

# Chapter 3

## The History and Philosophy of Science History

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Past science is studied from two perspectives. The intellectual history of science, which focuses on the development of ideas and arguments, and the social history of science, which focuses on the development of science as a social undertaking within its broader contexts, are both alive in the academy. Nevertheless, these two approaches do not interact very well, and the field of science history is bifurcated along these lines. Indeed, intellectual and cultural historians of science tend, basically, to ignore one another. They have different training, different aims, different audiences, and often different institutional homes. Intellectual historians of science tend to be conversant with philosophers, social historians of science associate with mainstream historians, but they do not often discourse with each other. In turn, this has led to remarkable naïveté on each side regarding the work of science historians across the disciplinary fence.

This disciplinary divide is signaled by the two dominant “brands” of science history. On the one hand, scholars focusing on social history constitute the majority of “History of Science” (HOS) graduate programs, which are often housed within History departments, where socio-cultural approaches likewise predominate.<sup>1</sup> One can include scholars of “STS” (“Science and Technology in Society” or “Science and Technology Studies”) in this group, though STS comprises sociological studies of science more broadly. On the other hand, scholars of “History and Philosophy of Science” focus on intellectual history of science and are usually aligned with Philosophy departments, at least in practice if not explicitly. There are also many scholars in philosophy departments who examine the intellectual history of philosophy, including natural philosophy. The study of science history thus breaks down into an HOS approach on one side and an HPS approach on the other.

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<sup>1</sup>It is important to note that “History of Science” is *not* identical to the academic study of past science. To avoid confusion, I use ‘science history’ for the latter.

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I should note that I am describing, in overly general terms, *methodological* approaches to science history. Caveats abound. For one thing, the brands I describe here often, but not always, line up with the *institutional* names of departments and programs. HPS-style scholars can be found in “History of Science” departments, and vice versa. For instance, the Science and Technology Studies department at University College London maintains a heavy HPS cast, while the History and Philosophy of Science department at Indiana includes both HOS- and HPS-type scholars. Moreover, my overly general characterization does not capture all scholars working on science history. There are “straight” intellectual historians, for example, who are as skeptical of philosophy as they are of social history. I will try to locate their work in the intellectual landscape below.

The division of science history into two dominant brands has never been comfortable, and what started off as problematic has only gotten worse. There have been several attempts to redress the situation over the years, and a recent spate of conferences has revisited the issue. These meetings have lamented the failure of integration between intellectual and cultural history of science in particular and the history and philosophy of science more generally, though I will argue below that the conflation of these questions is part of the problem. Nevertheless, the conferences and their participants have reflected the strict disciplinary divides they sought to overcome.<sup>2</sup> To speak only of meetings I have attended, the program at the March 2007 conference at Duke, titled “Do Historians and Philosophers of Science Have Anything to Say to Each Other?” and the basis of the present volume, consisted primarily of professional historians of science—HOSers. Meanwhile, the “Conference in Integrated History and Philosophy of Science” at the University of Pittsburgh in October 2007 featured mainly historically-inclined professional philosophers—HPSers. In both cases, there was a remarkable amount of confusion and even disdain regarding the aims and activities of the other camp, which was not sufficiently represented. These conferences illustrate in microcosm the general recognition that the disciplinary boundaries between historically- and philosophically-inclined studies of past science should be broken down. But they also show the level of misunderstanding about where those boundaries lie, and thus the total lack of consensus as to how one should reconcile the two sides of the discipline. There is almost universal agreement that there is a problem, and nearly universal disagreement about what the problem is. HOS and HPS, it seems, have completely lost track of one another, to the point of not knowing where each other are.

I share the opinion that the entrenched distinction between intellectual and social history of science is deleterious to science history. A brief perusal of the history

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<sup>2</sup>Symposia at meetings of the Philosophy of Science Association in 1970 and 1992 are representative. See Ruse (1992), Steinle and Burian (2002). More recent events also include “Do the history of science and the philosophy of science have a future together?”, University College London, June 2006, and “Do the History of Science and the Philosophy of Science Have Anything to Say to Each Other?”, Florida State, March 2008 (which shared no speakers with the similarly titled Duke conference). There have also been two subsequent conferences on “Integrated History and Philosophy of Science” (colloquially known as &HPS) in 2009 (at Notre Dame) and 2010 (at Indiana). Another is scheduled for 2012 in Athens.

and philosophy of the discipline reveals that the distinction is not native to science history. It was imposed from without as science historians sought institutional and intellectual refuge in Philosophy and in mainstream History. These allied disciplines then co-opted science history to their own ends, rending the field. The distinction, moreover, is pernicious since it does not derive from past science. By leading scholars to focus on one aspect of the object of study, the distinction artificially closes off legitimate routes of inquiry. Disciplinary prejudices blind the scholar to important interactions between the intellectual and the social that are clearly and unproblematically present in actual past science. Nevertheless, the distinction persists. It is continually reinforced by the training and practice of science historians.

The situation calls for critical self-reflection. Science historians need to identify the disciplinary boundaries that separate them as a first step toward overcoming them. It will be helpful, in particular, to situate the historical study of science in its historical and philosophical context. By recognizing the intellectual and social bases of their practices, scholars will be able to recognize where they stand in the disciplinary landscape and where others stand in relation to them—how they are separated and how they are continuous. This mapping of the field as a whole should at least engender a discussion that spans the discipline and will perhaps lead to the kind of interaction and cooperation that has been lacking heretofore. Both HOS and HPS approaches are necessary for a proper understanding of past science, and better dialogue between them would make scholarship more effective and more productive. Science historians will need to renegotiate the boundaries of their own discipline in order to integrate the segregated approaches and counteract the divisive, extrinsic demands of Philosophy and History. The divisions between HOS and HPS may be ultimately irreconcilable, but we should at least understand why this is so.

