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## HPS and the Classic Normative Mission

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### 1. Introduction

This session is about the future of philosophy of science, one of a series of negotiations occasioned by the suggestion that philosophy of science must engage, not only the history of science and/or contemporary scientific practice but also the challenge, raised by social scientists, of seeing science as essentially a cultural and dynamic activity. The session is timely because these negotiations are well underway. Many philosophers of science are already committed to an inter-disciplinary eclecticism that has generated a heterogeneous family of science studies programs and projects, all designed to situate science in its social, historical, and politico-economic contexts, but each with its own sense of what shape inter-disciplinary science studies research will take in the coming years. Philosophers who favor this new disciplinary eclecticism face many problems if they are to successfully forge a hybrid science studies that does not violate their integrity as philosophers. If the proposed transformation is to be successful, philosophers of science must isolate an intellectual space in which traditional agendas, such as the concern for the clarification of concepts, can hold court.

In this space, I cannot possibly comment on these contending varieties of inter-disciplinary science studies. I can, however, outline what I regard as a new brand of HPS, one that is deeply rooted in the history of the exact science and that takes the perspective of scientific and cultural actors at face value. The virtue of this New HPS, I will submit, is that it furnishes philosophers of science with a fresh perspective from which to carry on philosophy's classic normative mission.

### 2. Two Styles of HPS

While recognizing that philosophy of science includes a vast array of projects and characteristic styles of exposition, as a preliminary move I want to offer a general contrast on two general positions that we can take on the relationship between the history and the philosophy of science:

(1) we can see history *as something that is applied to the philosophy of science*. Following Karl Popper, Imre Lakatos, and Larry Laudan, we can regard the history of science as a constraint on philosophical activity and as an external court of appeal for the viability of philosophical theses. For lack of a better expression, let's call this view classical HPS. It reached its apex with Larry Laudan's conference "Testing Theories of Scientific Change," held at Virginia Polytechnic and State University in 1987.

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The regulative principle of Classical HPS is that philosophers of science ought to ground their work in an accurate representation of the practical activities of scientists, whether this consist in genuine engagement with real science or with historical narratives about the development of scientific practices and institutions. As applied to philosophical practice, this principle is registered in the insistence that there are important differences between “historical” and “philosophical” questions, “narrow” and “broad” questions, and, most fundamentally, between “historical” and “philosophical” methods and tools of analysis. Classical HPSers expend vast amounts of energy worrying about these differences, with the aim of isolating an intellectual space where traditional philosophical methods and tools of analysis still hold court.

The supposition that there are methods and problems special to philosophy is reflected in the fairly widespread conviction that philosophy of science should not be allowed to remain at the micro level, lest the broad questions that we traditionally associate with philosophical discourse be obscured by technical detail: “philosophers,” David Stump submits, in his *Afterward* to a forthcoming volume—*The Disunity of Science: Boundaries, Contests, and Power*—“must not ignore science studies and simply get lost in the technical details of scientific sub-disciplines without considering broader issues.” The message here seems to be that traditional philosophical methods and approaches must dictate how it is that context and, in particular, historical narrative, is to be engaged. We are to keep the broad issues in the forefront and only then examine the historical record to sustain or deny these general theses.

(2) we can see philosophy as something *that is applied to the history of science* and/or scientific practice. Here we regard philosophical agendas as constraints on historical activity; following Foucault (1970), Hacking (1983, 1995), and perhaps Lorraine Daston, the desire to get clear on philosophical concepts and broad systems of classification is the final court of appeal for the relevance and scope of historical narrative. Though ethnophilosophy might be a better term, let’s call this view New HPS. The regulative principle of New HPS is that philosophical agendas are to be seen in historical relief, that concepts and systems of classification that currently authorize behavior are themselves historical and dynamical entities. New HPS has barely got off the ground, though I would submit that Hacking’s conference on Historical Epistemology held in 1993 at the University of Toronto signaled the public recognition of a hybrid style of science studies that already involves many philosophers of science.

