What (Good) Is Historical Epistemology?

List of Abstracts

(1) Jutta Schickore (Bloomington): “Experimental Practice in Historical Perspective”

Experimentation has been regarded as an essential part of scientific research at least since the seventeenth century. Opinions may have differed as to the epistemic status of knowledge gained from experiments and its inferential relation to theories, but hardly anyone would have denied that in order to gain knowledge of nature, it was important to make experiments. However, most all experimentalists, past and present, would also agree that experimental practice is precarious and not always successful. Some even published essays on the “unsuccessfulness of experiments” (e.g. Boyle 1661). The history of concerns with the vagaries of experimentation has rarely been studied. This is unfortunate because these concerns are of great interest for the historian and philosopher of scientific method and methodology. The history of these concerns reflects the development of scientists’ concepts of nature, causation, intervention, and instruments.

In my contribution, I examine experiments with snake poison in the seventeenth and eighteenth centuries. A large number of people were involved in these endeavors, major figures like Robert Boyle as well as doctors and apothecaries now long forgotten. They performed vivisections, dissections, and in-vitro experiments with snake poison. One motivation for undertaking such investigations was practical and straightforward: finding an antidote for snake poisoning. But these experiments had far-reaching implications. They were expected to shed light on essential phenomena of life, including the nature of blood and its circulation, the function of nerves, and the mechanism of disease. Reports of these experiments are a treasure trove for the historian and philosopher of experimental methodologies because they are full of remarkable reflections about what may go wrong in an experiment and about how to make it work well. I show that in the late seventeenth and early eighteenth centuries, the most conspicuous methodological notion was repetition, the repeated performance of an experimental trial by one and the same experimenter. But repetition came in very different brands and fulfilled quite different purposes. Moreover, the meaning of repetition changed fundamentally during this period. My paper traces this change and considers the reasons for it.
Historical epistemology (HE for short) has many neighbors and competitors in epistemology, the philosophy of science, and the history of science. I shall compare HE with a variety of the history of epistemology (HoE) as practiced in many philosophy departments nowadays. Disagreement between practitioners of HE and HoE should, as I argue, not arise with respect to goals but more with respect to method. For instance, some historical epistemologists aim to study how epistemic concepts and standards were shaped by actual past scientific practices. Historians of epistemology, in contrast, favor the reconstruction and evaluation of past philosophical arguments. But many defenders of both approaches claim that their historical inquiries might be viewed as enriching current epistemological debates by bringing into play past or forgotten alternatives, thus broadening the spectrum of current options or even revising currently dominant problems within epistemology. Which of the methods is better suited to such goals? The answer lies not in the middle, but a bit more on the side of a philosophical history of epistemology.

As material for discussion, I consider the question of how one should study the history of the relation between perception and judgment. A central epistemological problem here is that while the passivity of perception is necessary for perception to be a window on the world, perception must nevertheless be related to conceptual or judgmental activities to play an epistemic role. Hence, we need to clarify (i) whether any contents of perception are independent of conceptual, judgmental, or inferential activities; and (ii) in case perception involves these kinds of activities, which of these activities are such that perception can still function as a window on the world – and which are such that it cannot. Now, one of the earliest – explicitly so-called – programs for an historical epistemology was developed precisely by reference to the claim that “perception has a history” by Marx W. Wartofsky in the 1970s. He didn’t mean a history of theories of perception, but of perception itself. He claimed that perception has a history because it is part of a complex “feedback loop” in which it causally influences, and is causally influenced by, two basic human activities—communication and production—as well as the resulting artifacts and representations they engender. Artifacts and representations are then tools for further refined action, but only by going through a stage of refined perception; and so on. Moreover, Wartofsky claimed that if one accepts his “feedback loop” model of the history of perception, this would lead to a
change in the nature of the philosophical problems concerning perception (such as the ones mentioned). To this I object, first, that Wartofsky’s view underestimates, if not ignores, the implasticity of perception. Second, Wartofsky did not show how the epistemological problems of perception would be redefined in the light of his historical epistemology. Such worries can be extended to the more recent claims by Daston and Galison that scientific practices shape modes of vision and observation as well.

