New Frontiers in Science and Technology Studies

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Introduction

Science and Technology Studies (STS) is an interdisciplinary field usually defined as the confluence of three fields with distinct intellectual lineages and orientations: *history of science*, *philosophy of science*, and *sociology of science*. All three have been marginal to their named disciplines because surprisingly few of the original practitioners of history, philosophy, or sociology of science were primarily trained in history, philosophy, or sociology. Rather, they were natural or exact scientists who came to be disenchanted with the social entanglements of their chosen fields of study. In effect, they fell victim to an intellectual bait-and-switch, whereby the reasons they entered science failed to explain science's continued support in the wider society. This point often makes STS appear more critical than many of its practitioners intend it to be. STS researchers are virtually in agreement that people tend to like science for the wrong reasons (i.e. they are too easily taken in by its hype), but relatively few STS researchers would thereby conclude that there are *no* good reasons to like science.

There have been three generations of STS research, and each tells a different story of disenchantment with science. First, the logical positivists, including Karl Popper, were keen to protect the theoretical base of science from the technological devastation that was wrought in its name during World War I. Next, Thomas Kuhn and his contemporaries on both sides of the Atlantic – including Paul Feyerabend, Imre Lakatos, Stephen Toulmin, Derek de Solla Price – tried to do the same vis-à-vis World War II, though,

more than the previous generation, they relied on science's past for normative guidance, largely out of a realization of technology's contemporary role in scaling up the scientific enterprise and giving it forward momentum. Finally, the leading lights of the sociology of scientific knowledge – be they aligned with the Edinburgh School (David Bloor, Barry Barnes, Harry Collins, Steve Shapin) or the Paris School (Bruno Latour, Michel Callon, Steve Woolgar) – came of age during the Cold War and the decolonization of the British and French empires. While the Cold War's strong state-centered science policies kept alive what remained of the classical ideal of science governed by a univocal sense of rationality, objectivity and validity, the processes of decolonization presaged science's "postmodern" turn: namely, the devolution of funding and legitimation to reflect local cultural interests. In these diminished circumstances, much like the state itself, science has been forced to explicitly articulate its import in much more specific ways.

There is also a history, a philosophy and a sociology *of technology*, to which a similar response to world events applies. However, it is harder to forge a coherent narrative because many of the people who are now regarded as major contributors to these fields (e.g. Oswald Spengler, Lewis Mumford, Jacques Ellul, Herbert Marcuse, Marshall McLuhan) developed their signature views with an eye more to the general public than specialized academics, even when they were professional academics. In this context, STS has usefully resurrected these earlier inquiries as part of its own emergent focus on science as a "material practice," also known as "technoscience." However, this technological turn tends to underplay the distinctly *ideological* uses of science – that is, what science means and does to and for people, both elite policymakers and mass publics, who are *removed* from its material practices. Most of this book is concerned with redressing the balance on this point.

So far I have recounted the history of STS from an "external" standpoint, but there is also a more familiar "internal" story that STS practitioners tell themselves. This narrative recounts historical and ethnographic studies of "science in action" (Latour 1987) that from, say, 1975 to 1995 gradually falsified normative accounts of science advanced by philosophers, who themselves over this period had become more responsive to empirical (aka naturalistic) arguments. In this respect, the "science wars" of the mid-1990s were perfectly timed, as public questioning about science's disposition in the post-Cold War era coincided with the lowering of philosophical defenses (Fuller 2006a: chs 3–4). However, this internal story, which is now widely shared even by philosophers themselves, ignores the fact that science had been subject to special philosophical attention in the first place more because of what it justifies than what justifies it. On both the technological and the ideological front, science's power as a form of knowledge has rested on its ability to justify practices that might otherwise appear illegitimate. This speaks to the transformative character of science – indeed, as we shall see in these pages, even of the human condition.

