Ten Problems in History and Philosophy of Science

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Ten Problems in History and Philosophy of Science

*By Peter Galison**

ABSTRACT

In surveying the field of history and philosophy of science (HPS), it may be more useful just now to pose some key questions than it would be to lay out the sundry competing attempts to unify H and P. The ten problems this essay presents are grounded in a range of work of enormous interest—historical and philosophical work that has made use of productive categories of analysis: context, historicism, purity, and microhistory, to name but a few. What kind of account are we after—historically and philosophically—when we attempt to address science not as a vacuous generality but in its specific, local formation?

On 8 August 1900, David Hilbert took the podium at the Sorbonne to address the International Congress of Mathematicians. Rather than a presentation of the usual sort—summing up the speaker's own work or conducting a high-altitude overview of the field—he presented ten problems from across the discipline's map. While it would be foolish to belabor parallels between the history and philosophy of science (HPS) and mathematics, or between 2008 and 1900, or (worst of all) between this author and that one, perhaps this is a propitious moment to ask rather than tell.

That odd conjunction, "HPS," covers a multitude of sins, only some of which offer genuine temptation. True, some history of science in the 1930s through the 1950s aimed to use short historical accounts to bolster broad ambitions of a unification of scientific knowledge based on observation statements. That seems like an invitation both historians and philosophers of science could decline without fear of missing a great party. Nor is the inverted (*anti*positivist) version of HPS likely to compel much enthusiasm: a search for historical examples that would engender enthusiasm for a picture of science led by theory, reinforced by a subordinate experiment. More generally, the competitive campaign fought in the 1970s among philosophers to find *the* theory of scientific change no longer grabs philosophers of science as a plausible enterprise. Science seems far too heterogeneous for that: too diverse at a given time (especially now); even within the same subdiscipline too much has changed. Biology in 1808, 1908, and 2008 looks too different to capture with

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a single, simple model. Little as the philosophers these days want to hunt *the* covering theory of scientific change, historians have even less interest in providing potted examples to confirm or deny this or that one-size-fits-all picture.

In some measure, and for a variety of reasons, philosophers of science have turned to questions not particularly historical: bounded rationality, decision under uncertainty, game theory. Others have focused on foundational questions—issues that lie within or behind the special sciences. The quantum measurement problem comes to mind; so do some quite fascinating studies in the philosophy of economics, the philosophy of biology, the philosophy of mathematics, or the philosophy of general relativity. For their part, historians of science have, in some measure, turned away from explicitly philosophical questions to attend to the broad set of issues raised by sociology, anthropology, and ethnology.

In the ten questions that follow, my goal is not at all to cover the full range of work in history and philosophy of science but, instead, to gesture toward some problem areas where only an intense collaboration of effort can help us move forward.

PROBLEM 1: WHAT IS CONTEXT?[1]

One of the staples of recent history of science has been an increasingly sharp refusal to play by the rules of Cold War historiography. For a half century or so after World War II, discipline after discipline split its goals along the axis of autonomy and dependence. Formalism in art history set itself against the social history of art. Literary studies were marked by a division between those who wanted close text-alone readings and those who sought to set novels (for example) in their time and place. And history of science produced its own intellectual civil war, with internalists on one side and externalists on the other. Marching under various flags on various fronts, the terms of confrontation seem to repeat over and over that divide between Marxists and anti-Marxists.

Happily (in my view), some of the best recent work in history of science refuses this fight. A focus on the practice of science—the structure of scientific work in the field, museum, observatory, classroom, or shop floor—offers the opportunity to address scientific concepts and comportment in specific sites and circumstances *without* hewing to a vision of what is "truly scientific" or "merely exterior."

But here's the rub. These sorts of accounts rely heavily on the notion of "context," that elusive explanatory structure always invoked, never explained. And that leads to our first question, one that, as Kant said in an altogether different setting, we have so far been able neither to solve nor to avoid:

[1] **What Is Context? (Problem 1).** Examples of nonreductive contextualization of science include Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, N.J.: Princeton Univ. Press, 1985), which treats early modern debates over the validity and form of experimentation; Martin S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: Univ. Chicago Press, 1985), about the nature of stratigraphic evidence in the Victorian era; and David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: Univ. Chicago Press, 2005). Though philosophers often take "context" to designate purely textual surrounds, the following works (as *un*reductive as the preceding works from the history of science) push the history of philosophy closer to the kind of contextualization familiar from history of science: Nancy Cartwright, Jordi Cat, Lola Fleck, and Thomas E. Uebel, *Otto Neurath: Philosophy between Science and Politics* (Ideas in Context, 38) (New York: Cambridge Univ. Press. 1996); Michael Friedman, *The Parting of the Ways: Carnap, Cassirer, and Heidegger* (Chicago/La Salle, Ill.: Open Court, 2000); Klaus Christian Köhnke, *The Rise of Neo-Kantianism: German Academic Philosophy between Idealism and Positivism* (Cambridge: Cambridge Univ. Press, 1991); and Ronald N. Giere and Alan Richardson, eds., *Origins of Logical Empiricism* (Minneapolis: Univ. Minnesota Press, 1996).