### 3. New HPS and the History of the Exact Sciences

These two styles of HPS have different pedigrees. Classical HPS is standardly portrayed in the literature as a fairly recent development connected with the demise of logical positivism. Nicholas Jardine (1984), however, has argued convincingly, I think, that Johannes Kepler employed a history of hypotheses to defend the legitimacy of Tycho Brahe’s planetary hypothesis. Indeed, Jardine claims that the use of the historical record as a constraint on philosophical activity gave birth to what we now commonly regard as history and philosophy of science. If Jardine is right, Classical HPS was an intellectual reaction to a worry about the epistemological credentials of Tycho’s planetary hypothesis. This is what one would suppose, except that most of us get the date wrong by some four hundred years. Clearly, Kepler did not give Classical HPS a form that it has sustained throughout the centuries; his writings testify that HPS is not a recent creation but that it has been recreated many times.

New HPS is a comparatively recent creation with a very different pedigree. In order to get clear on this, we need to recount some details about the rise of history of the exact sciences. By the time Kuhn’s seminal work appeared in 1962, technical history of science (which I use here as short-hand for history of the exact sciences) was well-established; the *Archive for History of Exact Sciences* was established in 1960 under the editorship of Clifford Truesdell, a mathematician at Johns Hopkins University. Until fairly recently, technical history of science only existed in any proper form in the history of astronomy; now it exists in the history of exact sciences and, to a lesser extent, in some branches of biology. The organizing principle of technical history of science is the con-

viction that since 1700 (and in isolated instances much before that) science has been organized for control production in a highly specific way. Two convictions are responsible for this control production, that (a) special training (or at least intensive work) is essential to science, which is amply attested to by the production of exercise books, practice primers, instrument manuals, as well as the rise of institutionalized modes-of-inculcation; and (b) instruments provide reliable and special means for doing science. What technical historians are engaged in is figuring out how these two convictions cash out in the creation of specific sets of practical activities that include an attitude towards nature, namely, ones that enable access to control, manipulation, novel device production, and prediction. Many scientists are convinced that they are gaining intimate access to reality—that there is something in scientific practice that goes beyond manipulation and device production—but technical historians of science do not have to share these beliefs and, indeed, few do. When the technical historian reaches into the inner core of scientific practice and pulls out original meaning and skill—even when the skill is reproduced from archival and other evidence—no claims need be made about “reality” in a deep sense.<sup>1</sup>

The requirements for technical history of science are daunting—technical scientific training (usually to the graduate level), linguistic skills, including reading knowledge of Latin, French and German, training in historical methodology. This last requirement is critical since even competent scientists with good language skills can produce flawed work because of inadequate historical methods. As a field, technical history of science is associated with a number of canonical studies that serve as stable benchmarks for historical practice—Stillman Drake’s work on Galilean mechanics, Derek Whiteside on patterns of mathematical thought in the 17th century, Philip Lervig on Carnot’s theory of heat, Noel Swerdlow on Renaissance astronomy; more recently, Allan Shapiro on Newton’s optics, Jed Buchwald on 19th century wave optics and Maxwell’s electrodynamics, Istvan Szabo and Wilbur Knorr on the origins of Euclidean geometry, and Thomas Hankins on the Lebesgue measure.

Some of the major players are housed in scientific departments (Swerdlow in Astronomy, Stigler in Statistics, Edwards and Hankins, Feigenbaum, and Berggren in Mathematics). Others are housed in a handful of institutions—in North America, Toronto, Minnesota, The Dibner, Harvard and perhaps Wisconsin; in Europe, Aarhus, Utrecht, Hamburg, Frankfurt, and Copenhagen. It’s worth noticing that over the last twenty years a number of history of science departments have been absorbed into history departments—Johns Hopkins, Princeton and Montreal are prominent examples that come to mind. One of the advantages of independent institutions is that they provide a home for specialists who would otherwise be isolated in science departments or who are too technically-minded for history departments. On the other hand, this institutional separation tends to distance history of science from general history, such that historians of science are often seen by historians as not being real historians.

By the time philosophers of science warned up to Kuhn’s *Structure* in 1970 or so, institutions had already been created which were geared towards forging closer links between history and philosophy of science. The institutional negotiations that led to the creation of these institutions cries out for careful study, since they proved to be critical to the creation of New HPS. It may very well have been Kuhn’s book that persuaded philosophers of science that they ought to take the history of science more seriously, but those few philosophers (and, in some cases, classicists) who were shunted to institutions dedicated to the history of the exact science have discovered that technical history of science has a value in its own right that gives philosophers a fresh perspective for addressing traditional normative agendas.