I have entitled the book *New **Frontiers** in Science and Technology Studies*, but ultimately there is only one frontier that STS needs to confront. Let me first put it in philosophical shorthand: *what is the normative import of contingency, rather than necessity, as the modality for making sense of science and technology?* Science – and technology regarded as an application of science – has been treated in modern society as the standard-bearer of rationality, objectivity, and validity, the three normative categories most closely associated with "being realistic." This exalted treatment has rested on the idea that science develops according to an internal dynamic that is relatively unaffected by changes in the larger social environment. It still animates most popular science writing, which

effectively obscures any awareness of who is already paying for and is most likely to enjoy the benefits (and suffer the costs) of scientific research.

But what happens once science's "internal dynamic" is revealed as nothing but a strategically focused version of the contingencies that affect the rest of social life? Indeed, what if it turns out that the border between science and politics requires constant maintenance by the propagation of authoritative histories and philosophies of science that remain conspicuously silent – indeed, self-censoring – on political matters? This is the frontier that STS needs to negotiate. It helps to explain the field's controversial nature.

There are two basic attitudes one can take toward the contingency of science, once recognized. STS encompasses both of them. The first is that such recognition should not alter our fundamental attitude toward science, since science works on its own terms, a point to which scientists amply attest, even if it could have followed any of several trajectories. It follows that the contingency of science is of "merely philosophical interest," in Wittgenstein's sense of an understanding of world that "leaves it alone." When STS presents itself as an aspiring discipline in search of academic legitimacy, this attitude tends to hold sway. The second attitude interprets such contingency to imply that the course of scientific inquiry could be substantially altered in the future, if only because at various points in the past it could have gone in a variety of directions. In that respect, there is everything to play for by attending to certain features of contemporary science that might promote change in what is perceived to be a desirable direction. When STS presents itself as a political player, perhaps even a social movement, such an attitude toward contingency then prevails.

The contrast in sensibility here is captured in the two senses in which the practices of science might be said to be "conventional": they are either "traditional" or "reversible."

Readers not already familiar with my earlier work on *social epistemology* will discover in these pages that my own approach to STS falls squarely under the second category. I see STS as integral to the normative reorientation of science and technology that is taking place in our so-called postmodern times. If "science" still refers to the most authoritative form of knowledge in society, what is *now* the basis for that claim – and what are its consequences for policy, and life more generally? In breach of interdisciplinary tact, I believe that STS can address this question most effectively in terms of a particular chain of intellectual command. (For a precedent from the annals of logical positivism, see the tripartite sequence of tasks for "epistemology," in Reichenbach 1938: 3–16.) This ideal hierarchy would have historians mine the ore of science and technology by recovering its traces from the archives, philosophers smelt it in a usable form as they convert the historians' narratives to durable multipurpose theories and concepts, and sociologists finally market these refined products to policymakers and the general public.

This book is divided into three parts that divide the field of STS into three fundamental problems: *demarcation*, *democratization* and *transformation*. The first concerns the conceptual space occupied by "science" in our culture. The second concerns the political organization appropriate to science in society. The third concerns the material horizons within which we want science to change our world, including ourselves. Each part successively shifts the burden from the "science" to the "technology" poles of STS.

Part I is concerned with what philosophers call science's *demarcation problem*. The word "demarcation" implies that regardless of how one defines science in conceptual terms, the problem remains of distinguishing it from its concrete rivals and imitators. This problem is itself distinctive to science because of the many traditions that in principle can lay claim to producing authoritative knowledge in society. I

divide this problem into science's search for a *revolutionary moment* and a *unifying vision* – on the one hand, how science breaks from rival traditions and, on the other, how science becomes incorporated into a tradition in its own right. These are the respective topics of chapters 1 and 2, which together serve to introduce the reader to the modern history of cross-disciplinary discussions on the nature of science. A theme common to both chapters is the university as the site of a dialectic between science as a social movement and as a disciplinary formation.

One aspect of this discussion carries over into Part II, which is concerned with an STS-inspired discussion of what it means to democratize science in our times. Karl Popper sometimes recast the demarcation problem in political terms as the search for the *open society*, an endlessly self-critical and ever inclusive community of inquiry. But how should such a community be constituted, especially in light of the various historical conceptions of democracy, as well as contemporary demands to incorporate peculiar subjects in the science polity, notably the non-human natural world? Chapter 3 examines the metaphysical bases and political implications of two alternative democratizing strategies: Bruno Latour's *politics of nature* and my own *republic of science*. Although Latour's normative sensibility is *prima facie* more pluralistic than my own, his strategy for constituting a "parliament of things" may foster the dehumanizing consequences associated with the extreme forms of "species egalitarianism" of the animal rights movement. As for my own republic of science, it may be possible only under a limited range of political economies, the implication of which is that the scale and scope of science today is too large to be governable and must therefore be institutionally reduced.