Problem 1: Contexts of Argument. When philosophers talk about the context of an argument (say, by Descartes), they often mean bringing into the argument not only the text in question but also the texts of surrounding philosophers (issuing from the late sixteenth century, for example) addressing related issues. When historians speak about context, they often have in view the nontextual environment, which might be political, institutional, industrial, or ideological—as in "The context of Oppenheimer's remarks on atomic research was the detonation of the Soviet atomic bomb." At the simplest level—and it is not simple—the question arises: What kind of thing is a candidate for context? Further: Is a contextual explanation as strong as a causal account, the way we might say "The reason the temperature of that piece of floating soap is at 70 degrees is that it is sitting in a bathtub of 70-degree water"? (That seems too strong.) Is a contextual explanation as weak as saying that the surround offers "resources" taken up by the scientists we are studying? (But this seems too weak.) In short: What is context, and how does a contextual explanation work?

PROBLEM 2: PURITY AND DANGER[2]

Perhaps it is appropriate that the Cold War as a Manichean struggle should imprint that image on so much of our historiography. So much ink spilled over whether the sciences were truly pure or were merely tools in the hands of forces outside their purview. Enthusiasts mounted a strong defense to show that the sciences really did proceed as if in an unearthly bubble floating free of the bonds of politics, religion, ideology, industry, psychology, or gender. Science, according to this view, floated free in the realm of the superlunary. At the same time, opponents gathered their powerful offense to show precisely the opposite: that science lay tethered and responsive to the forces of the here and now. Science rose and fell through creation, use, and corruption—it lay fully in the realm of the sublunary.

So much seemed at stake in these Cold War battles. Superlunary science seemed the only hope for a model of democracy. Enlightenment reason, argued many, carried just that mixture of rigor and courage that could block the ferocious and demeaning demands of Hitlerism and Stalinism as they pounded on the gates of the academy. A generation of scientists, philosophers, and historians fought for the Unity of Science movement as an intellectual Archimedean point. They had seen—up close—science, the humanities, and social science yanked this way and that on the short leash of raw power. Whatever faults it had, the ivory tower stood for the interwar and World War II generations as a beacon of hope. But as the Cold War aged, as "the war" increasingly called to mind Khe Sanh rather than El Alamein, the symbolic register of science began to slip. For a generation of scholars who came of age after the 1960s, rather than in the 1940s and 1950s, science appeared not so much the last bulwark of reason against brute force as, instead, the sharp edge of endless war.

"Pure science" may have found its uses in the classic distinction between "pure" and "mixed" mathematics; "pure" science certainly can be found occasionally, without much

[2] **Pure Science (Problem 2).** On purity of knowledge, esp. in the social sciences, see Robert N. Proctor, *Value-Free Science?* (Cambridge, Mass.: Harvard Univ. Press, 1991); on mathematics that sought to divest itself of the referential see Herbert Mehrtens, *Moderne Sprache Mathematik: Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme* (Frankfurt am Main: Suhrkamp, 1990); on objectivity see Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone, 2007).

fanfare or prestige, in the eighteenth century. But by late in the nineteenth century, "pure science" and "applied science" became terms to reckon with, terms of approbation and prestige, terms to be found in professorial titles, institutions, and journals. "Pure mathematics" and "applied mathematics" (*reine und angewandte Mathematik*; also *reine und angewandte Physik*) were, by 1900, commonplace expressions, perhaps most dramatically in the German-speaking world and then much more widely in English, French, and other languages as well. "Pure science" played out not only in the natural sciences but also, as we know from recent scholarship, throughout the social sciences.

By the late Cold War, "pure science" signified differently—it had become a fighting phrase. Some defended the idea as an ideal, some attacked it as impossible, and yet others bathed the world in an elegiac light: pure science once was but is no longer; or it could have been but, in a gesture of weakness or greed, succumbed to temptation. It is this last position that seems to me to have dominated recent discussion, a kind of postlapsarianism that seemed to say: science had been or could be truly pure. Science could be purely "curiosity driven" (as that dead-letter phrase has it) but all too often had eaten apples proffered by the wrong species of serpent. There were apples of ideological correctness, apples of industrial greed, apples of military procurement. "If only . . . ," the postlapsarians keep saying: "If only physics had refused temptation, then truly we would live in a paradise of uncorrupted knowledge."

Those who wanted to denounce science greeted the idea of its embeddedness in its time and place with glee—as if science were nothing but the reflection of its specific sites. Those who wanted to say that science had been saintly—but subsequently lost that purity—welcomed contextualization as proof of the moral depravity of physics, chemistry, biology, and their relatives. And so:

Problem 2: Of Purity and Fundamentality. What is purity? How has this notion shifted from the pure and mixed mathematics of the Greeks to the Bourbaki rejection of mathematics-as-structure? What counted (and what counts) as purity in logic, in physics, in chemistry, in biology? How has this distinction played out in the medical sciences—and what relations, across the board, does "pure science" bear to "basic science" or "fundamental science"? What stands as the opposite of pure science and how has "impurity" changed?