In the interest of the self-criticism I advocate, I admit that I am trained as an intellectual historian of science in the HPS mode, and I bear the prejudices and biases pertaining thereto. What follows will surely be a product of those presuppositions and may therefore strike my audience as odd or ill-founded, especially since I am likely representative of a minority view.<sup>3</sup> If so, I welcome criticism, since it will illuminate and evince defenses of the otherwise tacit prejudices of both sides. The resulting discussion would be precisely the kind of reflective criticism and renegotiation that I aim to promote. From my limited, individual perspective, I cannot foresee the ultimate consensus, if any, that may be reached by the discipline as a whole, and I am not advocating one brand of science history over another. If I am against anything, it is the unreflective adherence to the norms of the HPS and HOS brands that I think is all too common.

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<sup>3</sup>HOS is by far the more common approach. To give one crude measure, of the fifty-nine American graduate programs listed on the History of Science Society's website, only two are separate HPS *departments* (Indiana and Pittsburgh) and only three more are listed as HPS "programs" (Notre Dame, Texas-Austin, and Montana State). Indiana, Pitt, and Notre Dame are excellent, well-respected programs, but Rachel Laudan's assertion that HPS-style historians of science are an "endangered species" still rings true (Laudan 1992).

### 3.1 A History of Science History

Scholarly disciplines are bounded by disciplinary prejudices. One must accept the presuppositions of a field in order to be counted as a member of it. Graduate training is intended to inculcate such assumptions, and the sublimation of foundational questions is the mark of a mature scholar. Of course, disciplinary prejudices are necessary, since the phenomena under scrutiny are simply too hoary to make sense of without them. The rules and methods of the discipline allow the scholar to filter out the subject of their interest from everything else. Disciplines differ because they focus on different aspects of phenomena—they use different biases. By the same token, though, scholars do not often scrutinize the foundational assumptions of their work. This makes it difficult to adjust methods and aims once a discipline has matured and foundational questions have been settled. Scholarly interest is understandably directed outward, not back on the scholar's own practice.

In most cases, disciplinary biases are not problematic, since disciplines tend to either spontaneously grow up around their subjects, cleave themselves off of parent disciplines, or combine the methods of existing disciplines. These developments are organic, occurring in the course of the dialectic between scholars and the phenomena they study. The disciplinary prejudices that emerge are natural, in the sense that they are motivated by and appropriate to the objects of study,<sup>4</sup> generating coherent and productive scholarly programs. However, when the disciplinary prejudices are awkward and uncomfortable, as in science history, one must question them to diagnose and resolve the problem. For instance, we can examine the history of the history of science in order to figure out how science history came to be divided into two distinct parts.

The history of science history has been told before,<sup>5</sup> but it bears repeating, at least in *very* broad outline, since it begins the process of self-reflection that is ultimately necessary to reconcile and reconstitute the field. Making their traditions explicit helps scholars of both HOS and HPS to recognize their activity in relation to that of the other brand. In particular, the history of the discipline and its separate brands partly explains the accidental and artificial nature of the prejudices afflicting its current practice. One finds that what began as an organic discipline came to be co-opted by Philosophy and mainstream History, and therefore became beholden to their extrinsic concerns. The disciplinary prejudices dividing HOS from HPS are, in this sense, artificial, since they did not arise from the history of science itself. The discomfort felt by scholars in the field is, in part, a recognition of this artificiality.

In the Enlightenment, natural science was held as the epitome of human accomplishment. Newton's achievements demonstrated the highest measure of illumination, and all the other disciplines sought to emulate the example. Thus, science

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<sup>4</sup>For a more substantial biological metaphor for science historiography, see Machamer (1994).

<sup>5</sup>For example, Christie (1990), Cohen (1994), Kragh (1987), Nickles (1995).

history of a sort was practiced within various fields by those aiming to establish their disciplines as “sciences” in the style of mechanics. The histories produced were whiggish, since the point was to emphasize the inexorable progress made toward certainty, not the actual development of a discipline, with all its sidetracks and red herrings. Hence the “historical” chapters of science textbooks and the biographies of “great men” typical of eighteenth and nineteenth century science history. Joseph Priestley’s histories of electricity and optics and Charles Darwin’s historical introduction to the *Origin of Species* are tokens of this type.<sup>6</sup> Hence also the work of William Whewell, whose study of the “inductive sciences” was meant to show their progressive consolidation of knowledge.

In its original form, science history was also naturally associated with Philosophy, since the main interest was epistemological: the eventual establishment of sure knowledge. Whewell, for instance, thought of himself as primarily a philosopher, and his *History of the Inductive Sciences* was part of the philosophical project expressed by his *Philosophy of the Inductive Sciences*. The naturalness of this association can also be seen in the strong effects philosophy, especially positivism, had on the natural sciences themselves around the turn of the twentieth century in the work of scientist-philosophers like Mach, Poincaré, Einstein, and Reichenbach.<sup>7</sup> Out of this whiggish, positivist, intrascientific tradition emerged the first professional historians of science, most notably George Sarton and Alexandre Koyré. Sarton, originally trained as a mathematician, lionized Poincaré, whose portrait he put on the frontispiece of the first issue of *Isis* in 1913. Koyré’s graduate work was in philosophy and mathematics, some of which he pursued at Göttingen under Husserl and Hilbert.

Meanwhile, mainstream History had been divided by the effects of nineteenth-century work by Hegel and Marx. Like their Enlightenment predecessors, Hegel and Marx believed in the progress of human history, but they evaluated progress differently. Hegel held it was an essentially intellectual phenomenon; Marx thought it was essentially material. So, for Hegel, the benefit of science was its ability to produce knowledge. For Marx, it was the ability to produce things.<sup>8</sup> The earliest incarnation of science history was naturally linked to the Hegelian approach, since it located progress in the approach toward certainty—i.e., in the intellectual realm. As we shall see, though, it is this initial difference of perspective, endemic to mainstream History, not science history, that laid the groundwork for the divisions that afflict science history today.