The rest of Part II brings the discussion of democratizing science down to earth. Chapter 4 deals with what I call the *critical deficit of science journalism*, which amounts to the

presumption that, politically speaking, the “good guys” do good science, and the “bad guys” do bad science. One of the clearest expressions of this presumption is *The Republican War on Science* (Mooney 2005), a recent best-seller among the disenfranchized liberal elites in US politics. To redress the critical balance, I urge a strong dose of what STS calls “symmetry,” namely, unless proven otherwise, the treatment of the ethical character of scientists and the epistemic character of their science as independent of each other. Chapter 5 shifts the focus to the emergence of *research ethics*, especially against the backdrop of the alleged rise in scientific fraud. I argue that the tendency here is to treat problems of science as a social system as if they reflected scientists’ personal moral failures. This occludes the genuine problem of *epistemic justice*, which relates to how one arrives at the standard by which scientific conduct is held accountable. In this context, I examine the controversy surrounding the publication of Bjørn Lomborg’s *The Sceptical Environmentalist*. This case provides an opportunity for elaborating contrasting regimes of epistemic justice – *inquisitorial* and *accusatorial* – that might be used as the basis for empowering a science court.

Finally, Part III turns to both science’s and STS’s role as participants in the technoscientific construction of global society – past, present and future. Chapter 6 presents technology as an evolutionarily adaptive feature of virtually all organisms, yet it is only with the advent of science as the West’s universal project of Enlightenment that technology starts to be seen as the infrastructure of a genuinely global society. I argue that this project is largely an extended and secularized version of humanity’s divine entitlement in the monotheistic religions. This entitlement has been put under severe strain, both metaphysically and politically, with developments in information technology and biotechnology in the second half of the 20th century. I consider how both provide the basis for a revival of the politics of *social*

engineering, about which STS has said remarkably little so far. Nevertheless, social engineering will be decisive in how humanity negotiates its position between the divine and the animal, an "essential tension" that STS tends to characterize as the *cyborg moment*, whose Cold War roots are explored.

Part III, and the book as a whole, concludes with a reflexive look at STS's own problematic position in the world today. The field straddles the divide between self-legislating democracy and client-driven consumerism. No doubt STS is a very useful instrument for policy research that produces a frisson by transgressing taken-for-granted distinctions, but perhaps at the cost of becoming a parasite with no intellectual integrity of its own. This chapter is an expanded version of the 2005 annual Nicholas Mullins Memorial Lecture in Science and Technology Studies at Virginia Tech, the university housing the largest STS graduate program in the United States, where I spent four tempestuous but fruitful years in the early 1990s.

Parts of this book have been substantially reworked from a "report on the state of knowledge" I was commissioned to write by UNESCO in 1999 for the introductory volume of its Encyclopaedia of Life Support Systems. I mention this point because its pretext is not unlike the critical survey that Jean-François Lyotard was commissioned to write for the Higher Education Council of Québec in 1979, which resulted in the "postmodern condition" coming to express the temper of our times. However, the spirit of my piece – and this book – is the exact opposite of Lyotard's: I draw on several strands of the history and sociology of philosophy and science to renew the case for knowledge integration in aid of a normatively unified conception of science. My own politics of knowledge reunification has precedent in both the German idealists and the logical positivists. In practical terms, the aim is to promote distinctly knowledge-based institutions, especially the university, as vehicles of democratic social

progress of potentially universal scope. STS so far largely stands outside this project. One goal of this book is to bring STS a bit closer to it.

