from the history of science: the history of science provides only illustration, not evidence, for the philosophy of science. It remains to be seen whether Kuhn’s last words on the subject will have the same effect on the philosophy of science and the history of philosophy, and their once-ancient and then-recent companion, the history of science, that SSR had in the four decades after its publication.
See also The historical turn in the philosophy of science; Logical empiricism; Scientific method.

References


Further reading

Every paragraph of our essay could be expanded to form an essay on its own. There are many works we could suggest as further reading; we cite here only a few. Harold Cherniss' Aristotle's Criticism of Pre-Socratic Philosophy (Baltimore, MD: Johns Hopkins University Press, 1935) examines Aristotle's treatment of his predecessors and Andrea Falcon's Aristotle and the Science of Nature: Unity without Uniformity (Cambridge: Cambridge University Press, 2005) the relation of Aristotle's philosophy and science. Similarly, for Descartes, Roger Ariew's Descartes among the Scholastics (Leiden: Brill, 2011) discusses Descartes's dealings with his predecessors and Daniel Garber's Descartes Embodied: Reading Cartesian Philosophy through Cartesian Science (Cambridge: Cambridge University Press, 2000) and his Leibniz: Body, Substance, Monad (Oxford: Oxford University Press, 2009), the interactions between Descartes's and Leibniz's philosophy and science. Studies devoted to later episodes in the philosophy of science and history of philosophy include such exemplars as J. Albert Coffa, The Semantic Tradition from Kant to Carnap (Cambridge: Cambridge University Press, 1993), and Michael Friedman, A Parting of the Ways: Carnap, Cassirer, and Heidegger (Chicago: Open Court, 2000). See also the various essays in Ronald N. Giere and Alan W. Richardson (eds), Origins of Logical Empiricism (Minneapolis: University of Minnesota Press, 1996).