What’s more, technical history itself has undergone an appreciable broadening of its scope, in large part because institutions dedicated to technical history have opened their doors to philosophers, sociologists, anthropologists and others who are interested in technical story-telling. In its first incarnation, it was burdened by an excessively narrow take on the factors that are proper to science and those that are not. Its pioneers championed an unwarranted distinction between factors “internal” and “external” to scientific prac-

tice—the adage “physics of the past” seems an apt description for the first wave which was hedged on this distinction. A second generation, however, recognized that technical history was a very particular way of constructing historical narrative and that, if managed properly, its underlying principles could address historical variables that members of the first generation would have castigated as “external” to science.

What’s more, New HPSers drew on the work of Foucault (1970) to discipline historical story-telling in order to make it responsive to our present circumstances and conditions. A New HPSer would be interested in Descartes’ scattered remarks about the possibility that God might deceive if it could be shown that they are relevant to our present concerns, say our views on plagiarism. Kepler’s optical researches—which gave modern physiology a blueprint for conceiving the human body in mechanical terms as a “system of corpuscles”—would be relevant for precisely the same reason. By the same token, however, New HPSers would not be at all interested in Newton’s remarks on the luminiferous ether or in Gassendi’s conception of causation unless it could be shown that they speak to our present conditions and circumstances.

For New HPSers, then, there is no domain of philosophy of science outside of the historical domain. None of the dichotomies that are constitutive of Classical HPS are countenanced. There are no “broad” issues and “narrow” issues; no “internal” and “external.” There are nuances and differences in emphasis that distinguish HPS from technical history of science but these differences by and large are inappreciable to those who do not engage in technical history of science. With the exception of this principle of relevance, New HPSers see no interesting differences between technical history of science and philosophy of science.

For philosophers of science working in Philosophy Departments, the opposite is necessarily true. There is the need to function as a general philosopher with one hand firmly on the “broad issues,” offering the standard courses in epistemology, logic, metaphysics, the history of philosophy (and the other sorts of courses that philosophers of science are obliged to teach). Since the other hand is firmly on one’s practices as a philosopher of science, this creates a pressing need to “rethink philosophy of science” in a way that rationalizes it with these other constraints on practice. The practice must still be philosophy, or at least perceived by moral philosophers, Kantians, and logicians to still be philosophy. One can engage technical history in a manner of speaking, but only at a safe distance. The situation is necessarily different with HPSers. HPS is shaped and defined by technical history. The need to rationalize it with general philosophy and to engage issues in a general way is eliminated. There is of a consequence no need whatsoever to rethink one’s work. The history is no longer external to the philosophy but is what one does as a philosopher. Of course, one is compelled to work in a narrowly historical way but that is part and parcel of the reason for migration to institutions for history and philosophy of science.

### 3. Contrasts

New HPS is not utterly removed from traditional philosophy of science—its legacy is apparent in its interest in the classification and clarification of concepts, though these concepts are now taken in a circumspect manner as historical creations. HPSers tend to produce novella-length studies, in contrast to technical history of science which tends to run into the hundreds and even thousands of pages. Focusing on an isolated concept will do that to the length of a paper, even one that is embedded in technical history of science.

New HPS, however, is quite distinct from technical history of science in that HPSers tend to focus on concepts that *authorize* our present behavior. Where a technical historian may spend years reconstructing a skill that came into existence and then vanished after a decade, the New HPSer tends to be interested in concepts that confer authority—i.e., concepts that have normative force and which, therefore, can tell us a great deal about ourselves. HPS, then, is not just technical history (i.e., it is not history of past science), but, to Foucault claims, the history of science insofar as it is still present.

It is instructive that proponents of these two styles of HPS have reacted very differently to the rise of the sociology of scientific knowledge (SSK). Those who insist that there are historical and philosophical domains, also tend to take the view that there are social and philosophical domains (sometimes called “the rational” and “the social”). A sociological project, on this view, is a different kind of project than a philosophical or an historical one. What’s more, sociological explanations are castigated as violations of the line between “internal” and “external” history that is invoked as a general strategy for assimilating technical history of science to philosophical projects. The reaction by Classical HPSers to the proliferation of sociological studies of science has been harsh and, not surprisingly, the startling success of SSK has occasioned yet another crisis and many new critical volumes geared towards seeking a new reconciliation. By now the pattern of denial, anger, and acceptance is familiar.