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Part I

The Demarcation Problem

1

Science’s Need for Revolution

A paradoxical consequence of the emergence of a distinct field of inquiry called “Science and Technology Studies” (STS) is it that has helped to undermine the classical justification for just such a field. Originally it was thought that there was something unique about science as a social and intellectual practice that warranted a field, if not exactly STS, then at least relatively autonomous specialities in the history, philosophy and sociology of science. Some

described this uniqueness in terms of a set of necessary and/or sufficient conditions that all properly scientific practices share. Others pointed to a mode of succession that characterized an "internal history of science," in terms of which any pretender to the title of science had to demonstrate their legitimate descent (Lakatos 1981). Together the image projected was of a unified conception of science potentially traceable to a canonical origin, aka the *Scientific Revolution*. This classical strategy of justifying science came to be seen in the 20th century as solving the *demarcation problem* – specifically, the problem of demarcating science from non-science, or pseudo-science (cf. Remedios 2003).

The demarcation strategy is familiar from the history of political thought as akin to the genetic basis used to legitimize royal dynasties. However, in the case of science, philosophers sought demarcation criteria that could have been applied across all of history. When something similar has been urged in the sphere of politics, typically under the name of natural law, it has often resulted in calls to overturn the current regime on grounds of illegitimacy. In science, it has resulted in a relatively bloodless coup that now represents the orthodoxy in STS. It consists of a *de facto* acceptance of, on the other hand, a disunified conception of science – or, spun more positively, a recognition of the plurality of "sciences" – and, on the other, the mythical status of a definitive world-historic "Scientific Revolution" (Galison and Stump 1996; Shapin 1996).

A famous 1983 paper by Larry Laudan officially declared the problem's demise (Laudan 1996). There seems to be a broad consensus today among historians, philosophers, and sociologists that science is whatever scientists do – and if they do different things in different fields constituted by recognized scientists, then so be it. Yet, this is precisely the sort of solution that the original statement of the demarcation problem was designed to *prevent*. How can

what was so obviously wrong 50 years ago now seem so obviously right? I happen to believe that the demarcation problem is worth reviving today. In particular, there is a need for a "non-providential" account of the nature of science – that is, an account that does not presume that the dominant tendencies in the history of science are *ipso facto* normatively acceptable. STS's rejection of the demarcation problem may be understood as an overreaction that has thrown out the teleological baby with the providential bath water in making sense of the history of science.

This chapter provides an autopsy of the demise of the demarcation problem (cf. Fuller 1988: ch. 7). The first part offers perhaps the most accessible entry point into the problem of demarcation, namely, the historical moment when science came to be formally set apart from other forms of knowledge in society. This is the so-called Scientific Revolution, which allegedly happened in 17th-century Europe. This topic immediately opens up into a consideration of the most influential theorist of scientific revolutions, Thomas Kuhn, especially his impact on STS. In sections 2 and 3, I explore how one might justify demarcation criteria from a historical and political standpoint. Together they constitute the demarcation problem's "social epistemology" (Fuller 1988: esp. ch. 7). Section 2 traces the origins of the demarcation problem to the need to decide between competing definitions of knowledge from a neutral standpoint, modeled on a judgment delivered in a trial. In section 3, I flesh out the politics that inform this backdrop, drawing on Popper's discussion of open and closed societies.

1. The Scientific Revolution: The Very Idea

Although the expression "scientific revolution" is most closely associated with Thomas Kuhn (1970), who

embedded the phrase in a general theory of scientific change, it also names a specific time and place – Western Europe of the 17th century – from which descend the modern institutions, methods, theories, and attitudes of science, as epitomized in the achievements of such figures as Galileo, Bacon, Descartes, and, most of all, Newton. Interestingly, the idea of localizing *the* Scientific Revolution dates only to the 1940s, when both the British historian Herbert Butterfield, known for his progressive "Whig interpretation of history," and Kuhn's own mentor in historiographical matters, Alexandre Koyré, an émigré Russo-French philosopher influenced in equal measures by Plato and Hegel, started to speak in these terms (Fuller 2000b: 23).

