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The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology

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The abrupt reversal of culturally ascribed primacy in the science–technology relationship—namely, from the primacy of science relative to technology prior to circa 1980, to the primacy of technology relative to science since about that date—is proposed as a demarcator of postmodernity from modernity: modernity is when 'science' could, and often did, denote technology too; postmodernity is when science is subsumed under technology. In support of that demarcation criterion, I evidence the breadth and strength of modernity's presupposition of the primacy of science to and for technology by showing its preposterous hold upon social theorists—Marx, Veblen, Dewey—whose principles logically required the reverse, viz. the primacy of practice; upon 19th and 20th century engineers and industrialists, social actors whose practical interests likewise required the reverse; and upon the principal theorizers in the 1970s of the role of science in late 20th century technology and society. The reversal in primacy between science and technology ca *1980 came too unexpectedly, too quickly, and, above all, too unreflectively to have resulted from the weight of evidence or the force of logic. Rather, it was a concomitant of the onset of postmodernity. Oddly, historians of technology have remained almost wholly unacknowledging of postmodernity's epochal elevation of the cultural standing of the subject of their studies, and, specifically, have ignored technology's elevation relative to science. This I attribute to the ideological character of that discipline, and, specifically, to its strategy of ignoration of science.*

Keywords: Technology as Applied Science; Science as Technology; Revolts Against Science; Modern; Postmodern; Heidegger; Marx; Sombart; Bukharin; Veblen; Dewey; Mumford; Lorenzen; Daniel Bell; Lyotard; Erlanger Schule; Starnberger Schule; Finalization; Romanticism; Lebensphilosophie; Technological Determinism

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Liberation of our conception of technology from the functional dependence and cultural inferiority implied by 'applied science' was a principal constitutive program of the discipline of the history of technology, and so it has remained through four decades to the present day. When the historians of technology first began to revolt against 'the linear model' and its view of science as originative source, as unmoved mover, of technological progress, they were setting themselves against prejudices deeply entrenched in modern culture.[1] Meanwhile, however, the times have been achanging. To campaign today against the linear model is to throw oneself against a door that has been wide open for two decades. In the epochal global transformation from modernity to postmodernity that has been taking place in recent decades, technology has acquired, beginning about 1980, the cultural primacy that science had been enjoying for two centuries world-wide, and in the West for two millennia.

Of this postmodern reversal of primacy between science and technology there is no more apposite evidence than the shift of the center of interest in all varieties of 'science studies' from science to technology. That shift began about 25 years ago, and today, if one asks a historian or sociologist or philosopher of science what they are working on, odds are they will describe an inquiry at the center of which is technology. This is true even also of those who are now old enough to collect their pensions, and who, consequently, having begun their scholarly careers at a time when science's primacy was unquestioned, could not then have imagined ending it oriented as they are toward technology.[2] This turnabout has come about because we historians and sociologists and epistemologists have become so largely postmodern—not postmodernists necessarily or even generally, but *anima* adapted to postmodernity.[3]

In modernity, the cultural rank of science was elevated by that epoch's most basic cultural presuppositions—not merely the presupposition of the superiority of theory to practice, but more importantly the elevation of the public over the private and the disinterested over the interested, and, more importantly still, the belief that the means sanctify the ends, that adherence to proper means is the best guarantee of a 'truly good' outcome. Today, on the contrary, technology is the beneficiary, and science the maleficiary, of our pragmatic-utilitarian subordination of means to ends, and of the concomitants of that predominant cultural presupposition, notably, disbelief in disinterestedness and condescension toward conceptual structures.

Some evidence of this unprecedentedly high cultural standing of technology in recent decades is presented in Section I of this paper. More particularly, I argue there that only a sudden and drastic shift *ca* 1980 in cultural presuppositions could explain the evident inclination across the scholarly spectrum—from philosophers to sociologists to scientists—to ascribe to technology that primacy in role and rank that previously all had ascribed to science. *Did* all ascribe such primacy to science prior to postmodernity? In order to establish the fundamentality of the postmodern reorientation in cultural commitments, it is necessary to show that in the two centuries, at least, prior to *ca* 1980 the primacy of science to and for technology was a firm fixture in nearly everyone's thought, regardless of their philosophical commitments or social interests. Thus Section II, the bulk of this paper, is devoted to making out that case.

That effort is needful as there does not exist, to my knowledge, any scholarly exposition of the fact of the near universal belief in the primacy of science to and for technology prior to postmodernity. The absence of such an exposition is surely due in good part to the fact that a generation ago, when we now pensionable historians and sociologists of science began our careers by taking science as our subject, we would have regarded a proof of science's primacy as pointlessly pedantic. The transposition of our point of view has meanwhile taken place so fundamentally and so complacently that today a presupposed primacy of science to technology is conceivable only as a preposterous, even malicious, mythology.[4]

Although the primacy that science enjoyed in modernity—a primacy not merely with respect to technology, but in cultural standing quite generally—came to appear preposterous only through the perspective enjoined by postmodernity upon layman and scholar alike, not everyone in modernity approved of the fact that science enjoyed such primacy. Of that I take note repeatedly in the course of my exposition. Among the malcontents were the adherents of the emerging discipline of the history of technology, who in the course of the 1970s rejected the primacy of science and took as programmatic objective liberation of technology, and of themselves, from subordination to science. I show in Section III that as their principal means of effecting that end, historians of technology, as a group, chose ignoration of science, i.e. the exclusion of science from their purview and their histories (except for purposes of disparagement). In consequence of adopting that strategy, historians of technology have seriously distorted the views and motives of the subjects of their historical studies. They have also—and this is my main reason for examining their ideology here—blinded themselves to the reversal of primacy between science and technology, and therewith the epochal transition from modernity to postmodernity.[5]

In a subsequent publication I will turn to the question of how to account for the extraordinary unanimity, in modernity, that science unquestionably held primacy to and for technology. That subordination of technology to science in modernity points, as it seems to me, to the foundational importance for the modern mind of means, of process, of procedure. Contrary to a common view, the primacy of means logically does not, and in modernity did not, imply a high valuation of technology. Rather, the primacy of means implied a high valuation of science, for science was modernity's prime exemplar of progress through reliance upon a proper means, the scientific method. The commitment to 'methodism' was most obviously foundational for modern liberal democratic societies, but was by no means limited to them. Rather, Weber emphasized, the modern bureaucratic state, whatever its ostensive political constitution, is characterized and legitimated by the regularity and impersonality of its administrative processes, i.e. by the primacy of procedure. To be sure, modernity knew many manifestations of romantic antipathy to methodical procedure, but only rarely and briefly did anti-methodism hold sway. Today, however, in post-methodist postmodernity, the notion of a scientific method is regarded as naïve and out-dated because ends have regained the primacy that, prior to the Enlightenment, had been ascribed to them always and everywhere.[6] This postmodern primacy of ends is a principal reason for the striking reversal since about 1980 of relative rank

and role between science and technology, to the advantage of technology and the great disadvantage of science.

I. The Postmodern Primacy of Technology

> *Of course, it would be naïve*
> *simply to equate Postmodernism with technology.*[7]

A recently published semi-popular, hemi-historical work of reference, *American Women in Technology: An Encyclopedia*, includes a list of 'Women Nobel Laureates in Science and Technology'.[8] There is, of course, no Nobel prize for technology as such, and none of the individuals there listed would have regarded herself as technologist primarily. The conflation of science with technology that pervades this work, and more particularly the capability of the title word 'technology' to comprehend science as well, are typical for postmodernity—indeed, specific to postmodernity. Simplistically stated, the thesis of this paper is that modernity is when 'science' denotes technology too; postmodernity, when 'technology' denotes science too. More exactly, the cultural transvaluations that constitute postmodernity entail a reversal, in general discourse, of the denotative capacities of the terms 'science' and 'technology'. That is, in modernity the primacy of science to and for technology permitted the word 'science' to mean science *and* technology, but never—or almost never—permitted the word 'technology' to denote science as well. In postmodernity, with technology acquiring primacy, the word 'technology' gradually becomes capable of including science in its denotative compass.[9]

The capability 'science' possessed in modernity of standing also for and fully representing technology appears clearly in the late-modern term 'big science'. Introduced around 1960, the term was applied, almost without distinction, not only to large-scale enterprises whose nominal goals were mainly, if not wholly, within science (pre-eminently, accelerator-based research on elementary particles), but also to enterprises of a primarily technical, even politico-technical character (pre-eminently, getting a few Americans to the moon and back).[10] That is, in modernity science was conceived as so much the essence of every enterprise in which it had any role at all that a mere 'touch' of science sufficed to turn those enterprises into science of a sort. The desuetude of the term 'big science' since the early 1990s is thus an indication of the loss of belief that technological miracles are worked by the kingly touch of science. Yet that loss of belief did not result from having *learned* better—Section II will show the unteachability of modernity in this regard—but from *seeing* differently, i.e. seeing technology as the broader, more inclusive entity.[11]

Yet even though the extension of the meaning of the word 'technology' to include science may eventually become the clearest marker of the cultural shift from primacy of science to primacy of technology, it has not been an especially early or sensitive indicator of the onset of that shift. This is not surprising. The adjustment of discourse necessarily lags the emergence of new cultural presuppositions and their first expressions in practice. Thus even the earliest instances of the word 'technology' covering science are later than the first performances of the conflation of science with technology. So, for

example, in the early 1990s, Collins and Pinch wrote a popular book in the Latourian spirit of enlightenment, titled *The Golem: What Everyone Should Know About Science* (1993), in which all their examples are taken from basic scientific research and all their conclusions are about technology. However, their sequel, published five years later, *The Golem at Large: What You Should Know About Technology* (1998), while not different in subject, analysis, or conclusions, exploits the newly realized denotative potential of 'technology' to stand for both technology and science.[12]

So when did we 'change our thinking caps'? It is often suggested, by advocates and antagonists alike, that the origins of a reversal of primacy between science and technology lie in the Second World War.[13] Against that view stands the fact that the presupposition of the primacy of science with respect to technology emerged greatly strengthened from the Second World War.[14] More justifiable is to see the origins of this reversal of primacy in the cultural revolt of the 1960s. Yet although the undermining of the cultural standing of for-its-own-sake science was begun by the demand for 'relevance' that came forward so broadly and insistently in the 1960s, still, as I instantiate in Section II, the fundamentally modernist epistemological presupposition of the primacy of science to and for technology continued to govern ideational constructs and their rhetorical expression into the late 1970s. The last years of that decade saw the first indications of the reversal to primacy of technology—not, initially, in intellectualizations, but in sentiments regarding social life and the purposes of government as reflected in governmental science policy.[15] Only about 1980 did the rejection of the primacy of science to technology begin to take hold as presupposition for intellectualizations. The word 'technoscience', coined in French at about that time, well expresses the fact that the conflation of technology and science initially proceeded not from any especially high valuation of technology, but rather from a still modern preoccupation with science, and a still more old-fashioned disapprobation accompanying the newly arisen recognition of science's manifold entanglements with technology.[16]

The reconception of 'science studies' as being the scholarly study of an entity technology/science, without any difference or distinction between technology and science, and thus without any primacy imputed to science or subordination of technology, began to take place in the early 1980s. Stages in that reconception are clearly discernible in the writings of Bruno Latour, writings that brought him to center stage in 'science studies'. Thus *Laboratory Life* (1979) was still modern in its total scientocentrism. 'Give Me a Laboratory …' (1983) shows Latour reorienting his attention toward technology while still presupposing the preposterous primacy of science, presupposing it so completely that he did not even speak of 'technology', but took as the phenomenon requiring explanation the extension of scientific results stabilized in the scientific laboratory beyond the walls of the laboratory to encompass the world at large. Thus Latour was there obliterating the boundary between science and technology, but he did so from science outward, treating technology as super-stabilized science.[17] Somewhere in the following four years the relation between science and technology reversed itself for Latour. *Science in Action* (1987) telegraphs in its subtitle, *How to Follow Scientists and Engineers through Society*, that Latour is equating and conflating scientists and engineers, science and technology. As indication of a cultural shift, more significant

than this conflation itself is that it is almost totally implicit, taken for granted, as though needing no argument or justification. As indication of the equally implicit epistemic shift accompanying this cultural shift: where Latour does, so very briefly, consider science and technology as distinguishable entities, primacy is accorded to technology, now regarded as the greater miracle.[18]

The staggering contradiction to that primacy of science that even just ten years earlier had still been the very nearly universally received view goes unnoticed by the 1987 Latour. Yet however stylistically idiosyncratic *Science in Action* may be, Latour's representation of the relation between science and technology was anything but idiosyncratic. Quite the contrary: the huge success of that book must be ascribed to Latour's conflation of science and technology being right in line with the broad, largely unreflected-upon, modern-to-postmodern cultural reorientation: among 16 scholarly reviews of *Science in Action*, only three drew attention to Latour's conflation of scientists and engineers, and none took issue with it.[19] Nearly every other axiom of Latour's account was doubted by one or another of those 16, generally enthusiastic, reviewers.[20] However, this one, the equation and conflation of science with technology, fundamental to Latour's project and implied in his title, goes largely unnoticed and entirely unchallenged by the reviewers of his book. Evidently, by the late 1980s the presumption of the primacy of technology was well on its way to becoming just as universally received a view as the primacy of science had been through the preceding two centuries.