PROBLEM 3: HISTORICAL ARGUMENTATION[3]

For years, the history and philosophy of science has dutifully created a parallel universe in which the disciplinary divisions of mid-twentieth-century science are replicated. We

[3] **Historical Argumentation (Problem 3).** As in all the sectors of this question list, the relevant bibliographies would fill volumes. A few starting points on the cross-disciplinary study of scientific practices relevant to demonstrations (historical epistemology) might be the following. On the use of statistics and probability see Ian Hacking, *The Emergence of Probability* (Cambridge: Cambridge Univ. Press, 1975); and Gerd Gigerenzer *et al.*, *The Empire of Chance: How Probability Changed Science and Everyday Life* (Cambridge: Cambridge Univ. Press, 1990). On modeling in science see Soraya de Chadarevian and Nick Hopwood, eds., *Models* (Stanford, Calif.: Stanford Univ. Press, 2007); and Ursula Klein, ed., *Tools and Modes of Representation in the Laboratory Sciences* (Dordrecht: Kluwer, 2001). On experimenting see Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge Univ. Press, 1983); and Peter Galison, *How Experiments End* (Chicago: Univ. Chicago Press, 1987). For more on the conjoint history and philosophy of scientific instruments see Galison, *Image and Logic* (Chicago: Univ. Chicago Press, 1997); Davis Baird, *Thing Knowledge: A Philosophy of Scientific Instruments* (Berkeley: Univ. California Press, 2004); and Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (Oxford: Oxford Univ. Press, 2004).

divide ourselves and our work along these lines—logging on to the History of Science Society Web site in 2008 precipitates the chance to find members of the profession by clicking on a topic and picking out the three-dimensional blocked-out space defined by a region, an era (mostly by century), and the elements in this proffered menu:

Astronomical Sciences
Biological Sciences
Cognitive Sciences
Mathematics
Earth Sciences
Medical Sciences
Physical Sciences
Social Sciences
Technology

As a shortcut to finding people (its intended use), such a scheme can be quite useful. But as we define problems for the next generation of work, as historians and philosophers define the objects of study, it is critical to bear in mind that—as categories of scientific work—these knowledge boxes are specific to their time and place.

The "cognitive sciences"—that evolving mix of disciplines involving (*inter alia*) artificial intelligence, physics, mathematics, linguistics, and philosophy—appear to be of quite recent vintage. Most people in the field date this concatenation from the early 1970s. In any case, it is certainly of a very different vintage than "mathematics." Of course, even mathematics, that most venerable of disciplines, has historically carved out different matters in different eras. Like practitioners in any other scientific field, mathematicians have done their share of major boundary crossing and boundary setting. It is certainly within time memorial—not immemorial—that algebra and geometry crossed, that probability and statistics came to count as mathematics, and that algebraic geometry, algebraic topology, and analytic number theory became primary fields of inquiry. Even the convoluted categorical hierarchy of the sciences is repeated among the historians: "Physical Science" quite typically includes "Earth Sciences" as a subcategory alongside its other constituent fields, "Chemistry" and "Physics," but in the HSS list "Earth Sciences" has a place alongside "Physical Sciences." This detail is important only as an indicator of the powerful desire to repeat inside historical analysis structures that happen to be given (now) by the sciences.

From the philosophical side, one finds something quite similar in the way of professional partition: philosophy of biology, philosophy of physics, philosophy of technology, philosophy of mathematics, and so on. More than the self-identification of scholars is at stake: these categories carve out the designated remits of journals, meetings, courses, books, on-line forums.

It should be said up front that this categorical division of labor functions relatively well for the study of HPS during the period from the mid-nineteenth century to the mid-twentieth century. Though the objects studied change, of course, there is something roughly commensurate about the nature of physics in 1930, 1950, and 1970. The discipline grew and new specialties arose, but one recognizes across these years much continuity in the teaching of physics (start with mechanics, proceed to electricity and magnetism, advance to "modern" physics of the atom . . .). The division into theoretical, mathematical, and experimental physics carries across the mid-twentieth century rather well. True, the boundary between physics and chemistry changes; but (whatever critics said as they raised their anti-Einsteinian voices) no one argued that special relativity was really a piece of chemistry or a fragment of biology—not in 1905, not in 1955, not even in 2005.

This relative stability within the modern period permitted a certain kind of HPS work to proceed. And the fruits of this collaboration in addressing certain problems are clear. That is not to say that all points have been covered or that the concentrated effort in this area is unwarranted. But one would be hard pressed to argue that historians and philosophers of science have attended inadequately to the study of special relativity, nonrelativistic quantum mechanics, the gene, or Darwinian selection. You may not be satisfied with the answers, but you won't have a problem assembling quite a stunning bibliography on the history and philosophy of "evolutionary fitness," "the nature of number," or "quantum measurement." More recently and more intensively, some in HPS have taken up questions that arise from areas that have not received this highly focused examination—joint work on the history and philosophy of chemistry or geology comes to mind.