In the early twentieth century, the Hegelian and Marxist accounts of human progress became associated with broad political movements. Hegelianism was perverted into nationalism, which stressed allegiance to and the progress of a national *idea*, and from there into fascism. Marxism was folded into socialism and thence communism. The resulting ideological tensions soon came to be reflected in the way

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<sup>6</sup>Christie (1990), Kragh (1987).

<sup>7</sup>Feigl (1970) also makes this argument.

<sup>8</sup>Christie (1990, 12).

science history was studied.<sup>9</sup> The Second International Congress for the History of Science, held in London in 1931, was a pivotal moment. The conference consisted mainly of intellectual historians with nationalist tendencies, but the seven delegates from the Soviet Union made a deep impression arguing the Marxist point of view. For them, science was a socially conditioned human practice, responsive to “external,” non-intellectual factors. Famously, Boris Hessen gave a talk reducing Newton’s science to the material and economic problems of his time, class, and so on. Though this was serious and striking historiography, it was also Soviet propaganda, and Moscow’s embassy in London furiously translated and published the delegation’s papers as *Science at the Cross Roads* within ten days.<sup>10</sup> Sympathetic scholars, like J. D. Bernal, Joseph Needham, and Edgar Zilsel, were converted to the Soviets’ approach, and the event marked the beginning of the widely recognized “externalist” study of science history. The traditional Hegelian “internalism” continued to prevail, however, and externalism remained relatively marginal. Nevertheless, the Hegelian-Marxist split had injected itself into science history, partly as a result of global politics.

In mainstream History, Marxism eventually penetrated much deeper, almost to the complete exclusion of intellectual approaches, which were driven off into other fields, such as political science. This was especially true after the rise and ultimate self-immolation of fascism up through World War II and the holocaust, which tainted Hegelian-style intellectual history with vapors of totalitarianism and moral turpitude. Even today, intellectual historians in general struggle to find a place in History departments, where they are seen as conservative, old-fashioned, and vaguely sinister. Science history, however, was a special case. The field proved resistant to the general historiographical trend, precisely because science itself, insofar as it makes claims to rationality and truth, resists complete reduction to material or social considerations such as economy, race, class, and gender.<sup>11</sup>

Such resistance was a mixed blessing. On the one hand, science history was left outside the practical and institutional pale of mainstream historical studies and thus relegated to minority, outsider status. On the other hand, intellectual historians of science found a welcoming reception by the Western conservative establishment after the war, which sought to use science history to explicitly counter the Marxism infiltrating the humanities. Science history was also seen as a way of encouraging student interest in science, and thus formed an essential part of a Cold War curriculum designed to promote the American scientific excellence that would overcome the Soviets. The notion of a Scientific Revolution, coined and popularized by Koyré, was also useful in demonstrating a kind of revolution that did not involve violence or, for that matter, the overthrow of a capitalist system. Moreover, science

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<sup>9</sup>As one might expect from their intellectual predilections, Sarton and Koyré had right-wing political inclinations. They were nationalists and anti-Marxist. Koyré, a white Russian who emigrated during the revolution, was particularly fervent in this respect.

<sup>10</sup>Cohen (1994), Young (1990, 80–84).

<sup>11</sup>Laudan (1990, esp. 51).

history fit with the broader cultural excitement about the power and profits of science and technology.<sup>12</sup> Altogether, then, these intellectual and cultural factors led to the first substantial institutionalization of science history. History of science programs (sometimes entitled “History and Philosophy of Science,” reflecting the involvement of philosophers, though not yet representative of the HPS mode) were set up, usually by science faculties, at Harvard, Princeton, Columbia, Oxford, Cambridge, Leeds, Sydney, Melbourne, and elsewhere.<sup>13</sup> Thus, science history found an institutional home, but it was a home apart from and in opposition to mainstream History. This Hegelian, intellectual tradition of science history remained dominant throughout the 1950s and early 1960s. The “internalist” stalwarts successfully defended the discipline from a few “externalist” critiques, which were dismissed as “a bit Marxist.”<sup>14</sup>

Everything changed with the publication of Thomas Kuhn’s *Structure of Scientific Revolutions* in 1962. Kuhn himself was a typical, scientifically-trained, philosophically-inclined, intellectual historian of science. In *Structure*, Kuhn tried to give an intellectual account of scientific change, but his argument located scientific knowledge in the “paradigm”—a communal entity consisting of shared concepts, practices, problems, and specialized languages. In a move Kuhn neither foresaw nor approved of, *Structure* seemed to warrant a reduction of science to a socio-culturally constituted paradigm.<sup>15</sup> Kuhn’s theory also raised the specter of radical incommensurability, which threatened the very notion of scientific objectivity, especially in the hands of Kuhn’s Berkeley colleague, the philosopher Paul Feyerabend. Hence, *Structure* weakened science history’s traditional defense against socio-cultural materialist reductions, the appeal to the fundamental rationality and objectivity of science itself. Soon enough, Marxist-style historians had taken over and assimilated science history into History departments proper. In some ways, this was quite welcome, since science history could now call on the resources of mainstream History. “Conservative” intellectual historians, however, were once again driven out of the “History of Science” as practiced in the universities.

Besides its Kuhnian justification, the turn toward a more sociological approach to science history found political motivation in the New Left.<sup>16</sup> Older Marxists

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<sup>12</sup>Mayer (1999).

<sup>13</sup>There were precursors. At University College London, a Department of History and Method of Science was founded in 1921–1922 and renamed History and Philosophy of Science in 1938. George Sarton had helped establish the Harvard Committee on Higher Degrees in the History of Science and Learning in the 1920s, and Harvard began granting PhDs in History of Science in 1936 (Bennett 1997; Cohen 1984; Hall 1984; Kuhn 1984; Smeaton 1997).

<sup>14</sup>Henry Guerlac reported this as a colleague’s response to his own work (Guerlac 1977, 36). Cohen speculates that the colleague was Koyré (Cohen 1994, 561n168). More damaging to “internalism” were critiques from other intellectual historians, like Frances Yates, who challenged the presumption of a well-defined rational “science” apart from other cognitive, but “irrational,” activities, such as magic (Hesse 1970; Turner 1990).