In contrast, an historical reconstruction employing methods used more often by philosophical historians of epistemology can allow us to see better how and why the concepts of perception and judgment (and the relation between them) have been understood and used differently. The example I will use concerns the 20th-Century debate concerning the Moon illusion that took place between the psychologists Boring & Holway, on the one hand, and Kaufman & Rock, on the other. Here the concepts of perception and judgment, originally made for the epistemological issues mentioned, came to be employed in research where the only goal was the explanation of perception. This lead to a questionable usage of the concepts, which is one of the reasons why the Moon illusion remains unsolved. In this way, historical investigations of debates about the relation between judgment and perception can show that shifts in meaning are due less to varying research practices than to different agendas and arguments. Moreover, the nature of these shifts becomes clear only if a good deal of rational reconstruction and appraisal of the arguments contained in empirical research is undertaken. Such inquiries are nowadays more typical of the history of epistemology than of historical epistemology, but this may be a case where every philosopher will admit that things can change historically, and due to our practices.


I approach the historicity of scientific explanation from the perspective of changing technologies of knowledge and the aphorism that “there is no knowledge without a technology of knowledge.” To capture the ubiquity of the role of technologies, I will begin from work in literary theory on written language as technology (Russell Berman, Fiction sets you free; Walter Ong, Orality and literacy: The technologizing of the word). On this view, writing materializes speech by providing a visual representation of oral expression. Its materiality helps to make it a
technology of distance in space and time, conferring a certain autonomy on the writer and the reader. And this autonomy supports the capacity to create new ways of thinking and acting. In brief, writing is a technology of creative imagination.

Much the same can be said of the technologies that provide the means of systematic exploration and of thought in the sciences. A quite general example from physics, and certainly a technology of creative imagination, is the continually evolving landscape of partial differential equations (PDE’s) and methods for solving them that constituted for nearly two hundred years both the means and the goal of physical explanation. The equations, roughly in chronological order, include: Lagrange's equations, Laplace's equation, diffusion equation, wave equation, Navier-Stokes equation, Hamilton's equation, Maxwell’s equations, Schroedinger equation, Klein-Gordon equation, etc. And the standardized toolboxes of mathematical techniques that every physicist of the 20th century learned to employ in dealing with this array came to be known simply as Courant & Hilbert or Morse & Feshbach. To explain the behavior of a system, one would try to write down a PDE to describe it and then seek solutions under appropriate symmetries and boundary conditions.

PDE’s are still a mainstay of description in many areas of physics but their relation to explanation has changed fundamentally since the 1960s. Their weakness had always been that they were soluble only for relatively simple systems (e.g., the Schroedinger equation for the hydrogen atom with one electron) and became unmanageable for the vast majority of real-world problems (e.g., an atom with ten or more electrons), which typically involve non-linear dynamics. In these areas of complexity, computer simulations, whether beginning from a PDE or not, have taken over the role of explanation. The temporal development of a successful simulation, typically followed visually on screen, now explains the dynamical behavior of the system. The technology of knowledge has changed dramatically and with it has changed both what we can know and how we know it.

Through this example and others more easily recognizable as material or mechanical technologies I will extract some implications for how the historicity of explanation is bound up with the changing technologies on which explanations are based. I would like to call views of this sort “materialized epistemology,” with reference to, for example, Rheinberger’s “epistemic objects,” Galison’s “epistemic machines,” Pamela Smith’s “artisanal epistemology,” and my own mediating machines.

(4) Michael Heidelberger (Tübingen): “Plurality and/or Historicity of Causes”

(no abstract available yet)
Historians and philosophers of science have amply documented the historicity of epistemic practices. Forms of explanation and argumentation, experimental methods, epistemic categories and values have been shown to vary considerably over time. This historical variation has lent itself to the development of historical epistemology, which, in my understanding, attempts to historicize the origin and establishment of knowledge claims. The question I will address in this paper is whether (and in what sense) the objects of those claims are products of contingent historical processes. I will argue that in order to understand the various ways in which those objects are (not) historical we need a pluralist metaphysics that can do justice to the rich diversity of the ontology of science. To illustrate my argument, I will draw upon the early history of electrons qua experimental objects. I will suggest that their experimental history has to be taken into account when we contemplate the question of their existence. The detection and manipulation of electrons in the laboratory, however, are not constitutive of their existence.