One avenue for future inquiry is a focus on problems that are not, by their very formulation, organized around questions raised within the well-established (modern) scientific disciplines. Probability, for example, is not a subject reducible to any single discipline; you can't, so to speak, get to probability from physics alone or mathematics alone. In fact, the history and philosophy of probability has, as we now know from important recent work, as much to do with the management of populations and the establishment of judicial procedures as any particularly technical problem in one of the natural sciences. This brings us to a query:

> **Problem 3: Technologies of Argumentation.** When the focus is on scientific practices (rather than discipline-specific scientific results *per se*), what are the concepts, tools, and procedures needed at a given time to construct an acceptable scientific argument? We already have some good examples of steps toward a history and philosophy of practices: instrument making, probability, objectivity, observation, model building, and collecting. We are beginning to know something of the nature of thought experiments—but there is clearly much more to learn. The same could be said for scientific visualization, where, by now, we have a large number of empirical case studies but a relatively impoverished analytic scheme for understanding how visualization practices work. So, cutting across subdisciplines and even disciplines, what is the toolkit of argumentation and demonstration—and what is its historical trajectory?

PROBLEMS 4 AND 5: FABRICATED FUNDAMENTALS[4]

From philosophy, we have inherited the distinction between methods of gaining and securing knowledge (epistemology) and methods of establishing the set of what there is in the world (ontology). Within HPS, this has been historicized to produce a set of questions related to Problem 1 (What Is Context?) but not identical with it: How do

[4] **Fabricated Fundamentals; Ethics of Making New Things (Problems 4 and 5).** The problem of new kinds of objects that are of both scientific and ethical interest is taken up in different ways in these works: Michael Sandel, *The Case against Perfection* (Cambridge, Mass.: Harvard Univ. Press, 2007); Davis Baird, ed., *Nanotechnology Challenges: Implications for Philosophy, Ethics, and Society* (Singapore: World Scientific, 2006); Steven Epstein, *Impure Science: AIDS, Activism, and the Politics of Knowledge* (Berkeley: Univ. California Press, 1996); and Hugh Lacey, *Values and Objectivity* (Lanham, Md.: Lexington, 2005).

historical conditions establish the acceptable forms of knowledge generation (historical epistemology)? Correspondingly, how—*historically*—do certain objects come into existence (historical ontology)? A more fully developed form of historical epistemology or ontology would precisely bring together some of the strategies of argumentation. For example: Under which circumstances and in what conceptual form do collecting and observing together bring new kinds of objects or classes of objects into existence?

In the course of debating history of science and ontology, the discussion turns to "natural kinds," that breakdown of entities that divides nature in ways that transcend "mere" human choice. Gold atoms are supposed to be natural, lawn chairs artificial. The history and philosophy of biology is full of debates over whether species, genuses, or varieties are natural kinds. Difficult, boundary battle–provoking cases are not hard to find. Light a hydrogen bomb—as the American Atomic Energy Commission first did on 31 October 1952—and soon, settling around you in the fallout, are new elements, including Einsteinium (99) and Fermium (100). Are they natural kinds because they have a place on the periodic table—are they, indeed, quintessentially natural kinds by analogy with gold (79)? Or are they not natural kinds because (given their short lifetimes) they tend not to be found lying around in quantities that the extraction industry can market? Such rare, short-lived objects have not generally provoked much interest in HPS, but the scientific landscape is changing.

We face a host of new things that are not nearly so retiring as the elements named after Albert and Enrico. Is drug-resistant tuberculosis artificial because it has emerged largely as the result of the overuse of (human-produced) antibiotics in meat and medicine? Does the artificial/natural status of an entity like this new tubercular bacterium change because it is of recent and human-inflected origin? HIV/AIDS has altered the demographics and economy of numerous countries. SARS precipitated a worldwide panic. Experts may debate the reality status of attention-deficit/hyperactivity disorder syndrome, but with 5 percent or so of all American schoolchildren taking Ritalin, AD/HD already has immense pragmatic consequences.

Here is a conjecture: the science of the first half of the twenty-first century will increasingly involve fabricated objects, not just as "applications" but as the primary objects of study in the traditionally designated "pure" parts of physics, chemistry, and biology. Increasingly, physical, biological, and medical scientists will be dealing not so much with questions of whether something like the Higgs particle exists (or how we know it exists) as with the problematic (or salutary) new existence of built things like manipulable nanotubes. Already, work in the nanosciences involves teams composed of unhierarchical assemblies of physicists, electrical engineers, and surface chemists. Increasingly, the idea of a pure science that finds application elsewhere does not fit the working practices of the laboratory.