<sup>15</sup>Whether Kuhn’s work actually warrants this move is still a point of vigorous debate.

<sup>16</sup>Porter (1990, 41).

had thought—in the Enlightenment style—that science was on their side. Science would provide the material culture needed for the post-revolutionary utopia. By the 1950s and 1960s, though, science had become part of the conservative, capitalistic establishment. In the view of the New Left, science was used to impose social norms and was therefore anti-democratic. Of course, this view was not entirely mistaken. The early to mid-twentieth century enthusiasm for race hygiene and eugenics is only one case in point. The social historians also had more recent examples of science's sometimes objectionable role in the military-industrial complex, such as nuclear weapons, MIT's Draper Laboratory, the Tuskegee Experiments, DDT, and so on. In order to undermine the establishment, they had to disparage science and reduce its social role. This made social historians, to some degree, anti-science. They sought to emphasize the social mechanisms by which scientists come to accept beliefs, and they downplayed the significance of the "objective" features of science, including its intellectual content and its predictive and explanatory power. Here, the social historians found allied interests amongst sociologists of science, descended from the work of Robert K. Merton. Merton's study of scientific values and institutions fit well with the study of paradigms, since the former help determine the latter.<sup>17</sup>

The conquest was as complete as it was sudden. In the 1950s, the internalists held the upper hand. By 1968, the so-called "internalism-externalism debate" had been declared over. The end of the debate did not represent a compromise so much as a sound defeat of the internalists, who ceded the field and the brand name to the externalist approach. Programs in "History of Science" were converted to the externalist approach, or even into STS programs outright. That the "internalism-externalism debate" is now outdated (and that it is gauche to revisit the issue) is evidence of total victory—there are simply too few internalists left in "History of Science" to make a stand. In fact, the very meaning of "internalism" itself has changed. In current usage, it often signifies the socio-cultural interactions *within* science, as opposed to science's interactions with the wider socio-cultural sphere. But even this would have been considered "external" on the earlier meaning of 'internalism,' which referred to a historiographical approach focused on the internal logic of scientific progress.<sup>18</sup> Internalism in this older sense has been practically effaced from "History of Science" as it is presently conducted. We should not confuse the practical victory of the Marxist-style externalists for rational propriety, though. The state of play does not mean the distinction is or ever was dissolved. The internalists have simply moved on.

Though no longer considered part of "History of Science" proper, intellectual science history found a ready reception amongst philosophers. Since the 1920s, the

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<sup>17</sup>The alliance was somewhat ironic, though, since Merton himself was fiercely anti-Marxist and intended his work to show, like his predecessor Weber's, the essential importance of Western, capitalistic, and individualistic values for the development of science. Merton claimed that "external" factors could affect the rate and not the course of scientific development, which was determined only by its internal logic, but later Marxist-style historians claimed him for their purposes (Shapin 1992, 336–37; Young 1990, 83).

<sup>18</sup>Compare, for example, Cohen (1994) or Hesse (1970) to Shapin (1992).

logical positivists and, later, the analytic philosophers descended from them, had strived to protect their discipline from the same sort of political impositions that had injected themselves into History. The early logical positivists of the Vienna Circle of the 1930s, for instance, were especially concerned to show that Philosophy could achieve transcendent validity and value. Recalling Enlightenment attitudes and inspired by recent mathematical and scientific achievements (e.g., Einstein's relativity theory and the formalization of mathematics by Russell and Whitehead), these philosophers held science to be a model of the transcendent objectivity they sought for themselves. They tried to turn philosophy into science, eschewing non-empirical "metaphysics," on the one hand and seeking out the methods that make a discipline scientific, on the other.

At first, this program was pursued "logically"—through a priori reasoning. By the middle of the century, though, it was accepted that the so-called linguistic turn had failed. Famously, philosophers could not even establish criteria by which "science" could be distinguished from metaphysics or pseudoscience. Kuhn and Feyerabend, along with Imre Lakatos, Norwood Russell Hanson, and Stephen Toulmin, were part of a generation of philosophers who sought to base their analyses of science on its actual past and present practice. In their work, Hegelian-style intellectual history of science offered both raw data and proving ground for philosophical models of rationality. Their calls for empirical studies of science led to a cooperation with the holdouts of the "right wing" of science history, which led to the establishment or reinvention of a handful of HPS departments and programs across the globe around 1970, thereby initiating the modern HPS brand. It must be noted, however, that these historically-interested philosophers primarily sought to use history *for* philosophical insights, but denied that history per se contained anything of philosophical significance. Even Lakatos, one of the most vocal supporters of the cooperation of Philosophy and science history, advocated "rational reconstructions" of science—science as it "should have happened"—and relegated the reporting of historical facts to footnotes.<sup>19</sup>

After Kuhn, science history has developed along the two distinct paths that separate the discipline today. In HOS/STS, the first sustained movement was Social Constructivism (or "Social Structure of Knowledge"), which first flourished in the 1970s with work by David Bloor and Barry Barnes. The social constructivists took the reduction of science to social mechanisms to a radical extreme (following a similar development in mainstream History), arguing that scientific knowledge is entirely constituted by social interactions.<sup>20</sup> Social constructivism itself has lost much of its impetus, but similar trends remain powerful amongst science historians of the HOS variety. Foucault, whose views are widely influential, emphasized the relations between power and "knowledge"—including scientific knowledge. Another dominant tendency, partially inspired by a move towards narrative in

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<sup>19</sup>Hanson (1962), Lakatos (1971). See also Kuhn (1977).

<sup>20</sup>In fact, Barry Barnes has argued that the historian is best served if she is completely ignorant of the beliefs held by the scientists she studies (Barnes 1990, 71).

mainstream History and exemplified by Shapin and Schaffer, is to study scientific practice in very particular geo-temporal contexts, to the exclusion of transcendent considerations.