The use of the same phrase "scientific revolution" in Kuhn's general and Butterfield's and Koyré's more specific senses is only partly justified. The specific coinage was intended to be provocative. It was an anti-Aristotelian and anti-Catholic gesture designed to consign the Renaissance to a pre-modern past that was superseded by the revival of a Platonic theory-driven science (Koyré) and the Protestant Reformation of the Christian conscience (Butterfield). These crucial elements of the modern scientific imagination had been held back by the demands of secular governance and everyday life. Thus, Koyré contrasted two Italians who had been previously seen in much the same light: Galileo's single-minded pursuit of a unified truth marked him as a scientist, whereas Leonardo da Vinci's jack-of-all-trades empiricism did not.

The rhetorical force of the distinction between the likes of Galileo and da Vinci was not lost in the postwar period. In the aftermath of two world wars that implicated science in the manufacture of weapons of mass destruction, the future integrity of science required that it be seen as having historically revolted not only against religion but, perhaps more importantly, technology. Thus, the Scientific

Revolution supposedly marks the moment when philosophers came to regard technology as an appropriate means for testing their theories without being seduced by technology's potential as an instrument of domination. In the more metaphysical terms with which both Butterfield and Koyré were comfortable, the Scientific Revolution was about matter coming under the control of spirit, the passions subsumed by reason.

However, the historical identification of the Scientific Revolution causes problems for the periodization of European cultural history that became popular at the end of the 19th century and still prevails, at least in popular treatments. It casts the early modern period as opening with a "Renaissance" that eventuated in an "Enlightenment." The Scientific Revolution supposedly happened at some point between these two epochs – perhaps when they overlapped in the 17th century. Yet, the import of the Scientific Revolution is seriously at odds with the narrative that postulates the Renaissance and the Enlightenment as consecutive stages in history. As represented in Kuhn's *Structure* and elsewhere, the import of the Scientific Revolution is that a group of people, whom we now call "scientists," managed to wrest control of the means of knowledge production from the politicians, religious fanatics, and others who made it impossible to pursue The True independently of The Good and The Just. This autonomization of inquiry epitomizes all the perceived benefits of academic disciplines. They include: (1) secure borders for inquiry that keep larger societal demands at a distance; (2) common standards for incorporating new members and topics, as well as for evaluating their efforts; and (3) discretion over the terms in which the concerns from the larger society are translated into "new" problems.

Yet this "order out of chaos" narrative fails to do justice to the progressive spirit of the figures normally identified with the Renaissance and especially the Enlightenment. These

figures – Galileo and Voltaire come most readily to mind – relished whatever immunity from censure they enjoyed but did not generally associate it with the self-restraint, even self-censorship, that is alleged to be a hidden source of power after the Scientific Revolution. Rather, this period (roughly 1400 to 1800) marked the emergence of the *arts of explicitness*, including such wide-ranging pursuits as satire, the quest for a language of pure thought, and indeed, experimental demonstration.

To be sure, the religious wars of the 17th century made Britain sufficiently dangerous to justify the non-sectarian declarations contained in the Charter of the Royal Society (Proctor 1991: ch. 2). However, it is all too easy to project into the past contemporary anxieties about the potential fate of dissident scientists. Indeed, issues of “respect” and “legitimacy” loomed so large in the early modern era because would-be autocrats were often incapable of enforcing their will in the face of resistance. On the one hand, the autocrats lacked the necessary means of surveillance and coercion and, on the other, potential dissenters were not exclusively dependent on a particular autocrat for material support of their work. Together these two conditions ensured that intellectuals could maintain their autonomy by moving between patrons.

The problem of identifying a Scientific Revolution was raised to a problem of global history with another postwar project: the multivolume comparative study of “science and civilization” in China undertaken by the British Marxist embryologist Joseph Needham (Cohen 1994: ch. 6). China was Europe’s economic superior until the early 19th century, yet it had never passed through a scientific revolution. Europe’s “Industrial Revolution” – a phrase coined in the 1880s, a century after it purportedly began – initiated the systematic development of technology by scientific design. Up to that point, technologies across the world had emerged by means that, for the most part, were

innocent of science to such an extent that aspiring innovators had to be accepted into an esoteric craft culture because the relevant knowledge was not seen as the common entitlement of humanity.