In other domains—the study of virology and the meshing with atomic physics—physical chemistry is a part of daily routine. Should we be able to produce molar quantities of atom-scale transistors, would they be described as a natural or an artificial kind? Is a modified, self-reproducing nano-scale circuit artificial or natural? In which camp does a modified strand of DNA belong—would its recency count against it as "natural," or would the offspring stemming from that genetic structure be artificial a thousand generations down the line? Is an ear of genetically modified corn or Dolly, the cloned sheep, "unnatural"? Thus:

Problem 4: Making Things. In an age of objects fabricated from their atomic structure on up, it is increasingly hard to separate the made from the found. This is not a purely abstract matter: legislation around the world restricts the production and sale of genetically modified organisms (GMOs)—launching a debate that has brought chaos to agricultural policy and exercised legislatures throughout Asia, Europe, Africa, and the United States. Our old dichotomy simply is not up to the task, and the history and philosophy of science would do well to address the conceptual and pragmatic criteria that pick out these new objects—objects that may range from the virtual objects of computer simulation to the carbon-fiber composites that are beginning to appear in tennis rackets and airplane wings. Once we begin to think this through, it may well be that the artificial/natural break *never* worked too well. (Is a highly hybridized rose still a rose, a rose?)

If, as in Problem 4, we address the problem of fabricated fundamental objects (or some such intentionally oxymoronic construction), there remains a further set of issues, ones that might cross the "H" of history of science with the more ethical branch of the "P" of philosophy. What do these quasi-natural kinds mean for discussions about what we *should* be making and modifying? So, then, to a more normative kind of inquiry, the onto-ethical aspect of these new basic things:

Problem 5: What Should We Make? Within this *tertium quid*—this ambiguous category of the fabricated fundamental—are a host of alterations to the body that seem increasingly imaginable: the enhancement of muscles, of memory, of height; sex selection before conception. Michael Sandel has written about what he calls the "case against perfection," an argument not grounded in the direct dangers associated, for example, with human cloning. Instead, and more broadly philosophically, he rests the case against perfection on the diminution in who we might want to be. That is, he grounds it in a concept of ourselves that would be diminished should we come to view our children and our own humanity as a willful product rather than a gift. Examples like these modifications of the human self suggest that the history and philosophy of many kinds of new objects cannot quite be treated with the same detachment with which one studied the unraveling of the atomic or cell nucleus. How—historically, ethnologically, but also and importantly *ethically*—*ought* we analyze the production of fabricated fundamentals?

PROBLEM 6: POLITICAL TECHNOLOGIES[5]

Bringing ethicists into the conversation about new diseases and enhancements suggests a wider concern. "HPS" has typically had in view philosophy as "M & E," metaphysics and

[5] **Politics of Science (Problem 6).** For a survey of the confrontation of politics and science see three special issues on this subject in *Social Research*: "Science and Politics," 1992, *59*(3); "Politics and Science: How Their Interplay Results in Public Policy," 2006, *73*(3); and "Politics and Science: An Historical View," 2006, *73*(4). Technologies of political practice are of course quite diverse, but they include voting, technology and privacy, bioprospecting and authorship, and national security secrecy. On voting see, e.g., John Carson, "Opening the Democracy Box," *Social Studies of Science*, 2001, *31*:425–428; on technoprivacy see Peter Galison and Martha Minow, "Our Privacy, Ourselves in the Age of Technological Intrusions," in *Human Rights in the "War on Terror,"* ed. Richard Ashby Wilson (Cambridge: Cambridge Univ. Press, 2005), pp. 258–294; on bioprospecting and intellectual property see Peter Jaszi and Martha Woodmansee, "Beyond Authorship: Refiguring Rights in Traditional Culture and Bioknowledge," in *Scientific Authorship: Credit and Intellectual Property in Science*, ed. Mario Biagioli and Galison (New York: Routledge, 2003), pp. 195–224; on science and national security secrecy see Galison, "Removing Knowledge," *Critical Inquiry*, 2004, *31*:229–243. A fascinating political-contextual account of Mendeleev's work can be found in Michael Gordin, *A Well-Ordered Thing: Dmitrii Mendeleev and the Shadow of the Periodic Table* (New York: Basic, 2004).

epistemology. But it may be that just as historians have increasingly taken on approaches drawing from sociology and anthropology, so the philosophical side of HPS may, in certain arenas, need to draw not only on ethical philosophy (as in the case of bioethics) but also on political and legal philosophy.

I say this because a host of problems have arisen in recent years that don't quite fit under the well-populated earlier rubric. The "politics of science" handled dramatic matters of nuclear war—how wars of first and second strike were to be imagined, the ethics of "city trading," "mutual assured destruction," and "counterforce." "Politics of science" embraced as well the autonomy of science—the endless battle to keep politicians and ideologues from dictating what science should say about matters such as the origin of the universe, its structure, the human body, and much else.

Questions of gross interference with scientific work may have had their roots in by now canonical matters pertaining to Galileo, Lysenko, and the *Deutsche Physik* movement. But such questions are hardly done and gone: the early twenty-first century has more than its share of questions of just this sort—around the scientific safety of abortion, the efficacy of sexual abstention programs for teenagers, or the permissibility of stem-cell research. Battles over scientific autonomy have provoked furious debate—with no end in sight. Less dramatic—but equally crucial—has been the continuing debate about the right way to fund (and to distribute funding for) science.