The point of this brief history is that science history resisted the general trend of History during the twentieth century, since it was traditionally and constitutionally aligned with the Hegelian-style intellectual history falling out of favor in light of world events. The work of Kuhn and its surrounding counter-culture atmosphere then undermined the very features of science that science historians had used to organically develop and defend their own scholarship. Never very large and still relatively young, the field was cut intellectually and institutionally adrift, open to the imprecations of more established disciplines, particularly History and Philosophy, that co-opted science history to their own purposes. This has led to a bifurcated field, resting uncomfortably between History and Philosophy, where different practitioners identify more with their institutional peers than with each other.

### 3.2 Philosophy of Science History

The situation would be unproblematic if science history's cognate disciplines could cooperate. Science history would then be a natural overlap, in the way that, say, biochemistry bridges biology and chemistry. Painting with a broad brush, the trouble is that mainstream History and Philosophy are inherently at odds with one another. While both disciplines have legitimate affinities and interests in science history, they end up exerting centrifugal forces that artificially pull the discipline further apart.

Especially in the English-speaking world, Philosophy sees itself as a normative discipline in that it, broadly speaking, studies epistemic norms and examines arguments in order to figure out how beliefs should lead to each other. The aim is to understand the nature of human reasoning (of all kinds).<sup>21</sup> Science has a remarkable power to produce convincing claims about the world. Philosophers are intrigued by this argumentative power, and they seek to isolate the forms of argument—the rational method—by which science achieves its epistemic efficacy. Science history, therefore, provides a store of arguments with which philosophers can construct and evaluate their models of rational behavior. They look to science history as a way to determine which epistemologies “work” and which do not.

History, on the other hand, sees itself as a descriptive discipline. The aim is to get as close as possible to past events and figure out the conditions and motors of human activity. Science is interesting because of its importance amongst human practices, especially in the Western Tradition, where scientific activities, products,

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<sup>21</sup> A simple description of Philosophy is difficult, and exceptions to my two-sentence definition are common. Still, I think this view of the general slant of the field holds. For instance, this characterization captures the many philosophers who see themselves as merely describing (as opposed to prescribing) norms, as in naturalized epistemology.

and values are central. Science has a remarkable power to affect the conditions of human existence, and historians seek the sources and effects of this power.

These disciplinary self-conceptions rest, in turn, on antithetical presuppositions. Philosophers (for the most part) presume that there is an absolute measure of rational conviction against which different epistemological methods can be measured, regardless of socio-cultural context. A good argument, most philosophers would contend, justifies its conclusion independently of the particular material and social conditions under which it is formulated. In philosophy, therefore, past arguments, including scientific ones, are shorn of any non-cognitive or even non-rational context. It makes perfect philosophical sense, for instance, to compare the views of two authors widely separated in time or to ask for an earlier author's possible response to a later author (e.g., how would Descartes respond to Newton?).<sup>22</sup>

Historians dismiss all of this as a "positivist" delusion. The assumption of a transcendental measure of argumentative "success" is unfounded. The failure of the demarcation project itself shows that one cannot decide a priori what counts as "rational" or "scientific." Arguments are always conditioned by their social and material context. Their power to engender belief depends, in part, on who makes them and to whom they are made.<sup>23</sup> Even the basic determinations "science" and "rational" must be contextualized in a particular time and place; the more detailed the contextualization, the better. In particular, there is no reason whatsoever to exclude non-cognitive factors as irrelevant. (Indeed, given the old Marxist prejudice against intellectualism, historians are likely to emphasize non-cognitive factors over cognitive ones.) The philosophers' preoccupation with the intellectual realm threatens to anachronistically distort the historical account. The philosophers' pursuit of rationality, says the historian, fails to say anything definitive about the essentially contextualized human condition. Hence, the philosophical study of science is ill-founded and uninteresting.<sup>24</sup>

To philosophers, meanwhile, historians commit a logical fallacy of their own by presuming the failure to describe transcendental rational norms entails that such norms do not exist. Historians, therefore, adopt a philosophical position for which no sustained argument is offered. It is impossible to study anything, history included, without some a priori framework by which phenomena are made meaningful. All

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<sup>22</sup>A partial exception can be made for feminist philosophy and related studies of "science and values." These areas do acknowledge the effects of social values on reasoning and science, though the morals drawn are often still normative: how reasoning and science *should* respond to external values.

<sup>23</sup>See Laudan (1990), Shea (1983), Thackray (1970a).

<sup>24</sup>To be fair, philosophers of science themselves have recognized the failure of the universalizing project. As a result, they have turned to more specific studies of particular disciplines (viz. the philosophy of physics, the philosophy of biology, and so on). While this sort of philosophy of science remains preoccupied with the normative and transcendent features of science, it is more sensitive to specifics of practice and argument. On the other hand, historians are generally unaware of these philosophical developments, so their conception of "philosophy of science" is often an outdated caricature—which is all the more reason to encourage dialogue.

observations are theory-laden, and it is the historians who are overly “positivist”<sup>25</sup> in their insistence that anachronism can and must be avoided.<sup>26</sup> Philosophers also see no profit in complete contextualization and they are not interested in antiquarian description for description’s sake. They want the payoff for their own, always present-day, essentially rational selves. Hence, the historical study of science is ill-founded and uninteresting.