Not everywhere, however, was the primacy of technology received so thoughtlessly, and hence so universally, as in the Anglophone world. France sustains today the best organized and most outspoken philosophical–ideological contestation of the conflation of science with technology, in part because over the past three to four decades France has been the locus of the best organized and most outspoken philosophical–ideological advocacy of that conflation, with its always implicit and often explicit reversal of primacy between science and technology.[21] Thus what Latour with *Science in Action* was the first influentially to perform in English, was then already a familiar contention in France, and the term 'technoscience', which Latour pretended to invent, was already widely used there to signify that contention.[22]

Is this not surprising? Is it not, after all, in France that, as Pestre and Krige observed, scientists 'appear above all as the heirs to a tradition that continued to attach great importance to "pure" science and kept "applied" science separate', while, by contrast, in the USA 'between the 1930s and the 1960s … was the emergence of a profound symbiosis previously unknown in basic science, a fusion of "pure" science, technology, and engineering'.[23] This ironic circumstance suggests that the actual, factual relationship between science and technology is relatively unimportant—certainly not dispositive—in forming the presumptive, putative, presupposed, relation between science and technology. Our presuppositions regarding the relation between science and technology have their basis, rather, in that wider culture definitive of the historical epoch. The following pages provide repeated occasions to affirm that view.

The confusion of technology with science, which for a century past was deplored as a vulgar misconception by all who pretended to an informed view of science, is now an

accepted notion not only in all the disciplines that take science as their subject but even also among scientists. So we find in the 1 October 2004 issue of the prestigious *Science* an invited essay, 'What Kind of Science is Experimental Physics?' written by a historian of physics at the Max Planck Institute for the History of Science in Berlin. Otto Sibum's answer is that experimental physics is a 'technical science'. The phrase, as any reader of German literature on the science–technology relation knows, is an assertion that experimental physics is a constituent of, is subsumed within—is—technology. Sibum advances his claim for the 'artificial technological character of experimental physics' from behind a stalking horse: Felix Auerbach, an early 20th century theoretical physicist, professor at the University of Jena, who was closely associated with the Zeiss optical firm. Writing in the early 1920s, i.e. in the context of the resurgence in Germany following the First World War of romantic antagonism both toward physical science and toward technology, Auerbach, as was typical in that context, even for a physicist with close connections to technology, half conceded the often-heard denigration.[24] Allowing that a phenomenon, such as X-rays, produced by physicists in their laboratories with the aid of instruments is 'not a "natural phenomenon"', Auerbach, as quoted by Sibum, further conceded that:

> Strictly speaking, physics with regard to its method is not a natural science like astronomy, geology, biology, etc.; it does not deal with natural phenomena but artificial phenomena produced by intentional acts of the researcher; in this sense we can speak of physics as a technical science.[25]

Sibum's translation of Auerbach is accurate, but only of the words he includes. Sibum omitted a qualification that Auerbach had inserted after the phrase 'physics with regard to its method'. What Auerbach had written is: 'physics with regard to its method (not its goal) is not a natural science'. Where Sibum ended his quotation, Auerbach continued to elaborate that parenthetical but crucial qualification:

> … in this sense we can speak of physics as a technical science. Understand me correctly: with regard to its method; for with regard to its goal physics is and remains a pure natural science, insofar as it does not aim (or at least not primarily) at technical applications, but serves pure cognition.[26]

Clearly, for Auerbach the distinction between physics and technics remained of high importance, and that precisely because what distinguished physics *qua* science—'pure cognition'—placed it so much higher than technics on the scale of cultural values of that time and that place. The grossness of Sibum's misrepresentation of Auerbach's view should thus be understood as a measure of the difference between the cultural values of Auerbach's still modern time and of our postmodern time.[27]

Sibum's is by no means the most forward and categoric among the recent assertions of the primacy of technology over science. Those have come from philosophers. To Heidegger, who 'apparently accords technology a dignity without precedent in the history of thought',[28] they commonly credit their 'sense that philosophy of technology is and ought to be broader than philosophy of science', that, Ihde writes, 'science, rather than being the origin of technology or technology as the application of science, becomes the *tool* of technology'.[29] William Lovitt, one of the first to be able to grasp the

importance Heidegger attributed to technology, has stated forcefully its elevation relative to science:

> Seen as Heidegger would have us see it, the technology so familiar to us emerges as the supremely determinative phenomenon of our day.
>
> This according of decisive supremacy to technology must surprise us. What a sweeping derogation it is of the other phenomena of our age, most importantly of modern science! After all, is not technology a latecomer on the stage of history? Is it not in fact a kind of derivative of science? And, far from its being supreme, does not the more fundamental, more far-reaching sphere of scientific activity and scientific knowledge actually hold priority over it, offering us a viable and proven standing ground from which to launch our technological enterprises, to assess and to control them? To this, Heidegger would unhesitatingly reply that it is in fact precisely in technology's relation to modern science that the priority belonging to modern technology is to be found most strikingly in play and that, accordingly, we cannot legitimately find in science any superior vantage point from which to survey and cope with technology.[30]

If one bears in mind that Heidegger has been in recent decades the most influential philosopher, world wide, hands down, then it seems probable that most of the versions of Sibum's thesis which have appeared in the past thirty years base themselves upon Heidegger, albeit more often indirectly than directly.[31]

Yet having denied a dispositive role to the factual relations between technology and science in forming our current cultural valuations of technology and of science, would we be warranted in granting such a role to a philosopher's ideas? The career of Heidegger's influence in just this regard—the importance attached to technology—provides a striking example of the impotence of even the most emphatically expressed ideas of the most highly regarded philosopher in the absence of a culturally created and propagated purpose that those ideas are fit to serve.

'In all of Heidegger's work subsequent to 1930 technology is a primary issue', Borgman observed in 1987. Yet, in surveying the scholarly literature on Heidegger, Borgman found that this fact had been largely ignored through the 1960s and 1970s, and was coming to be recognized in Heidegger scholarship only in the 1980s.[32] No question, by the late 1930s Heidegger had come to regard 'modern technology' as *the* great danger for humanity and *the* great problem for philosophy. Salvation would come only through recognizing what he had recognized—'the essence of technology' and the essentiality of that essence to philosophy. Notwithstanding that Heidegger said this loud and clear for 40 years, from that time forward to the end of his life, his epigone to the West were simply unable to recognize the primacy that Heidegger attributed to technology. They became able to do so about 1980, not because of anything that Heidegger, then dead, said, but because the presuppositions and predilections of their Francophone and Anglophone cultures had so altered as to potentiate a recognition of that aspect of Heidegger's thought.

As one among the many instances of this pre-postmodern incapability of 'hearing' what Heidegger was saying about the relation between science and technology, consider the acute and learned Grene writing, in the mid 1960s, the article on Heidegger for the eight-volume Macmillan *Encyclopedia of Philosophy*. Grene, who in 1951 had herself

published a translation of Heidegger's 1938 essay, 'Die Zeit des Weltbildes', wrote in that encyclopedia article that the 'later' Heidegger—i.e. the post-*Sein-und-Zeit* (1927) Heidegger—'draws no line between pure and applied science'. In so writing she had forgotten—had presumably never gotten—what Heidegger warned against at the very outset of that 1938 essay: the construal of technology as applied science. There Heidegger had said, in her own translation, that:

> Among the essential phenomena of modern times we must count science. A phenomenon with the same degree of importance is mechanical technique. This should not be misinterpreted, however, as a mere application of modern mathematical natural science to practice. …
>
> Mechanical technique remains the most visible product to date of the essence of modern technology, which is identical with the essence of modern metaphysics.[33]

To write that Heidegger 'draws no line between pure and applied science' Grene had not only to overlook Heidegger's warning against conceiving technology as applied science, she had also to overlook the radicality of Heidegger's conception of technology, of technology's primacy as evoker of science into its own service, and shaper of every other aspect of the culture of the age. In the mid 1960s modern preconceptions were evidently still too strong for Grene to be able to assimilate sufficiently to restate what she herself had written in translating Heidegger.[34]

Heidegger himself was so disturbed by this almost universal neglect of his main message that in the spring of 1976, just weeks before his death, as perhaps his last public act, he sent a brief greeting and admonition to the participants in the tenth annual North American Heidegger Conference in Chicago:

> Is modern natural science the foundation of modern technology—as is supposed—or is it, for its part, already the basic form of technological thinking, the determining foreconception and incessant incursion of technological representation into the realized and organized machinations of modern technology?[35]

Having posed this rhetorical question, Heidegger urged that 'each participant, in his own way, devote his attention to this question and accept it as stimulus for his own field'.[36]

The remarkable inability prior to the late 1970s to grasp what it was that Heidegger was shouting out and about is no less true—and thus all the more remarkably true—of his acolytes in France. They, after all, were so much in advance of the Americans in receiving Heidegger, and so much more completely under his spell. 'For me Heidegger has always been the essential philosopher', said Foucault at the end of his life. 'My entire philosophical development was determined by my reading of Heidegger'. The same could be said for Derrida and for Lyotard, and for almost any other French philosopher who came of age after 1940.[37] Yet, notwithstanding that Heidegger's influence in France has been enormous from the early 1950s onward, not until the last quarter of the 20th century was his view of technology, and more especially his view of the relation between science and technology, assimilable there. A different worldview—the postmodern world view—was indispensable to 'getting' what Heidegger was saying.

If we postmoderns are now able to think with Heidegger of technology as prior to science, possessing primacy over science, that is, in part because as postmoderns we are

not bearing that load of animus which Heidegger's words express. Here too, the cultural determination of the freight and feel of Heidegger's 'influence' is striking: in giving primacy to technology, postmodernity has by-and-large reversed the minus sign that Heidegger placed before technology.[38] We postmoderns have rejected that rejection of modern technology that for two centuries had been characteristic of the broad and strong, chiefly German, romantic tradition in which Heidegger stood.[39] In that tradition, among the several grounds for the negative valuation of natural science, especially physical and mathematical science, was a view of such science as being nothing more, or other, or better than technology.[40] Of these motives and passions our postmodern society and culture—albeit in *other* important respects romantic—know nothing, care nothing. On the contrary, it is in part because technology is so positively valued in postmodernity that it has now superseded science in cultural primacy.[41]

While this paper is not, *per se*, concerned with the actual, factual relation between science and technology, it is difficult to doubt that if those who speak for science share these postmodern cultural values, then consequences will likely follow for the character, even the existence, of science. Technology—which is not, primarily, an '-ology', but simply the collective noun for all the many ways things are in fact done and made—technology is what it is independently, largely, of our conceptions of it. The opposite is the case with science, which is, largely, only what we think it is. That is, the boundary between science and non-science, as well as the bulk of the activities that are science, is not primarily a matter of fact but of a cultural consensus. Consequently, science will tend to conform to its supposed relation to technology as that supposed relation informs the expectations of actors, individual and corporate. The tendency today to emphasize the technological aspects of science so greatly as to obliterate the distinction between science and technology is not limited to philosophers, sociologists and historians of science, but has become common among spokespersons for science too. Those most influential in science not only emphasize the importance of new technology for scientific advance—and minimize the importance of new concepts—but also deny the existence of any clear boundary or distinction between science and technology. Increasingly, these influentials are discarding science's claims to distinctiveness and defining science in technological terms and through technological goals.[42]

A striking example of the disparagement of scientific goals relative to technologic goals is the conclusion of de Gennes's 1994 Dirac Memorial Lecture—a lectureship created by Cambridge University to honor one of its three greatest, and most definitely its purest, theoretical physicist. Observing that,

> These days a lot of time is spent on *ironing*: in a country the size of England, something like ten million people ironing for one hour a week. If, by some intelligent reflection, we can gain, say, six minutes on this hour, a 10% effect, we are saving 10^5 man-hours per day—we are providing a non-negligible improvement for many individuals who come back exhausted from their work,