In contrast, the idea of science in its modern hegemonic sense presupposes that *all* humans enjoy a privileged cognitive position in nature (that at the moment may not be fully realized), a status associated with the great monotheistic religions descended from Judaism but not those of the East, where humans were seen more as one with the natural world. The idea that humans might transcend – rather than simply adapt to – their natural condition so as to adopt a "god's eye point-of-view," especially one that would enable the "reverseengineering" of nature, was profoundly alien to the Chinese way of knowing. In this respect, the Scientific Revolution marked a revolt against nature itself, which was seen as not fully formed, an unrealized potential. Francis Bacon's account of experimentation famously expressed this sensibility as forcing nature to reveal her secrets, namely, possibilities that would not be encountered in the normal course of experience.

The idea of humanity giving a divinely inspired reason to nature had become widespread in the West by the late 18th century, especially after Newton's achievement moved philosophers – not least those behind the American and French Revolutions – to envisage society as something designed *ex nihilo* on the basis of a few mutually agreeable principles, what continues today as "social contract theory." In this context, the pre-contractarian "natural" state of humanity appears unruly because its wilder animal tendencies have yet to be subject to a higher intelligence, secularly known as "rationality" (Cohen 1995).

The joining of political and scientific revolutions in this radical sense is due to the Enlightenment *philosophe* most responsible for the rise of social science, the Marquis de

Condorcet (Fuller 2006b: ch. 13; cf. Baker 1975). He specifically connected the successful American Revolution and the ongoing French Revolution via the rhetoric of the first self-declared scientific revolutionary, Antoine Lavoisier (Cohen 1985). Lavoisier had recently reorganized chemistry from its traditional alchemical practices into a science founded on the systematic interrelation of atomic elements. However, Lavoisier himself was not an enthusiastic supporter of revolutionary politics, unlike his great English scientific rival, Joseph Priestley, whose radical Unitarian theology forced him into exile in the newly constituted United States, where he was warmly received by the Founding Fathers (Commager 1978: ch. 2). As Priestley celebrated the French Revolution in exile, Lavoisier was guillotined by the revolutionaries at home.

Lavoisier believed that a scientific revolution would stabilize (rather than dynamize, as Priestley thought) the social order. Here he fell back on the classical conception of "revolution," suggested in the Latin etymology, as a restoration of equilibrium after some crime or period of political unrest. Specifically, Lavoisier opposed Priestley's continued support for the practically useful, but logically confused, concept of "phlogiston," the modern remnant of the ancient idea that fire is an ultimate constituent of nature. In this context, Priestley is best seen as an epistemic populist, much like the positivist philosopher-physicist Ernst Mach who, a century later, wanted scientific judgment to be grounded as much as possible in practical experience, as opposed to theoretically inferred entities that only an expert class of scientists might observe (Fuller 2000b: ch. 2).

Kuhn's relevance as a theorist of scientific revolutions emerges at this point – and not only because his own most carefully worked out case of a scientific revolution was the dispute between Priestley and Lavoisier over the nature of oxygen. Kuhn also agreed with Lavoisier that revolutions mainly restored stability to a science – and by implication a

society – fraught with long unsolved problems. Kuhn portrays scientists as the final arbiters of when their knowledge has sufficiently matured to be applied in society without destabilizing it. This doubly conservative conception of revolutions reflects Kuhn's definition of science as dominated by only one paradigm at any given moment. Consequently, despite Kuhn's broad cross-disciplinary appeal, especially among social scientists, Kuhn consistently maintained that only the physical sciences satisfy his strict definition because it is only in these fields (and arguably only until about the 1920s) that scientists are in sufficient control of the research agenda to determine when and how a revolution begins and ends, and its results spread more widely.

Kuhn's conception of scientific revolutions appeared radical in the late 1960s because it was conflated with the then-prevalent Marxist idea of revolution as an irreversible break with the past, something closer in spirit to Condorcet's original conception (Fuller 2000b: ch. 5; Fuller 2003a: ch. 17). This conflation was facilitated by Kuhn's portrayal of scientists in the vanguard vis-à-vis the direction of their own work and its larger societal import. This image was in marked contrast with the perceived captivity of scientists to what C. Wright Mills called the "military-industrial complex."