Of course, both sides are, basically, right.<sup>27</sup> Within their respective disciplines, though, historians and philosophers are entitled (and expected) to ignore such criticisms and hold up their basic presuppositions as regulative ideals around which to organize their inquiry. After all, these prejudices grew up organically within those disciplines, and they effectively shape scholarly discourse into something manageable. History is meant to study the conditions of human experience, Philosophy is meant to study human reason, and both perform their functions well. However, the science historians now operating in the midst of these disciplines are forced to make an impossible choice. To be an accepted scholar within one discipline or the other, they must either place an inordinate focus on the cognitive and universal aspects of science and minimize the contingent and contextual, or they must contextualize away the very universal claims that give science its special socio-cultural status and mark it out as a distinct human activity. In other words, science historians face the “barren antithesis”<sup>28</sup> of studying science without history or history without science. Both approaches are risible. They completely vitiate the intensions of the scholarship. HPSers are left with a gross misunderstanding of scientific reasoning, and HOSers fail to recognize the primary motor of scientific activity. Still, far too many authors capitulate. One is frequently frustrated by attempts to make sense of a scientific episode without any reference to its historical context or to describe a historical context without making any sense of the science it surrounds. As Larry Laudan has put the point: “Many have evidently concluded that the only alternative to the disembodied history of scientific ideas is a lobotomized history of scientific institutions.”<sup>29</sup>

The other distinctive features of HOS- and HPS-style scholarship derive from the prejudices and practices of History and Philosophy. In *very* broad generalization, science historians of the HOS stripe today tend to be trained as historians, not as scientists, which is to say they tend to be socio-cultural historians like their mainstream History peers.<sup>30</sup> They lean toward a deflationary view of science as just another human activity without any universal pretensions. The heartland of HOS/STS is

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<sup>25</sup>The overlapping but different connotations of this pejorative further signify the incommensurability between the philosophical and historical worldviews.

<sup>26</sup>Baltas (1994), Burian (2003), Nickles (1995, esp. 151–55).

<sup>27</sup>See, for further discussion Burian (1977), McMullin (1970).

<sup>28</sup>Thackray (1970b).

<sup>29</sup>Laudan (1990).

<sup>30</sup>HOSers, in my experience, sometimes excuse their focus on socio-cultural factors by pleading ignorance about the actual workings of science, which forms a sort of “black box” at the center of the institutions and activities they study. However, this does not excuse the general lack of interest

post-war twentieth-century science, where funding structures, collaborations, and technological outputs exemplify the socio-cultural entanglements of the scientific enterprise.<sup>31</sup> Socio-cultural historians also favor “thick” explanations that include all relevant causes, including the “little losers” of science—those whose contribution was minor or even completely forgotten. Hence, HOSers, like their mainstream historian peers, favor books as the measure of scholarly contributions. These tend to be written for a non-scientific audience, and HOSers often find themselves pitted against scientists in the culture wars, since scientists see the relativism embodied in their contextual approach as a threat.<sup>32</sup>

On the other side, HPS has been largely co-opted by the disciplinary concerns of Philosophy more generally. Hence, intellectual historians of science today tend to be trained as philosophers, and to share the anti-historical prejudices of that discipline. They seek to construct transcendent models of scientific reasoning and anachronistically ignore the contingent, “irrational” factors in the development of science. HPSers focus on periods in which intellectual progress is most on display and the intellectual contributions of individuals is clearest. Thus, they concentrate on the scientific revolution and turn-of-the-twentieth-century physics. For the same reason, they also tend to focus on the “big winners” of science whose work was the most influential or “successful.” Also, HPSers often write only for the benefit of their philosophical peers or interested scientists, without trying to reach a general audience. They focus on particular epistemological issues and write papers. And so on.<sup>33</sup>

To me, all these divergent tendencies in a discipline that is ostensibly about one thing—past science—are evidence that the disciplinary biases by which History and Philosophy distinguish themselves do not answer to any clear distinctions in past science itself.<sup>34</sup> The thing we call “science” lies at the intersection of ideas and society. It is a complex set of human practices that occur in a social,

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in *trying* to understand, even to a small degree, scientific reasoning. Such interest, by contrast, is expected amongst intellectual historians and philosophers of science.

<sup>31</sup>I should note that HOS-type historians, following Shapin and Schaffer, have recently applied socio-cultural historiography to the scientific revolution. Westman and Biagioli, for instance, apply institutional analysis to the work of Galileo. The burgeoning interest in the history of renaissance and early-modern alchemy, astrology, and magic represents a similar trend. Though these were originally treated quite intellectually in the hands of Yates, Dobbs, Newman, Principe, and others, the *pseudo*-scientific status of these activities has allowed more recent authors to downplay the intellectual content of historical episodes. Also, the field of readership studies in mainstream History has allowed HOSers to focus on the conveyance of ideas, rather than ideas themselves. These trends might be considered an offensive maneuver, since they threaten to expel intellectual science historians from areas where they have hitherto enjoyed preeminence. On the other hand, these trends might be an opportunity for increased dialogue and cooperation. I fear the former and hope for the latter.

<sup>32</sup>Turner (1990).

<sup>33</sup>In a keynote paper at the &HPS1 conference, Peter Machamer listed over twenty such distinctions separating philosophy and history of science. See also Richards (1992).

<sup>34</sup>I do not mean to prejudge the issue as to whether there is any well-defined thing called “science.” For the time being, let the term signify an actors’ category—“science” is whatever is called

cultural, and material context. It has its institutions, its resources, its products, its traditions, its communities, its power relations, its values, and so on. But almost uniquely amongst human activities, science is also an argumentative discipline that makes claims to transcendent, objective truth. The scientific enterprise is a wonderfully efficient producer of accepted truths about the world. Science, in itself, has always chugged along quite happily both as intellectual endeavor with pretensions to universality and as socially conditioned cooperative enterprise. Scientists respond to a wide range of influences, some obviously cognitive, some obviously socio-cultural or material, and many in between. Moreover, they are unconcerned with Cartesian distinctions between their mental and physico-socio-cultural states or Reichenbachian distinctions between the “context of discovery” and the “context of justification.” It might be impossible for them or anyone else to say *how* cognitive and social factors interact (they may even deny such interaction), but it is clear *that* they do, just as it is clear that scientists operate without separating epistemic contexts. It is therefore possible and, indeed, necessary to study all these aspects of scientific activity in conjunction, without privileging one or the other, according to the peculiar demands of the scientific enterprise itself. Importing the prejudices of History and Philosophy into science history has caused science historians to ignore fruitful paths of inquiry and artificially constrained their narratives. Science history is pulled to the extremes when it should naturally seek the middle.