Why do some epistemic objects persist despite undergoing serious changes, while others go extinct? Scientists have often been careless in deciding which epistemic objects to preserve and which ones to eliminate; historians and philosophers of science have been on the whole much too unreflective in accepting the scientists’ decisions in this regard. Through a re-examination of oxygen and phlogiston, I will show that there are some benefits to be gained from challenging and disturbing the commonly accepted continuities and discontinuities in the lives of epistemic objects. In more general terms, and also drawing more briefly on some other episodes, I will outline two key consequences of such re-thinking. First, a fresh view on the (dis)continuities in key epistemic objects is apt to lead to informative revisions in recognized periods and trends in the history of science. Second, recognizing sources of continuity leads to a sympathetic view on extinct objects, which in turn problematizes the common monistic tendency in science and philosophy; this epistemological reorientation allows room for more pluralism in scientific practice itself.
According to Karl Polanyi, Aristotle discovered ‘the economy’. We have reason to believe there was one in Athens, or the Mediterranean region, and ever since Moses Finlay (The Ancient Economy, 1973) we have reason to believe it was very extensive and sophisticated. But there was no concept of an economy as an object of inquiry in Aristotle’s texts (notably his Politics and Ethics), nor even in Adam Smith’s Wealth of Nations (1776). Smith never referred to an economy, though he made use of the more traditional term oeconomy either to mean personal frugality or to appropriate the Linnaean sense of the oeconomy of nature. Smith’s analysis of wealth is deeply and directly imbedded in natural processes and the discourse of natural philosophy; there is no appeal to a separate realm of producers and consumers.

Michel Foucault observed correctly that it was only in the early part of the nineteenth century that ‘the economy’ became an object of discourse, although he argued wrongly, in my view, that this came about because wealth was reconceptualized as an ‘object’ after serving for two centuries as a ‘representation’ (Les mots et les choses, 1970). What I will argue here is that part of the conceptual work that enabled David Ricardo and John Stuart Mill to see a realm in which laws of production and distribution operated had much to do with the concept of human agency, and that this became increasingly explicit by the latter part of the nineteenth century. The market phenomena they studied were the result of what John Searle has called ‘collective intentionality’ and bear all the markings of Searle’s institutional facts (self-reflexivity, inscribed regulations, evolution). In sum, they were Durkheimians in the making, seeing features of the economy such as the interest rate as autonomous of individual agency (there were no individual characteristics to attend to as in contemporary economics, e.g. attitudes to risk or time). For some of the leading contributors to economic discourse in the nineteenth century the key phenomena had become objective social facts that could be detached from physical nature and studied as part of a realm known as the economy.

My aim in this paper is twofold. First I will offer evidential support for the above claims, drawing on the history of economic discourse and the literature in the philosophy of the social sciences. ‘The economy’ emerged as an object of reference for economists circa 1820 and endures to the present as an epistemic object. At some point, like the history of the concept temperature, it became part of everyday speech as well. Clearly, the economy can not be perceived directly, though we appeal to ‘leading indicators’ such as the interest rate or rate of
unemployment to talk about its overall performance and efficacy. ‘The economy’ must be constructed out of a conviction in the well-behavedness of certain phenomena in the market place (money, prices, labour supply, etc.). The source of that nometheticity stems from certain distinct commitments to the uniformity of human agency independent of the natural order.

Second, I will argue that discovering ‘the economy’ owed much to the drift toward secularism that so characterized the long eighteenth century. Part of this intersected with the novel speculations of an evolutionary nature that we associate with Buffon or Hutton among others. Economics, in short, was at the vanguard of secular thought and thus tugged the human sciences more generally in the direction of a separate and distinct social realm.

My paper will propose that social facts such as ‘the economy’ are emergent on other branches of knowledge and thus must necessitate some historical account to make sense of this object. The transformation of a science of 'the wealth of nations' to a science of ‘the economy’ required an evolutionary (historical) and secular sensibility that emerged over the course of the eighteenth and nineteenth centuries.

This paper follows up on some of the claims made in my recent book, The Natural Origins of Economics (Chicago 2005) and related work on the history of evolutionary thought and its intersection with economics.

(8) Uljana Feest (Berlin): “Remembering (Short–Term) Memory: The Death of an Epistemic Object?”

In chapter X of his Structure of Scientific Revolutions, Thomas Kuhn suggests that after a paradigm shift, “familiar objects are seen in a different light and are joined by unfamiliar ones as well” (p. 111). This statement is commonly taken to mean that paradigms provide the conceptual structures that allow us to parse up the world in particular ways. While this idea has been especially hotly debated in relation to paradigm shifts, i.e., grand restructurings, the focus of this paper is rather on a more fine-grained question, namely, how taxonomic changes can take place within the context of what we may – for lack of a better phrase – refer to as “normal science”. I will argue that within the context of such normal research the line between the familiar and the unfamiliar is much more fragile and dynamic than the above quote might suggest: phenomena become objects of research precisely because there is an unsettling sense of unfamiliarity associated with them, even when they seem very familiar. At the same time they can become objects of research only insofar as some things about them are
taken for granted. To study the process whereby a phenomenon is investigated empirically, therefore, is to study the productive interplay between scientists’ conceptions of what they know and what they don’t know.