De Gennes, Nobel Laureate, professor at the College de France, avowed that 'Personally I would feel more proud to achieve this than to solve an elegant formal problem in statistical physics'.[43]

Similarly disparaging of concepts relative to techniques was Thomas Cech, president of the Howard Hughes Medical Institute, in 2004 describing the emphasis that his new, large basic research campus, Janelia Farm, will place on the development of new technologies. 'We' scientists, Cech observed, 'like to think that our smart ideas are what are driving science forward, but we keep realizing that … the limiting factor really is … the technology'.[44] In so saying Cech, conceiving himself still as scientist, cleaves to a conception of science as something distinct from the technologies it employs, however much his program for 'basic' research tends to defeat that distinction. That defeat is the more certain as nowhere today are arguments advanced in favor of a distinction in principle between science and technology. Rather, what one hears are declarations such as that of the CEO of the American Association for the Advancement of Science:

> I'm a simple man. I've never been able to understand the delineation between science and technology and engineering and innovation and all that stuff. To me it is all one thing.[45]

The modesty of the self-presentation is winning, even as it is abdicating of the founding purposes of that mid-19th century institution. Then Joseph Henry could say to its applause that 'We leave to others with lower aims and different objects to apply our discoveries to what are called useful purposes'.[46] Today, in postmodernity, Henry's cynosure of for-its-own-sake science is without cultural understanding or support. Consequently, those who identify themselves as scientists have, overwhelmingly, no other ambition than to place themselves in the service of 'useful purposes'. In the experimental sciences, biology especially, this process has been far advanced for almost two decades now.[47] To be sure, cosmic-discovery science and history-of-life-on-earth science continue, but less as exceptions than as 'useful' to an increasingly credulous, 'spirituality'-oriented, romantic-illusionary, postmodern culture.[48]

It should be clear, then, that the last place to look for transformative effects of this epochal shift in cultural values is in the most characteristically modernist of our scientific enterprises, the high-energy, particle-physics accelerator laboratories—'big science' institutions specifically constituted for the production of end-in-itself knowledge.[49] Not that such effects can long be absent from those institutions too.[50] Yet by their very dedication to non-utilitarian, disciplinarily defined, means-justified ends, these institutions have little leeway to compromise with the demands of this our postmodern era. Their very existence depends upon limiting the impact of this revolution in cultural values. The high-energy physicists *must* continue talking publicly in foundational terms, even while privately acknowledging that they have given up thinking in such terms.[51] If we want to see postmodernization working radically and admittedly upon the scientific role and knowledge production, we should look, rather, into the 'little science' academic laboratories that in high modernity were lauded as the sites of true science. With an irony that none could have anticipated 40 years ago, it is today just those 'little science' laboratories that have reoriented themselves most completely toward technologically defined ends, while it remains to the earlier much-maligned 'big science' laboratories to shoulder the burden of sustaining 'for its own sake', 'fundamental', 'curiosity driven', or 'pure' science—all equally depreciated epithets.

One striking manifestation of this reorientation of little science toward technology is the burgeoning of nanotechnology. The nearly exponential rise of publication under the rubric of 'nano' in the period 1986–2002, to the level of several thousand per year,[52] reflects a chain-reaction of 'free' choices by thousands of individual 'little' scientists in a wide range of disciplines, with each successive convert finding it correspondingly easier to turn their back upon their prior, disciplinarily defined research enterprise. Meanwhile, in this same period, role and rank have reversed so completely between physics and biology that where previously 'physics envy' was commonly invoked to explain biologists' behaviors, today the rush into nanotechnology is commonly attributed to 'molecular biology envy'. Nor is it accidental that the concocted research program 'nanotechnology' is so thoroughly analogous to molecular biology in its tinker-toy imagery and in its salvific rhetoric as well as its strictly technologic goals.[53]

Physicists, aware that postmodern conditions are disintegrating their discipline, have turned to 'physics history' as a sort of cement. The effectiveness of that cement will depend, *inter alia*, on just what counts as 'physics history'. What does count? In February 2004 the 'This Month in Physics History' column in the American Physical Society's monthly newspaper commemorated 'February 9, 1990: Death of George de Mestral'.[54] Who was de Mestral and of what discipline-defining importance was his work, that the fourteenth anniversary of his death was so celebrated? The editors knew that among physicists de Mestral's was not yet a household name and so they explained: the honoree was a Swiss mechanical engineer, fond of pheasant hunting. Inspired by the local cocklebur adhering tenaciously to his hunting clothes, de Mestral conceived, and persevered to perfect and market, Velcro®. Thirty years ago this column could have intended only hilarity and could have been read only as a lampoon. In the fourth year of the 21st century it was meant wholly seriously and as exemplary for the members of this, the principal disciplinary association of physicists in the USA—indeed, in the world.

Certainly de Mestral manifested significant virtues, just as certainly as 30 years ago, physicists, then still being permitted their modernist conceits, would have been inclined to consider those virtues relatively insignificant. Yet in holding de Mestral up to the members of the American Physical Society as a model to emulate, the society's editors were implicitly disparaging those more esoteric virtues around which physics had solidified itself as scientific discipline—pre-eminently, the virtues manifested by the conceptually creative abstract theorist disengaged from practical interests.[55] In lieu of those difficult and distinctive virtues, physicists were being urged to take as avatar the technologist—indeed the least abstract and the most entrepreneurial sort of technologist. If such ideals come into effect—and there is much evidence that they have[56]—then that tautologous evasion, 'physics is what physicists do', which three decades ago seemed to be a witty and effective put-down of those critical of what physicists were then up to, will fail for want of any distinctive group identifying themselves as physicists.

Ironically, the unavoidability in postmodernity of this primacy of technology is evident even in the argumentation of those conceiving themselves as most opposed to postmodern positions—those polemically striving to sustain a conception of science as

attained truth, truth independent of every personal and institutional interest, hence unique and transcendent. The 'perspectivist' challenges to the unicity and transcendence of certified scientific knowledge that Gross and Levitt ridiculed in *Higher Superstition* (1994) were for the most part poorly founded in evidence and logic, because, like the newly asserted primacy of technology, so well grounded in postmodern prejudice. Yet Gross and Levitt, sophisticated critics that they were, knew that every affirmative defense of foundationalism is readily shown to involve itself in dubitable, question-begging postulations. Leaving it therefore to the 'serious philosophers of science' to 'refute, in abstracto, the constructivist view', Gross and Levitt themselves offered no other justification for science's truth claims than that, 'To put the matter brutally, science *works*'.[57] This line of argument, in its evasion as in its rationale, is not modern but postmodern. However, the tough-minded stance that Gross and Levitt assume is not the conclusion that postmoderns of any stripe need draw, nor should draw. Whether one says with Heidegger that Gross and Levitt merely acknowledge that the essence of science is technology, or one remains at home with our leading American postmodern pragmatist, Richard Rorty, and says that physics is the construct of a community with a shared interest in pushing objects around,[58] 'science *works*' may contribute to the maintenance of research budgets, but it undermines the conception of science that Gross and Levitt wish to defend.

Wherever we look in contemporary culture, we find this astonishing elevation of technology in rank and role relative to science. In every field of science, as in every field of science studies, technology forms both focus and epistemic fundament for our endeavors.[59] How to account for this? We will not comprehend so broad a cultural phenomenon unless we connect it with still broader cultural phenomena, namely, that constellation of values and presuppositions contrasting markedly with those obtaining for some centuries prior to *ca* 1980—contrasting so markedly that the conversion to them constitutes a new historical epoch, postmodernity. Although, as Mirowski has emphasized, 'it has now become commonplace in the science policy and science studies literatures to concede that something rather dramatic has happened to the organization of science and the university in the quarter-century or so since 1980', it has remained far from commonplace to regard that 'something' as something farther reaching even than the ecology of knowledge.[60] Broadly considered, a reversal of the presumed relation between technology and science involves nothing less than an inversion of our mode of being, and is therefore effectively a demarcator, both of state of mind and of point in time, between modernity and postmodernity.

II. The Primacy of Science in Modernity

> *Technics is a translation into appropriate,*
> *practical forms of the theoretic truths … of science.*[61]

The fact of the primacy of science in modernity—science *qua scientia*, science as disinterested *theoria*—is almost as hard for us postmoderns to remember as it was for the

moderns to doubt. Hence this paper devoted largely to pointing out how unanimously the moderns elevated science over technology and how preposterously they overrated the role of science in the advance of technology. The magnitude of the cultural phenomenon here in question can best be suggested by a concrete expression of those presuppositions. Consider the following affirmations by Gano Dunn in 1930—the highly successful head of a large engineering firm was addressing the alumni of his alma mater, Columbia University:

> They are no idle boasts, those legends written under the dome of the beautiful temple of science in Washington, 'Pilot of Industry', 'Conqueror of Disease', 'Multiplier of the Harvest', 'Explorer of the Universe', 'Revea1er of Nature's Laws', 'Eternal Guide Truth'.
>
> The priests who sacrifice in her temple know the joys of the freedom of the human intellect, … the scientist's ecstasy of thought.
>
> But beside her priests science has her worshipers who go out into the world and are a part of it. They are the engineers.[62]

However preposterous Dunn's imagery now appears to us, unquestionably it was meant entirely seriously, and, more to the point, was supposed by Dunn to be entirely acceptable to his auditors.

Dunn's affirmations bespeak two distinguishable respects or regards in which science was ascribed primacy to technology in modernity. In two words, those two regards are 'rank' and 'role'. Rank is position in a hierarchy of ascribed value, reflecting degree of manifestation or possession (or, conversely, privation) of leading values of the culture. This, I take it, is what Otto Mayr had in mind when, 30 years ago, he observed that the aspect of the relationship between science and technology to which historians ought to be attending is how science and technology rank, respectively, 'on our scale of values'.[63]

Role, on the other hand, is a matter to be ascertained not by referring science and technology each separately and directly to 'our scale of values', but rather by considering science and technology in relation to each other. Again, at issue here is not how technology and science 'really' relate to each other, but how these two entities are generally presumed and commonly alleged to relate, i.e. at issue is the *putative* relation between technology and science. For the cultural and intellectual historian the putative relation is not less important than the actual, factual relation between technology and science, and gains in historical importance precisely to the degree to which it differs from the actual, factual relation in the places and periods in question.

Returning to Dunn's affirmations, what we find there expressed in such exaggerated terms is an ascription of far higher social–cultural rank to scientists than to engineers. The scientist exists on a plane that transcends the plane of practical life to which engineers are confined. On that exalted plane the scientist enjoys the most highly rated attributes of thought. Second, as to role, the scientist, disconnected though he is from practical life, is nonetheless the source not only of technology's material benefits, but also of that far-seeing leadership by which they are realized. With all insight and innovation credited to scientists, the engineer's activity is necessarily subservient, dependent and derivative. The one element of originality that Dunn attributes to the engineer

is of a lower, constrained order: 'Engineering is the art of the economic application of science to the purposes of man'.[64]

Dunn's conception of science as playing the leading, guiding, originative role in relation to technological innovation was very nearly universally shared in the first half of the 20th century—and both halves of the 19th—not only in America, but across Europe, and wherever in the world beyond the hope of catching up with the West was cherished. While most of the strongest affirmations of this primacy of science came from scientists, rather than technologists, often the circumstances of even those articulations bespeak the scientist's confident expectation that such claims for science would find ready acceptance in technical and industrial circles. So, for example, when, in January 1921, physicist Wilhelm Wien appealed for adherents to the association for the support of 'physical–technical research' in German universities and institutes of technology, an association that he was then organizing together with several leading figures in Germany's iron, steel, electrical and chemical industries, he had no qualms about classifying 'metallurgical, mechanical, and electrical engineering' as 'applied physics'. 'If research in physics atrophies, then technology loses the scientific leadership, through which alone [!] it has worked itself up to its towering height.'[65]

Of course throughout modernity there were national differences in the degree of unanimity regarding the primacy of science. The USA, perhaps surprisingly in view of its populist traditions and its high valuation of technology, occupied a middle position on this spectrum. Germany, albeit the country with the strongest tradition of pure research, had exceptionally outspoken engineers who took vehement exception to affirmations such as Wien's. On the whole their protests were ignored because counter to the presuppositions of their culture too.[66] In France, as Pestre has stressed, the concept of 'scientific leadership' had both stronger underpinnings and wider application, for there it was seen as extending through all the institutions of French society, not stopping short of the political institutions, as it generally did in Germany and the USA.[67] Yet these national differences are 'second order' differences. Far more prominent and significant is the high degree of transnational unanimity regarding the primacy of science.