For all of the forgoing reasons, I am deeply pessimistic about attempts to “marry” or “integrate” the history and philosophy of science.<sup>35</sup> I am also, with Steven Shapin, suspicious of eclectic historiography of science that aims to be part internalist and part externalist.<sup>36</sup> The broader disciplines are simply too much at odds with one another. Though they may occasionally have something to say to one another, they will mainly turn their backs on one another. For the most part, they already have.<sup>37</sup> I am more optimistic about the possibility of a unified science history, though the two brands cannot be reconciled as long as they remain beholden to the conflicting prejudices and practices of the cognate disciplines in which they are ensconced. Philosophy is too anti-historical and History is too anti-intellectual for HOS and HPS as presently constituted to meet on common ground. Science history must reconcile its distinctions on its own terms, as an integrated unity with

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‘science’. I suspect that there is more to it than that, but this is one of the many questions I seek to open for discussion.

<sup>35</sup>See Feyerabend (1970), Kuhn (1977), McMullin (1970). I am more optimistic about the program recently expounded by Mary Domski and Michael Dickson (following Michael Friedman), which calls for a “reinvigoration” of a pre-Kuhnian (actually, Cassirer-ian) “synthetic approach” (Domski and Dickson 2010).

<sup>36</sup>Shapin (1992). See Nickles (1995), Steinle and Burian (2002) for such suggestions.

<sup>37</sup>Historians almost never reach out to philosophers. Philosophers are a little more circumspect, but proposals for dialogue with History usually amount to calls for the culling of History for philosophical ends, not a sincere interest in *doing* responsible history. See, e.g., Burian (1977), Hull (1992).

its own disciplinary bounds. Science historians should loosen the bonds of History and Philosophy and make their discipline anew, in dialogue with past science itself.<sup>38</sup>

### 3.3 Mapping the Field

One way to begin the unification of science history is to attempt to map the field as it now stands. This will allow us to formalize many of the distinctions between different kinds of science history, as well as encourage reflection, discussion, and discourse by allowing scholars to locate and defend their positions in relation to one another. If we seek to set up a big tent, a map will help us figure out where to pitch it, or at least have a coherent argument about where to pitch it.

As a start, I propose a representation of the historiography of science along two axes suggested by the historical and philosophical discussion above. There are many other ways of representing the field, but I think these axes offer a tidy way of constraining the discussion, since they seem to be both *orthogonal* and *comprehensive*. That is, a scholar's position along one axis does not determine his position along the other, and every historiographical approach in the field can be uniquely located somewhere in the space defined by the axes. I put forward these axes tentatively, however. They are meant merely as a starting point for discussion, not the last word. I welcome disagreement, since it forces us all to reflect on the nature of our own scholarly project, and it is precisely this reflexivity that will lead to compromise. Also, by plotting the field of science history on these axes, it becomes clear that intellectual and socio-cultural histories are continuous with one another. The disciplinary prejudices that separate them are more or less arbitrary boundaries on a homogeneous landscape. There may be reasons to accept them, but such reasons need to be clearly articulated and defended, since they do not follow apparent "natural" distinctions.

The first axis has to do with the causal role of intellectual and non-intellectual factors in the production of scientific knowledge. One can think of this axis as expressing a pseudo-numerical ratio between socio-cultural, external factors and intellectual, internal factors. The extreme internalist (at "0") denies any causal efficacy to socio-cultural context in the development of science. On this view, science develops entirely according to its own internal logic, via purely intellectual exchanges amongst its practitioners, as if they were all part of a single mental process. Moving away from this extreme, one allows more and more significance to socio-cultural, material, and institutional conditions, until one reaches (at "infinity") the extreme externalist, who believes that scientific reason is purely epiphenomenal, floating on top of the non-intellectual context surrounding scientific activity that is solely responsible for any change.

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<sup>38</sup>On the other hand, I would reject calls to let the two brands go their separate ways. See Pinnick and Gale (2000), Strasser (2005).

The second axis concerns the temporal *telos* or aim of scholarship. At one extreme is the view that history is meant to reconstruct the past “as it really was.” Extreme temporalists thus seek to immerse themselves in the past, with all of its twisting complexity, frightened by the specter of anachronism at every turn and hermeneutically circling ever closer to historical fact. At the other end of the scale are the extreme presentists, who study history in order to instruct the present or even the transcendent. These authors write from an anachronistic point of view and comfortably relate past events to their own concerns and interests, to the point of “rationally reconstructing” the past in their own image.

It seems that most, if not all, distinctive features of various historiographies can be read off from their position on this map. That is, one’s position on the map determines the kind of scholarship one produces. For instance, internalists will base their work more on primary sources than externalists; presentists will be more interested in generalization than temporalists; and so on. As we have seen, HOS tends toward temporalist externalism, while HPS leans toward presentist internalism. There are those toward the off poles, however. For instance, most scientists who write history, many pre-Kuhn historians of science, and “straight” intellectual historians exemplify temporalist internalism.<sup>39</sup> Meanwhile, many authors write externalist histories with presentist punchlines (viz. “The story of . . . and *how it changed the world*”), which can also be said of many popular science writers outside academe.<sup>40</sup> There are also any number of positions between the extremes. The axes, after all, are spectral, not binary, and since they are orthogonal, there is no position on the map that can be ruled out on a priori grounds. It remains for science historians to work out what part of the map they want to stake out for themselves.

### 3.4 A Hopeful Conclusion

Given the paradigmatic differences and institutional pressures acting on the HOS and HPS brands of science history, it is not altogether surprising that science historians have widely divergent views about what kinds of scholarship their discipline includes. Nor is it surprising that each brand of historian is remarkably ignorant regarding the methodology and aims of the other brand. Entrenched disciplinary prejudices have created insularity, lack of communication, an absence of cooperation, and even disdain. Meanwhile, those trying to mediate between the camps or operate in the middle ground gain recognition from neither side. Nevertheless, there is a feeling on both sides, I think, that something is wrong, both in their separation from one another and in their uneasy allegiances with other disciplines.