These sorts of questions (about scientists' autonomy, government policy, federal science funding) were and remain vital. But increasingly other disturbances have arisen at the edge of science and politics—not so much about scientific results as about seemingly smaller, more specific, and sometimes more technical aspects of the politics of science. One might (following Foucault) call this domain the technologies of politics:

Problem 6: Political Technologies. Increasingly, in the twenty-first century, we see technical questions of privacy that could have arisen earlier (and often have) but are strikingly accelerated by the digital world. Medical privacy, communication privacy, computer privacy—as data mining becomes easier, the notion of the "private" comes under great pressure. Government secrecy is hardly new—but, with new forms of surveillance, it too has expanded. Intellectual property, again, is not new as such—but we are experiencing a massive intensification of bioprospecting, of the cultivation of certain individuals' cell lines for medical research, of patenting of certain life forms. What, we can ask, is the politics of these new technologies? How does their prosecution alter our sense of our private sphere—or, for that matter, of what is "ours" in a personal or even species-specific sense?

PROBLEMS 7 AND 8: LOCALITY AND GLOBALITY[6]

The turn toward local explanation in the historical, sociological, and philosophical understanding of science may well be the single most important change in the last thirty years. True, case studies, long a staple of history of science, restricted the scope of

[6] **Local and Global (Problems 7 and 8).** On microhistory see Giovanni Levi, "On Microhistory," in *New Perspectives on Historical Writing*, ed. Peter Burke (University Park: Pennsylvania State Univ. Press, 1991), pp. 93–113; for one of its exemplary instances see Carlo Ginzburg, *The Cheese and the Worms* (New York: Penguin, 1983). For three discussions of how local knowledge moves see Bruno Latour, *Science in Action* (Cambridge, Mass.: Harvard Univ. Press, 1987); Simon Schaffer, "A Manufactory of Ohms," in *Invisible Connections: Instruments, Institutions, and Science*, ed. Robert Bud and Susan E. Cozzens (Bellingham, Wash.: SPIE Optical Engineering Press, 1991); and Peter Galison, "Material Culture, Theoretical Culture, and Delocalization," in *Science in the Twentieth Century*, ed. John Krige (Amsterdam: Harwood, 1997) (see also Galison, *Image and Logic* [cit. n. 3], Chs. 1, 9).

analysis; but in the period after World War II, case studies often were organized around the discovery of a large-scale idea in the history of science: the history of the vacuum, the atom, oxygen, germs. Such studies can well be highly dispersed in space and have often been particularly interested in the progressive accumulation of results "leading to" the unveiling of a now-accepted truth of science.

The kind of locality I have in mind is more like the *microhistoria* of Carlo Ginzburg in his account of a sixteenth-century miller, Menocchio, as he struggled, under the fierce interrogation of the Inquisition, to explain his views of the genesis of life. For Ginzburg the case, so to speak, is highly focused, the camera of our observation trained close-up on an individual with a name, address, and views. In attending to views, to the symbolic register of particular people of a particular (artisan) class, this is a history at once social and cultural.

The history of science analogues of such highly focused studies have had an enormous effect on the discipline. Laboratory studies, for example, have peered into observatories, field stations, table-top studies, and massive particle physics collaborations. In attending to the moment-by-moment, by following people, often in small groups, as they shape their ambitions, modify their instruments and procedures, and move step-by-not-quite-predictable-step toward publication, new questions arise. Now it is no longer just, or even mainly, "Who discovered X when?" but instead a matter of how knowledge stabilizes to the point where a result seems secure. Competition within groups, between groups, matters; though often not reducible to the world in which it sits, the laboratory is no longer a space station floating free and out of radio contact with home base. Instead, laboratories are more human, and the people in them—technicians, engineers, students, and researchers— bring a wealth of different expectations, skills, and experience to the bench.

Philosophically, the interest of this work often lies precisely in its acknowledgment that it is not possible to sort unambiguously the "internal (scientific) factors" from the "external (nonscientific) factors." How, the microhistorian asks, would one classify the view that telescopes or statistics or photographs deceive? Or that a proof is valid even though it is based on a simulation or a computer-aided procedure? When a prominent mathematics journal renounces images altogether, its views may be grounded in many ways— but surely such a decision is not "internally mathematical" in quite the same sense as, say, the refusal to publish an article claiming that that $2 + 2 = 5$.

One insufficiently understood question is therefore this:

Problem 7: Locality and Microhistory. What kind of explanation is involved in the microhistorical enterprise? At one level, it cannot simply be grounded in "typicality," at least not naively. All the rich detail, the quirky individual engagements, that make a microhistory of Menocchio or the late seventeenth-century Royal Society interesting work against the grain of simple generalization. Back in the postwar period, James Bryant Conant hoped that the Case Studies in Experimental Science that he organized would, by a kind of Baconian generalization, lead to a general understanding of scientific method. But it is hard to see this Baconianism emerging from microhistories today. Microhistory is supposed to be exemplification, a display through *particular* detail of something *general*, something more than itself. It is supposed to elicit the subtle interconnections of procedures, values, and symbols that mark science in a place and time, not as a method but more as a kind of scientific culture. This then leads to a hard question: What does it mean to aim for exemplification without typicality? And if case studies are the paving stones, where does the path lead?