The reactions of New HPSers have been very different. Conceiving philosophy as something that is applied to hand-crafted studies of scientific practice, New HPSers have welcomed those sociological and anthropological works that go at things at the micro level. *Laboratory Life* (Bruno Latour and Steve Woolgar, 1979) and (with the exception of a few pages and the beginning and at the end) Pickering’s *Constructing Quarks* (1984) have been well-received, for the most part. Though Pickering cautions that SSK takes science to be “social all the way to its technical core” (1992, p. 1), New HPSers have no trouble taking both as collaborative works—respectively, as anthropology and sociology as applied to the history of science. Other works do not measure up, not because they are seen to be sociological or anthropological, but because they are fueled by an attached interest that overrides any real concern for detailed analysis. Works by Barnes (1977), Bloor (1976), and Shapin and Schaffer (1985) have been less well received for this reason; they are viewed by New HPSers as exercises in history of science applied to the sociology of knowledge (an exercise that is reminiscent of Classical HPS, except that history is now yoked to an attached sociological agenda). For New HPSers it is the attached agenda that is cause for concern, not the fact that the micro analysis focuses on sociological, anthropological, economic, and other contextual variables. HPSers are equally wary of attached philosophical agendas, and are wary of the work of Fuller and Goldman for precisely the same reasons.

#### 4. Dealing With The Broad Questions

One of the reasons why many philosophers of science insist that philosophers of science need to look at things in their generality is that this seems to afford a platform for addressing normative issues. The surest way of responding to philosophy’s “classic normative mission” is to ensure that philosophers do not take the perspective of actors at face value and “simply get lost in the technical details of scientific sub-cultures.”

My own position as a practitioner of New HPS is that the best way to come to grips with these “broader issues” is to look at how they are realized in very localized contexts. This is not merely a theoretical preference but a practical response on the part of New HPSers to the appreciable shift in the kinds of normative issues raised by philosophers and the new kinds of normative issues raised by feminists, political theorists, and philosophers working with sciences that have traditionally received little attention. Many of these contextually-nuanced normative worries only make sense if we take the perspective of the actors in question. An example is anatomical illustration. What constitutes a gendered anatomical representation? Is the discovery that anatomical representations are grounded in a set of nineteenth century pictorial conventions that classified women and blacks in the same way normatively relevant? Well, yes, I expect that it is if, as a feminist or as a black activist, you are eager to advance a series of political reforms that impact on all cultural minorities and if, as a feminist academic, you have been treated by your male colleagues as a privileged white academic and, therefore, not deserving of initiatives in the name of equity.

I suspect that proponents of Classical HPS will strongly resist my contention that the local context is the best place to raise normative issues. David Stump gives voice to a powerful organizational principle of Classical HPS when he asserts that “the best philosophy of science has always engaged broad philosophical issues by being immersed in the

technical aspects of cutting edge science, just as the best history of science has always kept large issues in the forefront while documenting and analyzing the situated context in the most minute detail.” This contention is arguably false on both counts. If historians of the exact science are our benchmark, the “best history of science” has been the canonical work that I have cited—work that carefully avoids loose generalization and is highly sensitive to the attachment of interests that threaten to skew the narrative in favor of some global conceptions of science. If history of the exact sciences is one’s measure, Kuhn (1978) on black body radiation is “the best history of science,” and not Kuhn on normal science. By the same token, Hacking’s *Representing and Intervening* (1983), highly esteemed by Classical HPSers, does its best to focus on a handful of inter-related concepts—manipulation, control, and novel device production—that are central to the history of the exact sciences. Hacking’s interest in these ends was shaped by his proximity to science and to technical history of science, not by a concern for generalization. He gives us a new take on science and a new way of looking at the concept of reality in science by bracketing many concepts that philosophers have long regarded as their staples, and by refusing to talk about science, except as it is practiced in laboratories. Yes, I suppose that the so-called “broad issues” sneak back onto center stage from time to time but the virtue of the work is to be found in its narrowness and detail, not in its generality.

Hacking’s very latest work takes this historical project a step further. For some time, he has been working on the history of psychology in an effort to say something about the creation of human kinds—fugues, split personalities, syndromes, and other psychological classifications. The work is pioneering because it is written in the manner of technical history of science—it is built from the ground up from sources that philosophers who specialize in philosophy of mind have not bothered to examine. This work, which will soon be published under the title *Rewriting the Soul: Multiple Personality and the Sciences of Memory* (Princeton, 1995), is an exemplar of what I call “philosophy as applied to the history of science” or ethnophilosophy. The historical narrative focuses tightly on the creation of a few concepts; his historical practice is disciplined by his interest in these concepts.