The main concern of this paper is, then, to put into evidence the fact that a definite functional relationship between science and technology was almost universally taken for granted in modernity—namely, ascription of the value-ladened relational role of leadership to science, and of dependence to technology. Obviously, role, specified though it be through functional terms rather than posited cultural values, becomes value-ladened through the participation of those relational concepts in the hierarchies of ascribed value that gave science and technology each separately their cultural rank. That there is thus, at bottom, considerable redundance between 'role' and 'rank' does not diminish the analytic utility of distinguishing between these two dimensions of primacy. This appears quite clearly when the several cultural values here involved transform differently in the transition from modernity to postmodernity. Such is the case with the concept of 'leadership', especially leadership in innovation, which has retained its high cultural value, even while theory and abstraction have fallen drastically in cultural value.[68] It is this relative constancy of the one hierarchy of cultural value

together with a drastic change in the other that has produced so strong an effect upon the science–technology relation.

Now, if the proposition to be substantiated is true, if in modernity nearly all who gave any thought at all to the relation between technology and science gave primacy to science, then the evidences of this are obviously boundless. Hence limitations to this exposition are necessary. From here forward, I leave entirely aside all testimony anterior to the 19th century, as for the purposes of this argument modernity is most appropriately conceived as the era emerging from the Enlightenment, the industrial revolution, and the formation of nation states, especially those with democratic institutions. Further, though at the cost of ignoring some of the most ardent affirmations of the primacy of science to and for technology, I leave entirely aside articulations of that primacy by scientists, as being inevitably self-serving.[69] I put into evidence, rather, the views of just those who, given their intellectual postures and their social positions, ought to have been inclined to privilege technology. Here included are, first, philosophers upholding the primacy of practice; second, engineers and industrialists; third, modern man, the holder of public opinion, who experiences technology immediately in his daily life, but science only through hearsay; and last, scholarly anticipators of the role of science in late 20th century technology and society.

Proponents of the Primacy of Practice

Although there would be good grounds for going right back in the German tradition of 'practice' (and 'life') philosophy to the beginning of the 19th century—and it will become necessary to refer so far back when taking cognizance of the presence and power of romantic ideologies—it is with Marx that our canonical historiographic tradition originates the primacy of the material over the intellectual, of the economic and the technical over the scientific. On this point, however, Marx was no Marxian—nor, by and large, were his Marxist followers. For all of Marx's acuity in seeing through ideologies to the actualities they dissimulate, and for all that Marx gave, in principle, a determinative role in the shaping of culture to the mode of production, when Marx turned to the relation between science and technology it simply did not occur to him to suspect the preconceptions of his predecessors and contemporaries. While rejecting categorically the social–political views of the advocates of the beneficent hand, visible and invisible, in *this* connection Marx himself had just as little doubt as did they that the arrow of causation pointed from thought to practice, not from practice to thought.

Along with all his educated contemporaries, Marx simply knew it to be true that industrial processes had their actual and their necessary basis in, and were evolved out of, the natural sciences: 'natural science has intervened in human life and transformed it by means of industry'.[70] Consequently, Marx considered that historical materialism was the way of the world only prior to the full development of capitalism, at which point 'general social knowledge has become an immediate force of production, and therefore the conditions of the social life process itself have come under the control of the general intellect'.[71] The quotation is from the *Grundrisse*

(1857–8). In that impetuous draft of *Das Kapital* Marx even went so far as to equate industrial capital itself with 'technological application of the natural sciences':

> The transformation of the production process from the simple labor process into a scientific process which subjugates the forces of nature and compels them to work in the service of human needs, appears in the form of fixed capital in opposition to living labor.[72]

And this identification of capital itself with applied science was repeatedly affirmed in the Marxian tradition right into the 1980s.[73]

Marx's unwavering presupposition of the primacy of science is reflected already in his reading program in Poppe's treatises on the history of science and technology—this late 18th century encyclopedic writer in the cameralist tradition being Marx's principal source on these subjects as he prepared himself for his great work. Just as Poppe, Marx took it as beyond question that theoretical mechanics, physics and mathematics were foundational for technology. Consequently, Marx read Poppe's treatises on the history of natural science and mathematics before proceeding to Poppe's history of technology. True, Marx's notes show his attention flagging badly as he read dutifully through these treatises on the history of science, in which he could not, of course, find much to his purpose. In the end, only the notes that he took on Poppe's history of technology came to be worked into *Das Kapital*.[74] Nonetheless, Marx's reading program, and his dutiful adherence to it, themselves bespeak his unquestioned presupposition of the primacy of science.[75]

In *Das Kapital* this thoroughly 'unMarxian' presupposition of science's primacy to and for technology and modern industry generally, is evident wherever Marx referred to the relation between technology and science. Thus in the first chapter Marx observed that:

> The productive power of labor is determined by multiple circumstances, among others the average degree of skill of the worker, the level of development of science and of its technological applicability, the social organization of the production process, the extent and effectiveness of the production process, and by the obtaining natural conditions.[76]

Here 'the level of development of science and of its technological applicability' enters the list of determinative factors just where one would expect to see reference to the level of technology itself. Manifestly, it is in lieu of that reference: in Marx's mind the technological level was determined by the level attained by science. Again, in the thirteenth chapter, 'Machinery and large-scale industry', Marx took it as a matter of course that natural science is the basis of machine production and the source of new types of machinery:

> The principle of machine production, namely analyzing the production process into its constitutive phases and solving the problems thus posed through application of mechanics, chemistry, etc., in short, the natural sciences, becomes everywhere determinative.[77]

And again:

> In the sphere of agriculture, large-scale industry has its most revolutionary effect inasmuch as there it demolishes the bulwark of the old society. … In place of the laziest habits and most irrational organization of production there steps conscious technological application of science.[78]

Clearly, then, the primacy of science's role in relation to technology, especially since the 18th century, was for Marx a fixed and firm matter of fact. If a thinker of such great critical acumen, a thinker who was pre-eminently a systematist—and his system demanding the opposite causation—could not unthink the presumed primacy of science, not even when his historical investigations produced little evidence of it, then that presumption must have rested on an exceptionally broad and deep cultural foundation.[79]

How then does it stand with Thorstein Veblen, America's most original social theorist at the turn of the 20th century?[80] Running through Veblen's writings from the last years of the 19th century to the end of the First World War are various articulations of the primacy of practice, and, more pertinently still, articulations of the primacy of technology as a formative factor in all of modern civilization's intellectualizations: 'The scheme of thought or knowledge is in good part a reverberation of the scheme of life' (1899), and more specifically, 'men have learned to think in the terms in which the technological processes act' (1906), that is, 'the habits of thought enforced by the current technological scheme' 'impose themselves as ruling principles that govern the quest for knowledge' (1918).[81] Though apothegmatic statements of the primacy of practice in respect of the relations between science and technology are plentiful in Veblen's writings, elaborations of them are to be found only in a pair of essays published in 1906–8,[82] and in a couple of chapters of *The Instinct of Workmanship and the State of the Industrial Arts* (1914).[83] In the earlier essays Veblen proceeds from the observation that 'modern civilization is peculiarly matter-of-fact. … The modern civilized peoples are in a peculiar degree capable of an impersonal, dispassionate insight into the material facts with which mankind has to deal.'[84] This circumstance has a material cause: it is 'due chiefly to the ubiquitous presence of the machine technology and its creations in the life of modern communities'. Moreover, 'this characteristic of western civilization comes to a head in modern science', with its 'quest of matter-of-fact knowledge'. For it is, namely, 'those changes that took place in the field of industry' 'which seem to have had the most serious consequences for the methods and animus of scientific inquiry'.[85]

So, through this series of (causative) connections, Veblen arrives at 'the reason why scientific theories can be turned to … technological purpose'. The explanation—and it is much like that which Heidegger would later offer for this circumstance, only that it is nominally naturalistic rather than explicitly idealistic:

> is not that he [the scientist] aims, or can aim, at technological improvements. His inquiry is as 'idle' as that of the Pueblo myth-maker. But the canons of validity under whose guidance he works are those imposed by the modern technology, through habituation to its requirements; and therefore his results are available for the technological purpose.[86]

While Heidegger, as idealist, could regard the finally achieved relation between technology and science as sufficient cause of its becoming, and so brush aside temporal order as a triviality,[87] Veblen, as scientist constrained to efficient causes, could not so easily accommodate the historical fact that the world-view of modern science arose and

established itself long before machine technology began to dominate the 'scheme of life'. The inconsistencies in Veblen's exposition engendered by this awkward fact are more pervasive in *The Instinct of Workmanship* (1914), where he vacillated between having the mechanistic approach of modern science reflect the technology of the Industrial Revolution, reflect that of the early modern period, or reflect that of the late medieval world, and, still more fundamentally, vacillated between attributing primacy to technology or to economics. For if economics, not technics, is taken as fundamental, thus making every feature of the modern world a consequence of 'the price system', then the mind-set of modern science is the product not of technological practice, but of that which *also* determines technological practice: 'modern science may, indeed, be taken as the freest manifestation of that habit of mind that comes to its more concrete expression in the technology of the time'.[88]

Veblen returned to this question of the relation between science and technology in *Absentee Ownership and Business Enterprise in Recent Times* (1923). In failing health, Veblen was no longer able to withstand the truisms of his time. In place of the primacy of technology to and for science, Veblen's theme there is 'The Technology of Physics and Chemistry'. Where earlier he had often alleged that 'men have learned to think in the terms in which the technological processes act', now it is the other way around. Now the technologists are applied scientists who have learned to think in the terms which science provides them: 'the state of the industrial arts, as it runs on the lines of the mechanical industry, is a technology of physics and chemistry'. 'The twentieth-century engineer … feeds' upon 'those material sciences of physics and chemistry'.[89]

> As an after-effect of Faraday's experiments and speculations … the dynamo and the electrical industry came into the technologist's world in the course of the latter half of the nineteenth century.[90]

Similarly 'exact and calculable processes worked out by detailed experiment and computation in the chemical laboratories' transformed metallurgy, dyeing, tanning, baking, and brewing into 'recondite matters of applied science'. 'Industry has progressively shifted to a footing of applied science' and its day-to-day operations are now in the hands of 'men grounded in the material sciences'—Veblen's term for physics and chemistry collectively—'and instructed in the specialized application of them'.[91]

In order, then, to make Veblen out as attributing primacy to technology, one must, most obviously, overlook Veblen's late reversal of his position on that question. One must also choose to ignore Veblen's persistent inconsistency between attributing primacy to technology and attributing primacy to economics. Even ignoring Veblen's inconsistencies, one would not have reached more than half way to the primacy of technology: the primacy thus dubiously established would be only 'primacy for'; 'primacy to', cultural rank, was for Veblen a different matter. On this issue of the valuation of the intellectual, Veblen was of the conventional view from the 'get–go'. For this reason, if for no other, his assertions of the primacy of technology were for him intellectual exercises rather than statements of a deep conviction. Wavering and inconsistent in his affirmation of the primacy of technology, Veblen, like Marx, quite consistently placed pure, for-its-own-sake science far above technology in cultural rank and value. Of this

we have had a hint already in Veblen's vaunting the impossibility of the scientist, *qua* scientist, having a technological aim—we will encounter this again with Dewey—and vaunting the habit of mind of the scientist as being the freest of all, while that of the technologist is encumbered by the concrete in which he must mix. Veblen laid this out categorically and dramatically right at the outset of 'The Place of Science in Modern Civilization' (1906):

> The making of states and dynasties, the founding of families, the prosecution of feuds, the propagation of creeds and the creation of sects, the accumulation of fortunes, the consumption of superfluities—these have all in their time been felt to justify themselves as an end of endeavor; but in the eyes of modern civilized men all these things seem futile in comparison with the achievements of science. They dwindle in men's esteem as time passes, while the achievements of science are held higher as time passes. This is the one secure holding-ground of latterday conviction, that 'the increase and diffusion of knowledge among men' is indefeasibly right and good. … and no other cultural ideal holds a similar unquestioned place in the convictions of civilized mankind.[92]