Conversely, after the remarkably successful buildup of microhistorical cases, one can ask after the *limits* of localism. That is, suppose we continued to fill our journals with ever more case studies, packed encyclopedias with dozens of microscopic inquiries into every laboratory, field station, and observatory of any weight anywhere. Would there be, in principle, a residue? Would there be kinds of questions that simply could *not* be accessed even through the objectives of the most assiduous application of our fine, 1000x historical-philosophical microscopes?

An analogy from physics: Michael Faraday (experimentally) and James Clerk Maxwell (theoretically) offered a formidable critique of electrical and magnetic action-at-a-distance. By replacing these *nonlocal* forces with proximate action, they utterly transformed the discipline and provided a model for physical understanding that served for generations. Einstein's equations for general relativity were, as he often stressed, precisely the extension, by analogy, of Maxwell's equations to the domain of gravity: near-action distension of space-time geometry. Fields propagate over time, over space, at finite speeds—the processes tracked theoretically by the near-action causality encoded in differential equations.

That said, there are many very important effects in physics that *cannot* be understood locally, cannot be built up from the behavior of matter taken one bit at a time. Sometimes this is simply a matter of scale: to understand the weather, you cannot simply take measurements day by day or hour by hour in front of your castle (as every minor European princeling did for many years). The relevant scale for meteorological prediction is larger. You, with your thermometer, your castle keep, and your notebook alone, will *never* guess the existence—much less the dynamics—of cold fronts and warm fronts. Similarly, in many instances in the history of science, it is not enough to know how this or that bit of technique got taken up in a particular laboratory or moved from one lab to another. The large-scale synchronization of laboratories made possible a host of new disciplines, from the examination of migratory bird patterns to the mapping of the sky, from modern geology to cosmic-ray physics.

Physics has other kinds of nonlocality. Suppose (as the condensed-matter physicist Philip W. Anderson often points out) you knew everything there was to know about the behavior of an electron. Suppose you systematically listed all of its attributes. You would learn quite a lot—you could calculate the ways in which one electron scatters off another and a myriad of other interesting effects. And yet, Anderson insists, you would not find among the listed properties those behaviors that emerge only when many electrons act in concert—you would not find the astonishing aspects of superconductivity, for example, that bizarre state of matter where electricity flows through matter with no resistance at all. If you list the properties of an electron, superconductivity is not among them; it is the wrong kind of thing to expect in the list of a particle's properties. (One needs pairs of electrons, and in great quantity.) The reductionist physicists reply that it is true that you might not guess these collective behaviors; but if you ask *why* very cold copper superconducts, the answer includes nothing other than electrons and electrodynamics—there's no magical supplementary thing over and above these.

At an even more abstract level, the mathematician points out that there are certain facts about spaces that simply aren't ascertainable locally: an ant on a short ant-leash never, in his peregrinations on a little bit of the surface of a doughnut, *ever* discovers that there is a hole. Topological features—like the hole in a manifold—are simply not discoverable through local inquiry. What does the microhistorian *not* see? Here's the question:

Problem 8: Globality. There seem to be aspects of scientific practice that simply do not reduce to the local. Look too closely at particulars and you won't understand the creation of scientific languages that don't arise in the head of any single researcher. Examine one particular laboratory with too much magnification and you won't see the building up of ways of being a scientist—the scientific persona, changing over time, is not an individual's invention. (For example: Should a scientist be more like an industrialist, a sage, a divine, an artist, or an entrepreneur?) These larger, normative roles, techniques, and methods are not just misunderstood: they are invisible when the view is too close. To the problem, then: What aspects of scientific practice, scientific argumentation, and scientific self are not visible when looked at by a microstudy? Why?

PROBLEM 9: RELENTLESS HISTORICISM[7]

The nineteenth century historicized everything. The Bible got a history, the earth got a history—so did insects, animals, plants, and even *Homo sapiens*. Language was of course historicized; nothing seemed immune to the alterations of time. Shockingly, as Nietzsche insisted, morality itself was not the record of transcendental good but instead very much the outcome of earth-bound struggles over power and legitimation. Historicism claimed that there was no getting outside history, where history was the evolving sea in which everything swam.

Historicism, as usually understood from Hegel on down, leaves no room for philosophy to resolve matters by reference to universal and unchanging principles. Philosophers and their systems walk here on earth, Hegel insisted, and do not float high above in the superlunary realm. Bringing historicism to the relation of philosophy and history in HPS, one could pursue the relation of science and philosophy in either of two ways: both are interesting, one is more commonly found than the other.

Given that philosophy is often taken to be part of the context in which science is done, can one narrate the history of science in a way that makes philosophy part of the historicized "surround"? Einstein grew up paying quite a bit of attention not only to the physics/philosophy of Ernst Mach but also to Immanuel Kant, John Stuart Mill, and Arthur Schopenhauer. Similarly, historians of physics have made much of the way Niels Bohr used the ideas, directly and indirectly, of Søren Kierkegaard as he formulated his principle of complementarity. These accounts are important. "Philosophy as a context for physics" as a directionality of explanation has, rather surprisingly, survived relatively unscathed the big shifts from logical positivism to antipositivism (the generation of Kuhn and his followers) and again from Kuhn to much of science studies.