However, Kuhn's own reluctance to engage with his radical admirers suggests that his model was proposed more in the spirit of nostalgia than criticism and reform. This interpretation is supported by the original Harvard context for the restorative conception of revolution, the so-called Pareto Circle, a reading group named after the Italian political economist Vilfredo Pareto, whose "circulation of elites" model was seen in the middle third of the 20th century as the strongest rival to Marx's theory of proletarian revolution. This group was convened in the 1930s by the biochemist Lawrence Henderson, who taught Harvard's first

history of science courses and was instrumental in the appointment of chemistry department head, James Bryant Conant, as university president (Fuller 2000b: ch. 3). In that capacity, Conant hired Kuhn not only as a teacher, which enabled him to develop the more general ideas for which he would become famous, but also as a researcher on the origins of the Chemical Revolution, which eventually gave Kuhn's general thesis about scientific revolutions what empirical credibility it has (Conant 1950).

1.1. Deconstructing the myth of Kuhn as revolutionary

STS's biggest blindspot is its lack of reflexivity (Fuller and Collier 2004: esp. Introduction): unless STS researchers already come to the field with, say, feminist, Marxist or post-colonialist identities, they tend not to reflect on the conditions that maintain their inquiries. To be sure, there was a spell in the 1980s when some prominent British STS researchers devoted considerable attention to a very narrow sense of reflexivity, namely, linguistic self-reference (e.g. Woolgar 1988, Ashmore 1989). In retrospect, it can be seen as a relatively late adoption of Jacques Derrida's "deconstructive" textual criticism, whereby an author is caught in a pragmatic contradiction between the content and the context of her textual utterance. This serves to destabilize the meaning of the text, thereby placing the author's authority "under erasure" (cf. Culler 1982). Since most texts self-deconstruct under such intensive scrutiny, STS researchers rarely bother to attend to a more sociologically informed sense of reflexivity, which would interrogate, say, the extent to which STS itself is captive to an airbrushed disciplinary history that it so easily spots in other fields. (An ironist – in the very spirit of the 1980s reflexivists! – might argue that Derridean self-immolation was the only dignified way out of the Thatcherite straitjacket available to British academics.)

Thus, while some prominent STS practitioners (starting with Restivo 1983) have seriously questioned the ideological function served by the popularity of Kuhn's historiography of science, they have failed to alter the general perception that Kuhn turned the history and philosophy of science in the more critical direction from which STS emerged (e.g. Sismondo 2004). This tired tale of Kuhn's ascendancy depicts Kuhn as the one who overthrew the logical positivist hegemony in philosophy of science by demonstrating that science is a historically embedded collective activity that is not reducible to the strictures of mathematical logic and the probability calculus. Yet it is incredibly easy to puncture holes in this myth (my own demolition job is Fuller 2000b). Kuhn never attacked the logical positivists, mainly because he regarded their project as orthogonal, or perhaps even complementary, to his own. Indeed, the positivists were sufficiently pleased with *Structure* to publish it in their own book series. Moreover, at the time of *Structure's* composition, Kuhn admitted to knowledge of only the most general features of positivist doctrine. Most of what Kuhn learned about logical positivism and its analytic philosophical offspring occurred *after* the publication of *Structure*, once philosophers interpreted the book as relevant to their own ongoing problems and started engaging with its author.

An even more glaring hole in the myth is Kuhn's alleged uniqueness in drawing attention to the historical and social dimensions of science. As a matter of fact, the entire lineage of people we normally call philosophers of science, from Auguste Comte and William Whewell in the 1830s to Otto Neurath and Karl Popper in the 1930s, were preoccupied with the socio-historical dimensions of inquiry. The very use of the word "science" (and its cognates), as opposed to a more generic term like "knowledge," signified the recognition of an activity pursued by many people over a long time, not a solitary individual staring at a fixed

object. The open question for these philosophers – many of whom were practicing scientists – was how inquiry should be organized to maximize knowledge production. The various movements that have travelled under the rubric of "Positivism" since the early 19th century have been the main locus for addressing this question, though it has also figured significantly in the major academic philosophical schools – Kantianism, Hegelianism, Pragmatism – and of course positivism's great rival for the political left, Marxism. Moreover, the question has remained the focus of my own project of "social epistemology."