True, Veblen concluded this essay expressing doubts about whether 'the manner of man which this quest of knowledge produces or requires comes near to answering to the current ideal of manhood'—much the same doubts that William James had been expressing so much more prominently and emphatically for a decade and more as of that date.[93] Certainly Veblen, like James, was ambivalent about the impersonal, dispassionate, matter-of-fact mind and character with which the then prevalent conception of scientific method invested the scientist.[94] These, however, were only reservations—never even close to rejections.[95] Veblen's most fundamental commitment was to science as the highest cultural value and that necessarily meant placing a high value on the character requisite for its pursuit. This is evident from the un-ironically positive connotations of the terms 'science', 'scientist' and 'scientific', so frequently used by Veblen. To take but one example: writing at this same time (1906) of Karl Marx, Veblen insisted that 'As to the motives which drive him and the aspirations which guide him, … he is primarily a theoretician busied with … a consistent and faithful system of scientific knowledge'. As Veblen, like Marx, saw himself as a scientist, so, like Marx, Veblen regarded 'scientist' with its implication of disinterestedness as, on the whole, the most estimable of identities. Consequently, Veblen denied that Marx's 'propagandist aims have in any substantial way deflected his inquiry or his speculations from the faithful pursuit of scientific truth'.[96]

The work most revealing of Veblen's own values, the work in which he most completely put aside his posture of social–scientific skepticism and spoke *pro domo*, is *The Higher Learning in America* (1918). Not, of course, that Veblen avowedly abandons his historical materialist perspective in this polemic subtitled *A Memorandum On the Conduct of Universities By Business Men*. On the contrary, as business is the hegemonic institution in American life, its 'pecuniary values … will necessarily leave their mark on the ideals, aims, methods and standards of science and scholarship'.[97] So Veblen says—and then he proceeds to insist that it just ain't so. For although,

> There are many of these workday interests extraneous … to that disinterested pursuit of knowledge in which the characteristic intellectual bent of modern civilization culminates

> … there is no general or abiding consensus among the various classes of the community in favor of … including in the peculiar work of the university anything beyond the pursuit of knowledge for its own sake.[98]

Similarly, notwithstanding his general principle that modern man formulates such for-its-own-sake knowledge in mechanistic concepts, Veblen regarded as essential that 'the final test of this reality about which the inquiries of modern men so turn is not [*sic*!] the test of mechanical serviceability for human use'.[99] Rather, Veblen insisted, in 1918 as in 1906, that,

> … during the past few generations, this learning has so far become an avowed 'end in itself' that 'the increase and diffusion of knowledge among men' is now freely rated as the most humane and meritorious work to be taken care of by any enlightened community or any public-spirited friend of civilization. The expediency of such 'increase and diffusion' is no longer held in doubt, because it has ceased to be a question of expediency among the enlightened nations, being itself the consummation upon which, in the apprehension of civilized men, the advance of culture must converge. Such has come to be the long-term common sense judgment of enlightened public opinion. A settled presumption to some such effect has found lodgment as a commonplace conviction in the popular mind, in much the same measure and in much the same period of time as the current body of systematic knowledge has taken on the character of matter of fact. For good or ill, civilized men have come to hold that this matter-of-fact knowledge of things is the only end in life that indubitably justifies itself. So that nothing more irretrievably shameful could overtake modern civilization than the miscarriage of this modern learning, which is the most valued spiritual asset of civilized mankind.[100]

The great length of this quotation is justified by its great interest and pertinence: for all that Veblen as historical materialist theorizes technology's primacy to science, and offers *avant la titre* Heidegger's explanation for 'the easy copartnership' between modern science and modern technology, so that they 'play into one another's hands', Veblen as modern mandarin brushes the historical materialist aside and insists that end-in-itself scientific knowledge is the highest value of modern civilization. More, Veblen insists on this not merely as his own view, but as the universal view of his age.

As previously with Marx, we see here once again with Veblen the helpfulness of the distinction between rank and role—between the 'absolute' rankings of science and of technology on scales of cultural values and the 'relative' roles of science and technology in their relationship with one another. Such primacy as Veblen, in exploring the consequences of his historical materialist principles, inconstantly conceded to technology's role in the science–technology relationship was crushingly trumped by science's ranking on Veblen's scale of cultural values.

Veblen's German contemporary, Werner Sombart, economic historian and political economist, was less the historical materialist than Veblen, but even more the admirer and emulator of the author of *Das Kapital*.[101] Though Sombart's reputation, much higher than Veblen's in their own time, has deservedly not held up so well as Veblen's, he is cited by historians of technology as a weighty figure who conceded to science no primacy in rank or role in relation to technology.[102] Such citations point, regularly, to Sombart's 1911 essay, 'Technik und Kultur', and to a short section in the concluding

volumes of Sombart's *Der moderne Kapitalismus* (1927), which section opens with a declaration that 'modern technology … is a twin sister of modern science'.[103] Sombart continued this thought on the following page, insisting that it is 'a useless, indeed a false, question (one that I myself once posed) which of the two is genetically the earlier, which generated the other. They are simply one, and consequently their course of development is one and the same.'[104]

Nonetheless, it is with Sombart just as it was with Veblen: that parity accorded to technology in their programmatic statements evaporates in their subsequent exposition because at bottom both writers believe in the creed of the mandarins—believe that theoretical, abstract and, above all, interest-free, for-its-own-sake, knowledge stands higher than every and any practical activity. Thus Sombart in the immediately following lines of *Der moderne Kapitalismus* inferred from this unity and identity of science and technology that 'We can therefore determine the main features of the stages of modern technology from the stages of the development of natural-scientific knowledge'.[105] Without offering the slightest suggestion of a symmetrical determination of science by technology, Sombart proceeded then to list the major advances in rational mechanics associated with Galileo, Newton, Euler and Lagrange, and, likewise, the major advances in chemistry associated with Lavoisier, Priestley, Wöhler and Liebig, concluding:

> If natural science conceives the world as mechanical or chemical, then technology creates artificially a world that runs according to the formulas that natural science has set up for the world as a whole.[106]

So quickly does Sombart forget that parity of technology with science that he had declared two pages earlier, and reascribes to science an unqualified foundational primacy to and for technology. Being so much more affective a thinker than Marx or Veblen, Sombart was so much quicker and opener in confusing science's functional role in technology with science's superior rank on his own scale of cultural values.

For all that they admired Marx and, like Marx, gave primacy in principle to practice, neither Veblen nor Sombart was a Marxist. Surely a life-long Bolshevik like Nikolai Ivanovich Bukharin will give consistent primacy to technology? No: for Bukharin too, it was beyond question that science was both the originative source of technological innovation and the most needful aide and guide in the tasks of socialist construction generally.

Lenin apart, Bukharin was the one true scholar–intellectual in the leadership of the pre-Revolutionary Bolshevik movement and afterwards in that of the Soviet regime—until liquidated in the great purge of 1937/38. Admittedly, Bukharin was so much a historical-materialist theorist in his own right that Lenin had doubts about the orthodoxy of Bukharin's Marxism. Admittedly, Stalin, who had shared supreme power with Bukharin after Lenin's death in 1925, stigmatized Bukharin as a rightist deviationist when forcing Bukharin out of his most important leadership positions in 1929. Admittedly, among Bukharin's conflicts with Stalin leading to that ejection from power was his disagreement with Stalin's concept of the Five Year Plan and specifically

Stalin's principle of the priority of heavy industry. Bukharin's contrary view, as his biographer Stephen Cohen stressed, was that genuine industrialization would have to be based on a technological revolution, and that therefore '*the scientific-research network must grow faster than even the leading branches of socialist heavy industry*'.[107] Yet it is unlikely that Stalin, whose Five Year Plan aimed quite deliberately at a vast expansion of existing technology, could have imagined any objection to Bukharin's implicit *minor* premise: a technological revolution—were that the goal—would have to be based upon scientific research.[108]

In April 1931, Bukharin, then in his much reduced position of director of the Supreme Economic Council's Scientific–Technologic Department—the very name bespeaks shared assumptions of the Soviet leadership—gave the keynote address at an All-Union Conference on the Planning of Scientific-Research Work. The text of this address 'The Methodology and Planning of Science and Technology'—with which I am acquainted through an as yet unpublished English translation prepared by Professor Alan Mackay, Birkbeck College—shows quite clearly that the reason the harnessing of science to the tasks of socialism seemed to Bukharin so necessary and important was science's putative primacy to and for technology.[109]

Of course Bukharin begins his address with the hackneyed, but canonical, demonstration that 'Science grows out of practice, from the practice of the economy and from the practice of the class struggle'. Indeed, it is 'known to everyone that astronomy arose from the necessity of of orienting ourselves … . Chemistry arose on the basis of mining.'[110] More interesting as revealing the German idealism at the base of Bukharin's materialism is his proof of this origination of science out of technology through the fact that the inverse relation obtained in the Western world of his day. Thus after quoting a number of recent writers—one British, one German, one American, one French—all emphatic that contemporary technology is, and is increasingly, applied science, Bukharin, with much the same logic employed by Heidegger, insists that if we bear these statements in mind 'we are directly required to formulate the position that the practical roots of scientific disciplines are now uncovered'.[111]

Does then the fact that, on the one hand, science originates from technology, while, on the other hand, science originates technology, imply to Bukharin an identity between science and technology? No, a historical-materialist revision of the bourgeois–idealist conception of science is not at all Bukharin's aim. On the contrary, his thesis is that only the socialist state knows how to value science for its own sake; only under socialism does science become truly free:

> The practice of the bourgeoisie really disparages science, putting it at the service of the exploiters, making it an exploiting science. This leads science into a cul-de-sac. Proletarian practice liberates both the productive forces and science … .[112]

Bukharin does not condemn the bourgeois ideology of end-in-itself knowledge on the grounds that Marxism shows it to be an impossibility, but on the entirely practical grounds that the new state had urgent need of everything that science could provide: 'the slogan: all in the service of the great socialist construction and the defense of the proletarian country. This slogan is the central directive of the whole planned scientific

research work, the alpha and omega … .'[113] Idealizing and romanticizing science quite as highly as any bourgeois writer, Bukharin concludes his address with a paean to science—to science as something that, though not entirely self-sufficient, is yet in its essential principle quite distinct and distinguishable:

> The bigger the elements of planning, reasoning, weighing, in the social economy, in living, in all social life, so much greater must be the role of *science*, which fulfills the principle of *reason*.

Thus:

> Only … increasing the specific weight of science in the whole social life of the Union, only courageously uniting it with the industrial and agricultural economy above all, will we be able to endure the great, world-historic examination standing before us.[114]

It is no wonder, then, that 'pure science', generously supported by the socialist state (albeit not under just that rubric), flourished in Russia in the following four decades as never before.[115]

John Dewey

Turning back from Russia to the Western world, there is one further proponent of the primacy of practice whose views must be considered. John Dewey was not merely the most prolific philosopher writing in English in the period between the two world wars; he was also the most influential. His stance, after freeing himself in the last years of the 19th century from the thrall of Hegelian idealism,[116] was radically instrumentalist: Dewey was, during the entire first half of the 20th century, the most important and consistent exponent of an instrumental conception of knowledge—of every brand of knowledge, but first and foremost, of science. Ihde, paying Dewey the highest compliment that a postmodern philosopher of technology has at his disposal, credits Dewey with having 'simultaneously with Heidegger argued for a precedence of technology over science'.[117] Wrongly. As our review of the Marxian tradition of the primacy of practice has primed us to anticipate, it was simply beyond the capacity of a modernist intellectual, however radical and consistent an instrumentalist, to unthink the primacy of science to and for technology. Dewey, having exchanged the Hegelian world spirit for that of science, would have been among the last to do so.

To be sure, Dewey inscribed many a text that, taken out of context, implied some such precedence of technology over science. We can find in Dewey the same hackneyed attribution to technology of genetic precedence to science that we found in Bukharin:

> As the arts and crafts develop and become more elaborate, the body of positive and tested knowledge enlarges, the sequences observed become more complex and of greater scope. Technologies of this kind give that common-sense knowledge of nature out of which science takes its origin.[118]

Further, in various ways and various places Dewey insists that 'the gist of scientific knowledge is control of natural energies', that 'the scientific attitude … is … a practical attitude. … Its interest in change is in what it leads to, what can be done with it, to what

use it can be put.'[119] Moreover, the practice and progress of science is inseparable from technology:

> The history of actual scientific advance is marked by the adoption and invention of material devices and related techniques—of complex and refined forms of apparatus and definite related techniques of using apparatus.[120]

More than that, said Dewey in his Gifford Lectures (1929),

> The progress of inquiry is identical with advance in the invention and construction of physical instrumentalities for producing, registering and measuring changes.[121]

Not only does science have its origin in technology, and retain a technological character and orientation in its modern development, but science, Dewey at least once went so far as to say, 'is itself essentially a technology of apparatus, materials and numbers'.[122]

Yet we must not allow ourselves to be misled by these statements, no matter how factually correct they may be. However far Dewey went in articulating an instrumental view of science, and however often he repeated his indictment of the ancient despise of craftsmen as philosophy's original sin, setting it on the wrong path for millennia,[123] it is obvious that he himself assumed implicitly the superiority of *teoria* to *techne* and measured degree of good with a yardstick handed down from antiquity.[124] This is evident in Dewey's off-handed use of hierarchical metaphor when referring to those two activities: 'no matter how great his learning' the physician who merely subordinates the individual case to a general rule 'sinks to the level of the routine mechanic'; and again, 'scientific inquiry has raised activities, materials, tools of the type once regarded as practical (in a low utilitarian sense) into itself'.[125] Although Dewey assumed, he only rarely and only barely asserted, this social and moral superiority of *teoria* to *techne*.[126] Yet the quoted terms in which Dewey disparages practice—'in a low utilitarian sense'—alert us to a set of more conscious and explicit prejudices, namely, the moral as well as intellectual superiority of disinterested to interested knowledge.