But that the ways in which philosophers themselves exist are in significant measure shaped by their embeddedness within material, political, or scientific developments is recognized less often. More specifically: more accounts of the development of science than I can count put Ludwig Wittgenstein on a transhistorical pedestal and use his claims (of family resemblance or of continuing a series) as an unmoved prime mover, wisdom without origin. And this leads us to the twinned problems:

[7] **Relentless Historicism (Problems 9a and 9b).** There is not an extensive literature on historicism within history and philosophy of science, but for the agency-structuralism debate within social history see, for an opening into the problem, Perry Anderson, *Arguments within English Marxism* (London: Verso, 1980); and E. P. Thompson, *The Poverty of Theory and Other Essays* (London: Merlin, 1980).

Problem 9a: Relentless Historicism. Is it possible to write a history and philosophy of science in which the story told truly is historicist? That is, can there be a history with no transcendental "theory package" that escapes historicization? What if we can't lean on the crutch of a pre-established philosophical framework into which the history is inserted? What if we can't take as a permanent given the positivist framework in which it is taken for granted that in the beginning is experience, nor even the logical positivists' framework in which there are elementary bits of experience and the predicate logic? What if we can no longer invoke Wittgenstein or Kuhn or Peirce as the scaffolding on which historical detail is to be pitched? Such a fully historicized project would no doubt be hard to write. Philosophy, small group sociology, semiotics, anthropological "culture"—these would not be "givens" but instead would play out as part of the historical field, though with different rhythms and breakpoints. The challenge: Is it possible to write a history and philosophy of science with no day pass from history, one where the philosophy enters the stage *with* the history, not *before* the account begins? That would truly be a relentless historicism.

Or perhaps the problem should be stated the other way around:

Problem 9b: Escape from History? There are strong countercurrents to historicism. Structuralists, for example, have for several generations argued against the view that agency can be coherently and completely prosecuted in historical accounts. The self-anonymizing collective of French mathematicians who called themselves Bourbaki despised the idea of assigning "meaning" to mathematical objects and disdained any attempt to bring history or philosophy to bear on the nature of mathematical objects. Similarly, Gottlob Frege rejected any form of "psychologism"—he insisted that we do not gain the number concept by counting, with J. S. Mill, pebbles on the beach. And within the human sciences structuralists (and more recently poststructuralists) derided historical accounts that were supposed to be complete and causal. Call the question: Lay out that part of historical explanation that is *not* itself subject to historicization and argue the case why not.

PROBLEM 10: SCIENTIFIC DOUBT[8]

Finally, we come to one last question—one that, like so many others, could have been asked long ago but arises now in a particularly intense form. The search for scientific certainty runs deep, but the problem of uncertain knowledge does as well. Historians and epistemologists have grappled with science caught between the two; *scientia media*—neither deductively certain nor arbitrary—pained the seventeenth-century Jesuits and Dominicans as few other questions did. The history of probability and statistics is, in a certain sense, the hunt for scientific knowledge that was neither absolutely, apodictically true nor a matter of arbitrary assertion. That is, probable reasoning aimed at knowledge in

[8] **Scientific Doubt (Problem 10).** On manufactured doubt in the three instances of climate, tobacco, and evolution versus intelligent design/creationism see Naomi Oreskes, "Beyond the Ivory Tower: The Scientific Consensus on Climate Change," *Science*, 2004, *306*:1686; Allan M. Brandt, *The Cigarette Century* (New York: Basic, 2007); Robert Proctor, *Cancer Wars: How Politics Shapes What We Know and Don't Know about Cancer* (New York: Basic, 1995); Edward Humes, *Monkey Girl* (New York: HarperCollins, 2007); and Londa Schiebinger and Proctor, *Agnotology* (Stanford, Calif.: Stanford Univ. Press, forthcoming).

the realm of the finite, neither infinite nor zero. But we now face another kind of doubt, doubt raised for its own sake, doubt as a tool of political intervention, doubt marshaled to thwart scientific consensus, block political action, and protect quite specific interests. And so the problem:

Problem 10: Scientific Doubt. The debate over the dangers of smoking tobacco continued for a long time, but industry took pride—in secret, of course—in boosting that uncertainty: "Doubt is our product" became a guiding slogan of the tobacco lobby. "Teach the controversy," militated the Creationists and Intelligent Designers, demanding that their version of the beginning of things be taught alongside Darwin. The "global warming controversy" became a major political talking point for those bent on blocking international accords. But this appropriation of doubt, this use of doubt as a weapon, raises difficult questions for HPS: What is controversy? What is scientific doubt in a world where it can no longer be treated purely as an offshoot of this or that scientist's research? What role does HPS have in handling such matters, when one of the standard means of research—examining controversy—would itself reiterate and reinforce one side in a political confrontation with major consequences?

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These questions, of course, draw on a myriad of writings. I apologize for including only the briefest of notes indicating some starting points. But perhaps a few of these problems might provoke some interesting thoughts and debates—and that would be a very good thing.