In this paper I will present an analysis of this dynamic relationship in psychological research. I will do so by means of a particular example: shifts in the way that short-term memory is taxonomized. I will argue that the experimental study of memory is guided by specific conceptual presuppositions about the object in question, namely that to have memory is to have the disposition to display behavioral indicators of past experience. This presupposition, in turn, is closely tied to a particular paradigm of investigating the object empirically: by experimentally manipulating research subjects in ways thought to actualize this disposition. In conjunction with other constraints, however, this method has led researchers to the surprising result that some memory phenomena are not really memory phenomena at all.

I will relate my story to the topic of historical epistemology in two ways. First, I will suggest that my account can provide an analysis of Rheinberger’s idea that the “blurriness” of an object is an essential aspect of its knowledge-generating capacity. According to this analysis, we need to distinguish between the notion that an epistemic object is only partially understood and the notion that in order to do any research at all, scientists need to operate with a preliminary concept of the object. Second, I will argue that since preliminary conceptions of epistemic objects are closely tied to norms of experimental research, this opens up a way of reconciling the descriptive aims of a historical account with the normative ones of an epistemological account.

(9) Michael Friedman (Stanford): Extending the Dynamics of Reason: Generalizing a Post–Kuhnian approach to the History and Philosophy of Science

The dynamics of reason is an approach to the history and philosophy of science developed as an essentially historical philosophical response to Kuhn's theory of scientific revolutions. It aims to present a neo-Kantian conception of the distinctive intersubjective rationality of the modern physical sciences by embedding Kuhn’s account of the development of these science from the Copernican revolution to Einsteinian relativity in a wider historical narrative depicting the interplay between these developments and the parallel developments in modern scientific philosophy leading through Kant up to the early twentieth century. This talk discusses two generalizations of my approach: (i) an explanation of the sense in which the neo-Kantian
conception in question represents a drastically historicized version of scientific rationality; (ii) an attempt to extend the historical narrative from purely intellectual to social, technological, and institutional history as well. John Heilbron's recent work on cathedrals as solar observatories furnishes the main example for such an extension.

(10) Peter Barker (Norman): The Cognitive Structure of Scientific Revolutions

Philosophers of science frequently talk about conceptual systems and conceptual structures, and attribute these things to individual scientists or to research communities, however there is little general agreement on the nature of these structures. Recent experimental research in cognitive science has considerably refined the theory of concepts. Drawing upon the results of that research, philosophers can construct more concrete and empirically defensible representations of conceptual systems. I will suggest that this research supports a modest and useful sense of both normal and revolutionary science, not as epistemological continuities or discontinuities, but as particular patterns of conceptual change.

(11) Mary Tiles (Manoa): “Is Historical Epistemology Part of the ‘Modernist Settlement?’”

Bruno Latour, as part of his advocacy of science studies urges us to move beyond what he calls “the modernist settlement” that, among other things, separated science from politics and subject from object. As part of this project he has frequently called for the abolition of epistemology, including quite specifically the historical epistemology/epistemological history of Bachelard and Canguilhem.

Pierre Bourdieu, on the other hand, deploys the resources of historical epistemology, to dismiss Latour's science studies. At issue here are two competing sociological accounts of contemporary scientific knowledge and the conditions of its production. They agree in their concern over the deleterious effects of the current socio-political positioning of science but differ sharply in their analyses. A comparison of their positions may at least serve to help us understand why historical epistemology is assessed both as an obstacle and an asset by different protagonists in the politics of nature.

The label "historical epistemology" has been used to characterise a wide variety of different historical and philosophical projects. One common denominator of several of these projects is that they count Ian Hacking's theory of "styles of (scientific) reasoning" as a key influence or model. This paper will be a critical discussion of Hacking's theory. I shall relate some of my observations to a recent key text in historical epistemology: Lorraine Daston's and Peter Galison's *Objectivity*

(13) Barry Stroud (Berkeley): “The Value of a Historically Oriented Epistemology”

A brief discussion of the ways in which awareness of and sensitivity to the history of philosophy can contribute to epistemology even if epistemology is understood as a distinctively philosophical and not primarily historical enterprise.