Of disinterestedness as virtue in every human endeavor, and categorically in every social endeavor, Dewey was both certain and assertive. That inferiority of the interested to the disinterested, which Dewey as pre-postmodern took to be beyond question, alone and of itself condemned technology to a lower rank than science in civilization. Thus although the above quoted passage from Dewey's Gifford Lectures very nearly equating science with 'invention and construction' continues with Dewey insisting that, 'Moreover, there is no difference in logical principle between the method of science and the method pursued in technologies',[127] the 'in … principle' conflation of science and technology ends right there. In the next sentence enters a 'practical' differentiation between science and technology, based upon a differential moral ranking of those two practices.

> The difference is practical; in the scale of operations conducted; in the lesser degree of control through isolation of conditions operative, and especially in the purpose for the sake of which regulated control of modifications of natural existences and energies is undertaken; especially, since the dominant motive of large-scale regulation of the course of change is material comfort or pecuniary gain.[128]

To our postmodern sensibility this reference to 'material comfort or pecuniary gain' suggests no disparagement of technology. Indeed, quite the contrary! However, we must bear in mind that Dewey—like Veblen, whom he greatly admired, and to whom he stood politically close—was of a very different mind. These two proponents of practice shared a strong distaste for 'the pecuniary institution'—as Veblen termed it—both as entrepreneurs in an antithetic economy of ideas, and again as political and economic socialists.[129] Expressions of antipathy toward pecuniary aims and motives, and equations of disinterestedness to virtue, are found everywhere in Dewey.[130] To take just one especially passionate expression delivered at a depression-era conclave:

> Our discoveries and our inventions in the field of electricity and radio—did the business men who operate our economic system produce these things? No, I say again. They were produced and invented by the scientists who were working disinterestedly and honestly and who then placed the fruits of their labor at the disposal of others.[131]

Consistent with his insistence that nothing truly good came from the efforts of the interested, nothing enraged Dewey more than the construal of his instrumentalism as an individualistically interested utilitarianism:

> when truth is defined as utility, it is often thought to mean utility for some purely personal end, some profit upon which a particular individual has set his heart. So repulsive is a conception of truth which makes it a mere tool of private ambition and aggrandizement, that the wonder is that critics have attributed such a notion to sane men.[132]

An antipathy to the interested, especially the pecuniarily interested, has, since antiquity, been integral with a conception of the intrinsic superiority of science to technology.[133] This ancient prejudice Dewey, like other moderns, shared. He took it as a matter of course that science—and the scientist, too, *qua* scientist—being as such necessarily disinterested, are inherently morally good; not neutral, but good. Technology, however, is only potentially good, only contingently good, and more often than not, is not:[134]

> The technologies of industry have flowed from the intrinsic nature of science. For that is itself essentially a technology of apparatus, materials and numbers. But the pecuniary aims which have decided the social results of the use of these technologies have not flowed from the inherent nature of science.[135]

Thus, Dewey's assertion quoted above that science 'is itself essentially a technology', seen here in context, turns out to be Dewey's foil—just as the like assertion had been Auerbach's—in making the *opposite* point, namely, that there is in just this regard an intrinsic difference between science and technology, a difference that makes all the difference.

What Dewey here voices is what in every era prior to postmodernity nearly everyone firmly believed: the pursuit of science is intrinsically incompatible with the motives that ordinarily orient technological activities. We have seen even a Veblen insist upon this, and so we should not be surprised that Dewey did too. Among the many instances—early and late—none better points up the alieneity to postmodernity of that presupposed incompatibility than the fantasy by which Dewey, addressing Harvard University's Tercentenary Conference (1936), sought to convey 'the difference between the aim and operation of the free individual in the sphere of science and in that of

current individualistic economic enterprise'. To grasp this difference—a difference that we in postmodernity no longer recognize—Dewey urged upon his auditors their,

> … stretching the fancy to the point of imagining a scientific inquirer adopting the standards of the business entrepreneur. Imagine the scientific man who should say that his conclusion was scientific and in so saying maintain that it was also the product of his private wants and efforts goading him on to seek his private advantage. The mere suggestion of such an absurdity vividly discloses the gap that divides the manifestations of individual freedom in these two areas of human activity.[136]

That is, what today we take as a matter-of-course mix of motives in any and every scientist, whatever the setting and the goals of their seeking, seemed to Dewey totally immiscible motivations. So much so that Dewey could offer his Harvard Tercentenary audience the fantasy of their easy coexistence in a scientist as a *reductio ad absurdum* proof of the incompatibility between being personally interested and being scientific.[137]

Dewey's conviction of the intrinsic goodness of science was no less modern than ancient—and all the more modern in that he identified science with the method employed more than with the knowledge attained. However, there was a problem, a very large problem for a philosopher largely concerned as Dewey was with the inequities and iniquities of the world as it is:

> factually speaking, the present human scene, for good and evil, for harm and benefit alike, is what it is because … of the entry into everyday and common (in the sense of ordinary and of shared) ways of living of what has its origin in *physical* inquiry.[138]

But if *physical* inquiry—and the emphasis is Dewey's—is the veritable *fons et origo* of the modern world, then Dewey, having traded his theistic Hegelianism for an atheistic scientism, needed a scientistic theodicy. It was this: if the success of physical inquiry produced much harm and evil on the human scene, that was proof only that mankind had not yet learned enough from the success of physical inquiry—had not learned to emulate that success and to order by intelligence the human scene as well.[139]

Dewey stated and restated that faith over and over through five decades. Writing early in 1927, at the broad peak of his influence, he concluded his reply to an attack upon pragmatism by Lewis Mumford—an attack that Dewey had reason to feel to be traitorous as well as insulting:

> The implied idealization of science and technology is not by way of acquiescence. It is by way of appreciation that the ideal values which dignify and give meaning to human life have themselves in the past been precarious in possession, arbitrary, accidental and monopolized in distribution, because of lack of means of control; by lack, in other words, of those agencies and instrumentalities with which natural science through technologies equips mankind. Not all who say Ideals, Ideals shall enter the kingdom of the ideal, but those who know and respect the roads that conduct to the kingdom.[140]

Dewey's reference here to 'those agencies and instrumentalities with which natural science through technologies equips mankind' is indicative of a view of technology as derivative from science. It was indeed Dewey's view that science held such comprehensive primacy to and for technology. Nor could it possibly have been otherwise. The 'in principle' primacy of practice not withstanding, the hackneyed geneses of geometry

and astronomy in practical activities not withstanding, the view of science as 'essentially a technology of apparatus' notwithstanding, Dewey was so firmly possessed by so many prejudices ranking science above technology in so many respects, that when he addressed the relation between the two he had necessarily to regard technology as subordinate and derivative entity. Over and over again Dewey states his complete certainty that 'modern industry *is* so much applied science';[141] that 'in fact, the entire modern industrial development is the fruit of the technological applications of science';[142] that 'practically every phase of our present technique of industry and commerce has its roots in some discovery made somewhere in some laboratory by some scientist engaged in physical or chemical research';[143] 'that the application of natural science, through the medium of inventions and technologies, is the finally controlling and characteristic fact of modern life'.[144] One would be hard pressed to find any firmer or more categorical assertions of the primacy of science to and for technology than those Dewey provides, in plenty.[145]

Engineers and Industrialists

We have seen that in modernity even grand theorists who took as axiomatic the determinative role of a society's material practices for the character and the contents of its higher cultural productions, disregarded the logic of their intellectual positions when addressing the relation between science and technology. Similarly, as is now to be shown, in modernity nearly all of those who earned their livings maintaining, multiplying, and milking their society's material practices disregarded the logic of their social positions and affirmed science's primacy to and for technology. The evidence here presented relates to the USA and to Germany, only. There are, however, in all European countries—and, in consequence thereof, in India, China, Japan and elsewhere around the world—many indications of prevalence of the conviction of the primacy of science among engineers and industrialists, as well as among the elites exercising the powers of government.[146] France, in particular, is regarded by all who have made her their special subject as pre-eminently a country and culture affirming the primacy of science, and more especially 'of pure science, of ivory tower science'.[147] At the other pole, ideological and geographical, anti-occidental Japanese racial and cultural nationalists of the 1930s, though they rejected imitation of the West and insisted on 'a uniquely Japanese path of technological development', nonetheless took the primacy of science as beyond question:

> The existence of scientific research, which may give birth to this new technological creativity, will provide a firm basis for the cultivation of the Co-Prosperity Sphere, and for this reason the promotion of such research is currently an urgent necessity.[148]

Between these two poles, stretching from the one very nearly to the other, Soviet Russia, just as Tsarist Russia, gave quite preposterous primacy to science in its ever-renewed effort to 'catch up and surpass' the West.[149]

The main focus of the case that I seek here to make is the USA, and so I return to Gano Dunn, the man of affairs and influence at the national, even international, level, whose

extravagant obeisance to science I quoted at the outset of this exposition of the primacy of science in modernity. Kline has amply documented the near universality of the overestimation of science among American engineers from the late 19th century to the mid 20th century, and shown that it is only in rhetorical excess that Dunn departed from the commonly expressed view of his peers.[150] Thus Dunn's affirmations to his fellow Columbia University alumni in 1930—which reproduced in heightened rhetoric the content of his 1912 presidential address to his fellow members of the American Institute of Electrical Engineers—were, Kline points out, very similar to the propositions that J. J. Carty, chief engineer of American Telephone and Telegraph (AT&T), put to that same professional association in 1916 in *his* presidential address.[151] Indeed, Kline found that throughout the half-century before the Second World War 'presidents of engineering societies followed the time-honored tradition of tying their field to science (in a subordinate manner)'.[152]

That subordination of technology to science was both in respect of science's role and in respect of science's rank: 'The investigator in pure science', said Carty,

> may be likened to the explorer who discovers new continents or islands or hitherto unknown territory. He is continually seeking to extend the boundaries of knowledge.
>
> The investigator in industrial research may be compared to the pioneers who survey the newly discovered territory in the endeavor to locate its mineral resources, determine the extent of its forests, and the location of its arable land, and who in other ways precede the settlers and prepare for their occupation of the new country.[153]

Ironically—and because ironically, significantly—the enormous expansion of industrial research in the USA in the early 20th century, far from diminishing the putative purity of the extra-industrial investigator, had the opposite effect on the common representation of the scientist: in order to differentiate science from industrial research, even spokespersons for engineering and industry were pushed to heighten still further the asserted cultural rank of the pure scientist, exaggerating still further science's primacy relative to technology. Thus Carty, continuing with this exaltation of the scientist, explained to his audience of electrical engineers that 'the work of the pure scientists is conducted without any utilitarian motive, for, as Huxley says, "that which stirs their pulses is the love of knowledge and the joy of discovery…"', and consequently that in the scientist 'there must be some of the divine spark and for him there is no higher motive than the search for truth itself'.[154]

Such extraordinary virtue and merit as Carty attributes to the scientist must needs create a corresponding moral obligation in engineers, and all the more so as engineers are so directly and so heavily dependent upon the results of the scientists' inspired efforts. Thus Carty, in concluding his 1916 presidential address, urged upon his audience:

> By every means in our power, therefore, let us show our appreciation of pure science and let us forward the work of the pure scientists, for they are the advance guard of civilization. They point the way which we must follow.[155]

Carty's declaration was made as leading figures in American science, following the example of European countries locked in the First World War, were beginning to turn their attention from making discoveries to making weapons. It was reaffirmed nearly

30 years later in a late phase of the Second World War, with its far more thorough mobilization of scientists into technologists, by the man who then held Carty's hugely enlarged research portfolio: 'all that we call "technology"', declared Frank Jewett, head of AT&T's Bell Telephone Laboratories, 'is nothing but the application of fundamental science discoveries and the employment of scientific methods for useful or desirable purposes'.[156] This conviction was in no way peculiar to the Bell System. On the contrary, Kline found that in the inter-war years 'leaders of industrial research at General Electric advocated the … pure-science ideal more strongly than Jewett'.[157]

The form of Carty's above-quoted invocation of T. H. Huxley shows that his engineering audience was well familiar with his authority and his theses. Indeed, one of the stereotypic tropes through which American engineers exalted the pure science ideal and enacted ritual self-disparagement was affirmative quotation of Huxley deploring 'this phrase "applied science".' In 1880 at the inauguration of Josiah Mason's Science College at Birmingham, and specifically in the context of defending the planned science-based curriculum for that institution—whose purpose the eponymous benefactor had defined as being 'to promote the prosperity of the manufactures and the industry' of Britain—Huxley had said:

> I often wish this phrase, 'applied science,' had never been invented. For it suggests that there is a sort of scientific knowledge of direct practical use, which can be studied apart from another sort of scientific knowledge which is of no practical utility, and which is termed 'pure science.' But there is no more complete fallacy than this. What people call applied science is nothing but the application of pure science to particular classes of problems. [158]

That is, Huxley deplored the concept of 'applied science' because it could lead the practical-minded person to think that pure science is unnecessary to his practical interests, as though there were some other form of science that is sufficient to the needs of the practical man. To the contrary, Huxley insisted that pure science is not only the only science, it is also the only significant source of such technologies as involve science.