The prejudices dividing the discipline are actually accidental impositions, born in mainstream History and then extrinsically enforced when science history sought

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<sup>39</sup>As exemplars of this type, I have in mind Jed Buchwald, William Newman, Mordechai Feingold, Peter Dear, Nicholas Jardine, and (in more general history) Quentin Skinner and Jonathan Israel.

<sup>40</sup>Peter Galison’s *How Experiments End* and Stephen Johnson’s popular *The Ghost Map* are examples, respectively (Galison 1987; Johnson 2006).

institutional and intellectual relationships with the more established disciplines of History and Philosophy. This led to the artificial segregation of the field into its HOS and HPS brands. Yet science history is neither Philosophy nor mainstream History. It has disciplinary demands of its own, stemming from the peculiar nature of science itself. If we are to begin respecting those unique demands, we need to seriously evaluate our own various approaches in our own specialized discipline. To reiterate, my aim here is not to advocate one historiographical position over another. There may be good reasons for emphasizing cultural factors over intellectual ones and vice versa. My point, rather, is to encourage science historians to explicate the reasons why they choose certain historiographical approaches over others. One should not adhere to disciplinary biases that are not appropriate to the object of study, even if they are the methodological dictates of a “home” discipline. Science history should be defined as a separate discipline in its own right, something *both* HPS- and HOS-like, from which morals for History and Philosophy can be drawn, but which does not serve the sole purpose of producing such morals.

I have argued that science historians should question the historical and philosophical prejudices they have used to define the divergent strands of their discipline. Admittedly, this is a somewhat dangerous proposal, since it leaves science historians without a clear understanding of what their discipline *is*, at least for the time being. It also threatens to undermine the institutional support HOS and HPS have received from History and Philosophy. On the other hand, the proposal is not *very* radical. It only concerns the emphasis placed on certain factors and aims in the production of scholarship, with the suggestion that the degree of emphasis be left indeterminate. There are features of science history that remain uncontroversial. One would expect universal agreement that the discipline is concerned with the description and explanation of past science, which is itself the human enterprise of describing and explaining natural phenomena.<sup>41</sup>

Above all, we can fall back on a basic aim of science history: the descriptions and explanations we produce should be plausible. They should at least seem worthy of refutation, if not convincing outright, to fellow practitioners. If this is our aim, we will seek intellectual causes where they seem important and necessary and socio-cultural factors where they seem important and necessary, without a priori expectations favoring one kind or the other. In an adequate account, each step of historical development will seem plausible. The skilled reader will understand how and why things happened as they did, and the plausibility of the explanation will be determined by past events, not by its instantiation of disciplinary norms. There will be some latent anachronism, of course. Just as the strength of arguments depend on their time and place, the plausibility of an account partially depends on its historical context. That is to be expected, since the only way to make sense of the past is to “fuse our horizon” with that of historical events. Just where such fusion

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<sup>41</sup>The particulars of this definition might be open to dispute. For instance, depending on one’s point of view, it might or might not include human interventions in natural phenomena through technology and medicine. Ultimately, the scope of the discipline should be another point open for discussion.

should take place—close to the past or close to the present—is another matter for negotiation. The communal evaluation of plausibility, meanwhile, implies the need for venues and outlets open to the questioning of disciplinary bounds. The fact that cross-disciplinary scholarship is usually refereed by a scholar in one camp or the other, and therefore according to one set of entrenched biases, only redoubles the present difficulties.

The looseness of the plausibility constraint forces a science historian sincerely interested in constituting a coherent, cooperative discipline to be reflexive. Scholars need to be continually conscious of what it is that they are doing, even as they do it. One way to do this is to apply the methods of the study to the study itself, so that one is always calling oneself to account to oneself. One should defend not only the plausibility of the histories one produces, but the plausibility of the production of those histories, and let this defense be made consciously and publicly, subject to the observation and criticism of one's peers. The account of the account is just as important as the account itself. Of course, this point is nothing new. Reflexivity is a common denominator in arguments for most scholarly methodologies. Moreover, the best and most interesting scholarship is *always* the best and most interesting precisely because it is consciously reflexive in the sense I suggest. Indeed, conscious and explicit critical attention to the presumptions underlying responsible scholarship is what accounts for such work's ability to shape disciplines.<sup>42</sup>

I have tried to practice what I preach to some extent in the present essay—producing an historio-philosophical account of the way science historians do and perhaps should behave. In particular, I have tried to pay heed to both intellectual and socio-cultural factors in the development of our discipline, without privileging one over the other. Luckily, there are many other models to follow. Among recent work, I can draw particular attention to Hasok Chang's *Inventing Temperature*, which conscientiously and explicitly combines HOS and HPS approaches into something he calls "Complementary Science." Chang also shows how self-critical reflection leads to progress in science through a process of "epistemic iteration." To my mind, this is reflexive evidence that a reflexive approach is natural in the study of science.<sup>43</sup>

Ultimately, science historians will need to renegotiate the boundaries of their discipline so that their disciplinary prejudices are more organically suited to the historical study of science and not so beholden to the demands of mainstream History and Philosophy. It will require a considerable period of consensus building, and it is impossible to predict how the discipline will be constituted beforehand. I hope the present paper forms a starting point for negotiation, even by eliciting dissent.<sup>44</sup>

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<sup>42</sup>See Laudan (1990).

<sup>43</sup>Chang (2004). This estimation is not just mine. *Inventing Temperature* is a work of science history that shared the prestigious Lakatos Award for books in Philosophy of Science.

<sup>44</sup>This paper grew out of discussions with Bruno Strasser and was greatly improved by comments from Jutta Schickore, Andrew Janiak, and the editors of this volume. All error, overstatement, and ignorance is my own.

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