These concepts for Hacking are historical and dynamical entities, but the ramifications of this kind of work are exciting—it opens up the black box on psychological classifications. Since the concepts that Hacking scrutinizes authorize the way that we sort out people, his work tells us a great deal about the creation of our identity, at least insofar as we are subject to these classifications. What’s more, his work on human kinds furnishes us with a new way of raising normative issues. These issues are now part of the story: the interest in what he calls “human kinds” is part and parcel of the narrative; it is not imposed on it.

Hacking is not the only philosopher of science who approaches normative questions in this way. Nor is it only philosophers of science who raise normative issues in this manner. Feminist studies of the creation of the dual concepts of race and gender are by-products of detailed historical research. The normative concern is the story, the central claim being that you cannot understand race and gender as inter-connected systems of social classification (and stigmatization) unless you are fully apprised of the history. Here the philosophy and the history are not distinct entities; there are no philosophical questions to be asked over and above the history. There are no supervenient “broad questions” but there are plenty of positive results that makers of political and scientific policy can reference in their deliberations. We have no use for social epistemologists holding court over the history, asking “what can we now do with this history to make it a force for social change?” The history has normative force because it tells us about the creation of our value systems and how they impact on us.

## 5. Objections

One objection to the hand-crafted science studies that I am here advocating is that it will prevent us from subjecting science to public scrutiny. Social epistemologists and those interested in science policy, for example, could object that the kind of micro analysis that I am encouraging encourages us to frame the philosophical questions in a way that makes scientific and cultural communities seem more autonomous from the rest of

society than they really are. Further to this, those who are convinced that scientists often engage in a great deal of rhetorical activity will object that this approach requires that we take the point of view of social actors at face value. Micro analysis will compel that scholar who knows little or nothing about, say, nineteenth century physics, to evaluate scientific activities with the scientist's own criteria; the further we get into these cultural communities the further removed we will be from a point of view that allows us to subject the products of these communities to public scrutiny.

This issue crystallizes in a very neat way what is at issue in the very idea of an interdisciplinary science studies. Philosophers have traditionally been highly suspicious of special claims to expertise—epistemologists, for example, have worked hard to set themselves up as gatekeepers to Solomon's House; their descendants, self-styled 'social epistemologists,' have taken this project a step further (some would say to its logical conclusion) by insisting that expert opinion is merely the willingness of an uneducated public to accept the technical and highly diffuse jargon of scientists at face value. For those who are housed in Philosophy Departments, gatekeeping is a strategy for sticking close to the action and, therefore, one can readily understand why Classical HPSers are eager to retain their traditional right to look at science in all its generality.

My own view, in contrast, is that the increasing specialization, not only of scientists, but also of scholars in the humanities and social sciences, is a reflection of our growing acceptance that knowledge is the possession of distinct cultures—that the knowledge possessed by women and aboriginal cultures, for example, is different in context but not in kind than that possessed by the scientific culture of particle physicists. Culture, as Max Weber (1949, p. 76) liked to say, is a value concept—it is always 'culture from a particular point of view.' Since we can only speak meaningfully of the culture of science from the perspectives of its practitioners, I question whether we can meaningfully embrace the vision of science as comprising many heterogeneous elements and communities and still cling to the globalist supposition that there is a privileged position for evaluating science.

The recognition of the difference that culture makes demands a less skeptical attitude on the part of science studies practitioners. We must rethink the "classical normative mission" in a way which is consistent with thinking in the small and taking at face value the claims of specialized communities and, in particular, science studies researchers who are engaged in different projects. We need to think of one another's projects as resources and not, in the manner of classical HPSers, as claims to expertise that need to satisfy global criteria. There is room for some good old fashioned philosophical debate, of course, to the extent that scholars mutually participate in the same projects, but philosophers would be surprised, I suspect, just how little debate there is among science studies researchers who deeply engage the perspectives of scientific actors.

### Notes

<sup>1</sup>I'm drawing heavily on the work of Jed Z. Buchwald (1994) for my characterization of the history of the exact sciences.

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