Kline set Huxley's statement as epigraph to his paper. In his most direct reference to it Kline wrote:

> In 1928 Charles Richards, former dean of engineering at two mid-western universities, repeated Huxley's argument of nearly fifty years earlier when he said that engineering 'can not be classed as a fundamental science. … It has often been classified as an applied science, although the term would seem to be a misnomer, for there is no other science than pure science.' [159]

I.e. Richards regretfully renounced engineering's claim to be science of a sort. Surveying presidential addresses to American engineering societies in the late 19th and early 20th centuries, Kline found that they 'ignored the autonomous sense of *applied science* and moved its meaning toward the subservient end of the spectrum.' [160] Similarly, toward the end of that period, '[MIT vice president Vannevar] Bush and other advocates of engineering research in academia took rather weak rhetorical stances against the … pure science ideal.' [161] If Gano Dunn was in any way out of line with his rhetorically extravagant obeisance to science, it was rather that:

> By calling the engineer's creativity 'art,' the traditional definition of engineering, Dunn allowed his profession some autonomy within a hierarchical relationship that subordinated it to science. [162]

Thus Kline stressed that in the seven decades following Huxley's 1880 address, American engineers often construed technology not as 'applied science,' but as the application of *pure* science. In so doing they underscored the primacy of *pure* science and the indispensability of the pure scientist for technological advance, and they affirmed a cultural hierarchy subordinating technology to science.

A further indication of the American engineers' posture of proud subservience to the interests of higher forms of culture, and to those embodying them, is their appropriation of Rudyard Kipling's 1907 poem, 'The Sons of Martha'.[163] The poem celebrated engineers as spiritual sons of Martha, Lazarus's sister—she who, though she performed for Jesus all the practical services of hospitality, was disregarded by Jesus in favor of Lazarus's sheerly worshipful sister, Mary.[164] The poem opens:

> The Sons of Mary seldom bother, for they have inherited that good part;
> But the Sons of Martha … must wait upon Mary's Sons, world without end, reprieve, or rest.

and its concluding stanza is:

> The Sons of Mary smile and are blessed—they know the Angels are on their side.
> They know in them is the Grace confessed, and for them are the Mercies multiplied.
> They sit at the feet—they hear the Word—They see how truly the Promise runs.
> They have cast their burden upon the Lord, and—the Lord He lays it on Martha's Sons!

Ruth Oldenziel has drawn attention to the pervasive and continuing appeal to engineers of Kipling's parabolic presentation of the ideal of selfless self-sacrifice for the good of the truly good.[165] While it must be said that citation of 'The Sons of Martha', in contrast with quotation of Huxley on 'applied science', provided engineers with definite grounds for self-esteem, the implication of their subordination in cultural rank, and their (self-imposed) subservience to those of higher cultural rank, remains just as clear and strong as in Dunn's characterization of the relation between the engineer and the scientist.[166] Pride of place, rather than of service, is the privilege of persons devoted to higher cultural goods and functions, among whom pure scientists held primacy.[167]

This preposterous disparagement by technologists of themselves and of their works relative to 'pure science' was, manifestly, a general and consistent feature of American technological ideology from the Gilded Age into the first decades of the Cold War. This is not to be rationalized by reference to any direct material or social interest of the engineers. On the one hand, as Seely has shown, through most of this period—indeed, well into the 1930s—American engineering education was not, and did not seek to be, based on science in any direct or substantial way.[168] That is, American engineers held and articulated a primacy-of-science ideology through a half-century during which their practice was not, in general, based in science, and during which they were making no serious effort to alter their practice in that direction. At MIT in the first decades of the 20th century the trustees chose a succession of physicists to head the institution, of

which the first, theorist Robert C. Maclaurin (1909–20), was highly successful in raising money from wealth industrialists on its board with a rhetoric of pure science as base for engineering.[169] This remained largely merely rhetoric until in 1930 trustees Gerard Swope, head of General Electric, and Frank Jewett, head of AT&T's Bell Telephone Laboratories, initiated the transformation of MIT into a science-based university by bringing in Karl T. Compton, who did not merely insist 'that "purely scientific research" was "absolutely prerequisite and basic to invention and development"', but energetically set about a corresponding reform of the institution.[170]

Neither was this paradoxical exaltation of science over engineering a stratagem enabling leading engineers and industrialists to evade concern with or responsibility for the support of 'pure science'. On the contrary, as early as 1880, R. H. Thurston, as President of the American Society of Mechanical Engineers, had 'exhorted the mechanical engineers to "find for these self-sacrificing students of science, pure and applied, means sufficient to enable them to work efficiently and productively"'.[171] As we have seen, in his 1916 presidential address to the IEE, Carty coupled his exaltation of the scientist with an explicit appeal to his fellow engineers to forward the scientists' work 'by every means in our power'.[172]

Two decades later, in the depths of the Great Depression, one finds again that same sense of obligation to, and concern for, the welfare of 'pure research', but with an emphasis upon practical benefits, befitting that time and the altered importance in practice of science in the interim. Such is reflected in the formation by the National Association of Manufacturers of a Joint Committee on Scientific Research. That committee, on which scientific research was represented by pure scientists almost exclusively, had as announced objective 'to raise the standard of living and otherwise benefit society as a whole through increasing production and employment by means of a more widespread use of scientific research in industry'. To this end it undertook to 'Study the importance of pure research to the public generally and the problem of its adequate financing', and to 'Promote the thought of employing scientists in industry for the purpose of interpreting the trends and results of pure research'.[173] Similarly, Kline found that 'industrial researchers at RCA, the Mellon Institute and AT&T … all paid homage to research in "pure" or "fundamental" science, either in universities or in their own laboratories, as the basis of their success'.[174]

Fully consistent with these circumstances is the fact that, notwithstanding the great change in the typical life-world of a physicist as a result of the enormous growth of industrial research, the pure science rhetoric and ideology adopted in the last decades of the 19th century by American physicists did not change in any significant way during the first decades of the 20th century.[175] The explanation for this unchanging ideology despite substantially changed institutional reality lies, presumably, in the increased support that ideology received, not only in the culture at large but more specifically from leaders of industrial research and technologic enterprise. This, again, is fully consistent with the historiographic thesis here advanced, namely, that the actual, factual relations between science and technology are not, in the main, determinative of the historical succession of assumptions and opinions about that relation: the putative relation between science and technology is not, primarily, a matter of fact, but of cultural values.

In the decade following the Second World War the ideology and self-image of US physicists did indeed change in important ways,[176] but not at all in regard to the relation between science and technology. This comes forward clearly with 'the Bush report'—in its title, in its text and in the history of the writing and the reading of *Science, the Endless Frontier*. Thus the stereotypic metaphor of exploration, embodying the indispensable leadership of science, is patent in its title just as it underlies its text. Bush's 'idiosyncratic' advocacy of Wright Brothers'-like innovative activity as also being properly within the scope of the proposed National Research Foundation was ridiculed by his drafting committee. He obtained the inclusion of more elevated sorts of engineering research only by overriding his committee.[177] 'Yet', Kline observed with evident disappointment, even 'these modifications to the pure-science-ideal—amounting to what may be called a "basic research" ideal—seem to have been overlooked by most readers of *Science, the Endless Frontier*—then and now.'[178] Rather, what almost all readers took away from the Bush report was what they brought to it, the unqualified affirmation of 'the linear model' of technological innovation:

> Basic research leads to new knowledge. It provides scientific capital. It creates the fund from which the practical applications of knowledge must be drawn. New products and new processes do not appear full-grown. They are founded on new principles and new conceptions, which in turn are painstakingly developed by research in the purest realms of science.[179]

That 'age of faith in the linear model', as Hounshell aptly characterized the two decades following the Second World War, brought a proliferation of industrial research facilities in splendidly modernistic architectural isolation from manufacturing activities.[180] Viewed from postmodernity, the preposterous credulousness of that mindset seems as alien as that of the age which expressed its faith through the architecture of gothic cathedrals. 'If the General Electric laboratories have taught any lesson', a 1950 editorial in *The New York Times* stated, 'it is this':

> There is nothing so impractical in science as the man who thinks only of profits when he endows research; there is nothing so practical as the theorist who gives not a thought to money.[181]

That faith in science—in 'pure' science, 'abstract' science, 'theoretical' science, 'basic' science—in disinterested science—as the *fons et origo* of all major technological advancement, remained virtually unchallenged through the early 1960s.[182]

Indicative of this postwar persistence of the preposterous primacy of science to and for technology is William O. Baker's prepared response to C. P. Snow's address, 'The Moral Unneutrality of Science', at the 1960 meeting of the American Association for the Advancement of Science.[183] Baker, in the direct line of descent from Carty and Jewett, had come up through the ranks at Bell Telephone Laboratories, becoming director of research in 1955; he would head the laboratory, 1973–1979. Himself a physical chemist, Baker had been chosen 'to respond on behalf of the community of scientists'.[184] He proposed instead to 'remark on a rather practical aspect of the matter, as becomes an industrialist'.[185] Yet so far from bringing any perspective that we could today regard as appropriate to an industrialist, Baker disregarded the openings offered

by Snow to stress that engineering too was a source and an embodiment of moral values. In particular, Snow had insisted that the engineer was the scientist's peer in respect of the aesthetic aspect of their activities.

> We should not restrict the esthetic values to what we call 'pure' science. Applied science has its beauties, which are, in my view, identical in nature. … Right down in the field of development, the esthetic experience is as real to engineers. When they forget it … engineers are the first to know that they are lacking in virtue.[186]

Baker, however, found in engineers—that class of researchers forming the overwhelming majority of his laboratory's workforce—no virtues worth mentioning, and instead placed all hope—including hopes for 'a cure for cancer, … a defense against nuclear weapons, … more reliable moon rockets'—upon 'the ideas of scientific discovery' that 'come one at a time from one person and one mind at a time'.[187]

Astonishing, but true—and what is even more astonishing, and more important for the historian to bear in mind, is that no one was astonished at that time. Yet how quickly that time would pass! It had taken more than three centuries for faith in disinterested science as generator of technology's miracles to reach this height; in less than another three decades that faith had all but vanished. The very rapidity of the descent from that height is a strong argument that the rise of skepticism about science as source of technological innovation is not to be attributed to the few, partial, and equivocal empirical disproofs of the alleged dependency of new technology upon scientific discovery that appeared here and there in the 1960s.[188] Rather, that descent, most precipitous in the decade between the mid 1970s and the mid 1980s, was a concomitant of the melting away of the modernist cultural presuppositions upon which science's exalted rank and role relative to technology were based.[189]

Among engineers and industrialists Americans were not exceptional in according preposterous primacy to science relative to technology. In Germany too leading industrialists accorded science a primacy that was, as historians of technology there have recently observed, 'more ideologically than rationally based'.[190] This circumstance I illustrated above through the conception of 'physical–technical research' put forward in founding the Helmholtz-Gesellschaft in the aftermath of the First World War. Two decades earlier, that same deference to natural science on the part of German industrialists and engineers was displayed more prominently, more indicatively and more influentially in founding the Deutsches Museum.