In all of the above cases, it was supposed that the optimal organization of inquiry would expedite social progress more generally. However, as heirs of the 18th-century Enlightenment, these philosophers equally believed that every aspect of the actual history of science should not be treated as normatively desirable – as if every secular error were a sacred virtue in disguise. That would reduce the history to theodicy (i.e. the theological attempt to prove that, no matter how bad things seem, this is "the best of all possible worlds"). Instead, these philosophers held (rightly, in my view) that intellectual maturity comes from recognizing that error is real but reversible. Of course, this sense of "maturity" might entail radical political consequences, especially if one were to argue that scientific – and hence social – progress has been retarded by traditional institutions like the Church or (as Popper's student, Paul Feyerabend, notoriously argued in our own day) the scientific establishment itself.

The key point here – one easily lost when Kuhn is taken to be the source of all philosophical interest in science's socio-historical dimensions – is that one may cultivate a deep and sustained interest in the actual history and sociology of science yet still find much of it wanting, according to standards that are believed (themselves for good socio-historical reasons) to be capable of expediting the progress

of both science and society. This was the natural attitude of philosophers of science before Kuhn. It is why the logical positivists, even as they were trying to recast physics in mathematical logic, were also supporting and sometimes even conducting studies into the history and sociology of science.

Indeed, the positivists' "formal" and "informal" projects were interrelated. The need to translate science into a neutral formalism for purposes of systematic evaluation was born of the ideological freight that normal scientific language had come to carry as a result of its secular entanglements with the German military-industrial complex in World War I. For Germans living with the memory of a humiliating defeat, science represented everything that had been wrong with the war strategy. Thus, as torch-bearers for the Enlightenment, the logical positivists saw their task as preserving the spirit of science from its ideological and technological corruptions, so that scientific hypotheses can continue to be given a fair hearing. In the irrationalist culture of the Weimar Republic, this task may have been futile but it was not based on ignorance of the socio-historical dimensions of science.

However, the image of the logical positivists changed, both drastically and deliberately, once the Nazis forced them – as intellectuals who were leftist, cosmopolitan, and/or Jewish – into exile in the English-speaking world. The ones who migrated to Britain, notably Neurath and Popper, retained their political edge and overarching sense of science as a vehicle of social progress (though Popper's leftism gradually drifted from socialism to liberalism). However, the vast majority who migrated to the United States stuck to the cultivation of scientific formalism, leaving the history and sociology of science for others to pursue as separate fields. This self-restraint may be explained by the positivists' desire for assimilation, which inhibited them from engaging in research likely to lead to a

critique of their hosts' socio-epistemic authority. It was in this spirit that the logical positivists, following the example of junior member Carl Hempel, rebranded themselves as "logical empiricists": the old phrase had suggested a continental European conspiracy to use science to launch a social movement, whereas Hempel's neologism evoked more politically correct roots in such genial Brits as Locke and Hume.

As it happened, the logical positivists were not overreacting by going "deep cover," the phrase used for undercover police work when the agents expect no backup if they are caught. Reisch (2005) has shed new light on the paranoid climate of Cold War America, in which many positivists – including their doyen Rudolf Carnap – were investigated by the FBI. Nevertheless, in their American captivity, the logical empiricists brought an unprecedented level of professionalism and technical sophistication to academic philosophy. By 1960, the memories of secular preachers with public missions, like William James and John Dewey, were in American philosophy's mildly embarrassing, pre-scientific past. (This was still the taken-for-granted view when I did my PhD in history and philosophy of science in the early 1980s at the University of Pittsburgh, the last great positivist citadel, where Hempel was one of my teachers.)

Kuhn introduced a domesticated account of the history and sociology of science into the repressive Cold War intellectual environment. Unlike the original 19th-century visions of science advanced by positivists, idealists, and Marxists, Kuhn's account portrayed the course of organized inquiry as not merely autonomous but, more importantly, *detached* from larger socio-historical developments. In particular, science no longer appeared as the engine of social reform, a potential challenger to the ruling orthodoxy. Rather, science was subject to its own self-regarding cyclical dynamic, the phases of which – "normal science,"