Conceived and directed by Oskar von Miller, a highly successful and highly energetic electrical engineer, Das Deutsche Museum von Meisterwerken der Naturwissenschaft und Technik in München (The German Museum of Masterworks of Natural Science and Technology in Munich), founded in 1903, quickly became the world's foremost technological museum. Receiving its financial support largely from German industry, the museum was intended to be a monument and an inspiring testament to the achievements of technologists, most especially German technologists.[191] Nonetheless—no, not nonetheless: necessarily—natural science was given precedence in its title. In its 'logo', similarly, the owl of science was perched above the gear wheel of technology. This

matter of the relationship between science and technology was dealt with quickly and decisively by its 'Protector', His Royal Highness, Prince Ludwig of Bavaria, as the opening speaker at its founding meeting:

> It is not my task here to make a long speech, but surely I am allowed to draw attention to the great significance of technology. Technology is nothing other than physics and physics nothing other than natural science. The difference consists in this: physics teaches us theoretically about the forces of nature and their utilization; technology carries that utilization into practice.[192]

However pompous, the prince was not being idiosyncratic. The preliminary statutes adopted at that founding meeting defined the purpose of the Deutsches Museum as 'to show the influence of scientific research upon technology and to make visually apprehensible the historical development of the various industries, especially through outstanding and typical masterworks'.[193]

If this statement left open just how exclusively the museum would propagandize for a conception of technology as dependent upon science for *every* step forward, the question was effectively decided through the adopted guidelines for collecting and exhibition:

> Such of the various branches of technology as stand in close connection with science are to be presented developmentally by means of historic apparatus, models, other typical and outstanding masterpieces, as well as by means of drawings and documents. Attention is to be given both to those branches of industry that originated out of scientific research and also to those branches whose later development has been outstandingly influenced by means of scientific efforts.[194]

In the following years this programmatic primacy of science was indeed extensively realized in the museum's practices, with physics dominating the collections and astronomy being given an especially large and prominent place in the initial exhibition.[195] While we today would regard astronomy as well suited to illustrate a view of science as based in and dependent upon technology,[196] such was by no means the museum's message. Rather, here as elsewhere, 'the technical artifacts stood above all else for scientific disciplines or were scientific ideas materialized'[197]—with the astronomer who conceived and employed the instrument, not the exceptionally skilled and ingenious instrument-maker who created it, accorded the role of its 'inventor'.[198]

Yet it is also true that in Germany, unlike the USA, protests were raised against this intellectual and cultural subordination of technology to science, and more especially against the social subordination of the graduates of the *Technische Hochschulen*, the German engineering schools. The last years of the 19th century saw a revolt in the *Technische Hochschulen* against the large place that had earlier been accorded mathematics in the education of engineers—a revolt occasioned in large part by the disciplinary drive of mathematics toward increasing abstraction.[199] At the same time, and pointing in rather the opposite direction, these institutions began to agitate to obtain the right to confer doctorates—in consequence of which that right was accorded the Prussian institutes of technology in 1899 and to the others soon after.[200] The 'Dr.Ing.' and the subordination of mathematics to engineering instruction in the *Technische*

Hochschulen put an end, largely, to organized efforts for institutional reforms, but some few individual engineers remained outspoken in deploring the continuing social and cultural subordination of *Technik*. Loudest and probably most influential among these voices of protest was that of Alois Riedler, professor of mechanical engineering at the *Technische Hochschule* in Berlin. For Riedler, who had been instrumental in fomenting the revolt against pure mathematics in the *Technische Hochschulen*, had the ear of the technology-friendly Kaiser Wilhelm II. When the constitutive documents of the Deutsches Museum came to Riedler's attention, he wrote von Miller demanding that the museum not be used to pay homage to science, but to promote a conception of technology *as* culture.[201] Although our present day historians of technology are generally inclined to side with Riedler, and thus to attribute considerable importance to his protest, Füssl points out that it found no resonance at that time, in that place: 'scarcely one of Riedler's academic colleagues, to say nothing of the majority of technologists and engineers, joined with his critique'.[202]

'Revolts' Against Science

Can the matter really stand as I have presented it? Is not the putative primacy of science already disproved by the fact of revolts against science, of which historical scholarship has recognized quite a few in the last two or three centuries of modernity? However, the historical-conceptual category 'revolt against science' is itself rather broad and loose. It requires to be examined in order better to understand what bearing the episodes so comprehended can have for the thesis of this paper, the primacy of science to and for technology in modernity. While some 'revolts' make seemingly inescapable difficulties, most of the episodes that have been categorized as revolts against science testify either to a conviction that science in fact enjoys a primacy that 'rightly' it should not have, or to a conviction that science is primarily responsible for the world being in so bad a way as it is—or both. Either way, they are implicit confirmations, fully or partially, of the primacy of science.

The revolts against science that present the clearest contradiction of my contention that modernity was characterized not only by a presumption, but also by an affirmation, of the primacy of science to and for technology are the populist revolts. National Socialism was such a populist revolt.[203] Similarly populist were the anti-intellectual ideologies of Jacobin France, Jacksonian America and Bolshevik Russia.[204] Denials of science's primacy—of science's right to primacy from any perspective, but specifically with regard to its role in relation to technology—followed from each of these rejections of, even assaults on, the alleged hegemony of a learned elite with its claims to superior knowledge. Yet these aberrations gained traction for only relatively brief periods, the longest by far being that of the Jacksonian decades in antebellum America. From about 1820 into the 1840s the American popular mind was intimidatingly disrespectful of all forms of elite culture.[205] That phase passed, and by about 1850 the American cultural elite was giving voice to a sense of relief.[206]

By contrast, the romantic revolt against science that thrived in Germany in the last decade of the 18th century and the first decades of the 19th, for all its romanticization

of 'the people', was never populist, always tended toward elitism in relation to culture as in ethical and aesthetic sensibilities. In that regard, but still more in the breadth and duration of its appeal, romanticism makes the most serious difficulty for my thesis of the cultural primacy of science in modernity. For unlike the populist aberrations, romanticism remained a powerfully influential tradition in German society into the late 20th century, while elsewhere—everywhere—significantly influencing the outlook and the self-conception of self-consciously creative types: artists, most especially, but in varying degrees also scientists, and humanistic scholars, and, yes, even engineers. Indeed, some of the most romantic self-fashionings were those of engineers.[207]

Yet by and large neither the episodic society-wide romantic revolts, such as that arising in Germany following its defeat by Napoleon Bonaparte, and again that following its defeat by Woodrow Wilson, nor the continuing romantic strain in the mindset of those self-consciously creative of culture, altered the role and rank relations between science and technology in modernity. Making allowance for elements of an inversion of those relations with Heidegger and a few of his contemporaries, the primacy of science to and for technology was never in question among pre-postmodern romantics. This was in no small part because however much romanticism deprecated natural science, it deprecated technology even more.[208]

All the aforementioned revolts against science present some difficulty, more or less serious, for the thesis of this paper. It is therefore with some relief that I turn to the 'revolts against science' in 20th century America. For these quite consistently are of the 'science is responsible' sort, and are as such implicit acknowledgements of science's primacy.[209] This is evident if one bears in mind that such revolts are but the reverse of the almost universal readiness to credit science for all changes in the life-world that are found wonderfully good. This readiness, so commonly noticed as to be a cliché, was placed on a scholarly foundation by Marcel Lafollette.[210] From her quantitative study of the representation of science in American middle-brow magazines over the first half of the 20th century, she too gained the strong impression that 'Each new product and social improvement brought praise to scientists, no matter how little they had actually contributed to its development'.[211] It is therefore only logical that the 'dilapidated waterfronts, … hideous … factories, endless rows of monotonous dwellings, the unceasing roar and grind of urban life', led the Bishop of Ripon, joining in 1927 with 'humanist' critics of modern civilization on both sides of the Atlantic, to call for a moratorium on scientific research.[212] That is, with the same logic, if the material conditions of life were deplorable, the blame was laid upon science, no matter that science actually contributed little, either directly or indirectly, to producing the deplored conditions.

Similarly, when the Great Depression set in, albeit that the massive unemployment was commonly described as 'technological', it was again science that was widely held to be responsible.[213] For nuclear weapons science largely escaped blame, partly because the first to deplore them were the atomic scientists, partly because science was so generally seen as indispensable to deterring Soviet aggression, but partly also simply because at that moment the general inclination was to praise, not blame, science. When, out of

the 1960s, there emerged intense anxiety about the effects of altered environmental conditions upon health, it was once again science that was blamed. President Johnson, handing out the 1968 National Medals of Science, reminded the attendees that 'An aggrieved public does not draw the fine line between "good" science and "bad" technology'.[214] While for critics farther on the left there was no line left: 'precisely science which should have been the wind of truth to clear the air, has polluted the air …'.[215]

Unmistakable in these reproaches is the presumption of science's comprehensive responsibility for the world in whatever way and to whatever extent it has recently been altered by man. It follows then, both logically and in fact, that through the first three quarters of the 20th century the public looked to science's leadership in the remediation of perceived social problems as much as in the achievement of new technological goals. The Bishop of Ripon had his tongue half in cheek in 1927 when surmising that 'the sum of human happiness, outside of scientific circles, would not necessarily be reduced if for, say, ten years every physical and chemical laboratory were closed', but he then went on show his own faith in science by suggesting more seriously that those evicted researchers could, if they then put their minds to it, make a big contribution to solving the problems of human society.[216]

It was therefore a more than logical response to 'the rising revolt against science'—science as responsible for the industrial system in collapse—that in 1933 President Roosevelt appointed a Science Advisory Board,[217] and so likewise the National Association of Manufacturers's appointment four years later of a pure-scientist-dominated Committee on Scientific Research.[218] If FDR's exceptionally un-ideological, planning-oriented, inner circle expected little immediate relief of the nation's pressing economic and social problems to come from the expenditure of large sums for fundamental research in the natural sciences as was recommended by the president's Science Advisory Board, their skepticism did not extend to doubts that science was fundamental to every form of engineering, social as well as industrial.[219] In any case, the success of the mobilization of scientists to develop new technologies during the Second World War quashed almost all skepticism from whatever direction and greatly strengthened the public's faith in technical leadership by scientists.[220]

The fullest expression of this faith in the scientist as both seer and masterer of technology was the appointment by President Eisenhower, late in 1957, in response to the crisis created by the Soviet Union's launch of an earth satellite, of a President's Science Advisor and a President's Science Advisory Committee. Although the committee was composed almost entirely of scientists—and those by no means 'rocket scientists', but basic researchers—science was the smallest part of its 'flabbergasting array of responsibilities'. The President, pressed and supported by the Congress, had accorded to scientists the roles of critic, judge, and leader in all matters relating to technology, ranging from new technologic systems, through remediation of problems with deployed technologies, to technologic aspects of economic policy.[221]

In a very similar way in West Germany, as Carson and as Osietzki have pointed out, the members of the *Deutscher Forschungsrat*—formed around 1950 to advise the central government on matters of science policy, and perhaps to play a more than advisory role in the distribution of research funds—were initially drawn almost

entirely from the pure, natural, sciences, for it was taken as a matter of course that such scientists could and should speak also for technical fields.[222] Likewise, Carson found, in the conception and promotion of a nuclear reactor station for West Germany in the mid 1950s,

> The plans for the center, like many of that era, were instinctively predicated on the predominant linear model of technology development—new science generates new technology, and new technology changes the world. … Science, finally, would supply the knowledge, imagination and (though this was negotiable) a substantial component of leadership.[223]

Just how unthinkable at that time was any other direction in which to seek salvation is suggested by the views of the otherwise iconoclastic Robert M. Hutchins. Then directing his Center for the Study of Democratic Institutions, Hutchins organized in 1962 a 'Conference on the Technological Order', inspired directly by Jacques Ellul's *The Technological Society*.[224] Notwithstanding that Ellul explicitly denied science's primacy for technology, and notwithstanding that Hutchins was at about this same time expressing a very derogatory view of the moral character of natural scientists, at this conference Hutchins, quite in agreement with the other conferees, saw only one promising approach to the solution of the problems of the technological order: the recruitment of scientists into government to work on those problems.[225]

Lewis Mumford

In the 1960s and early 1970s Lewis Mumford reached the apogee of a career that had begun about 1920. Over six decades as wide-ranging and hugely prolific critic, synthesist, and proponent of cultural renewal in the broadest sense—25 books, over a 1000 articles, columns, and reviews—Mumford was one of the most prominent 20th century American 'public intellectuals', and he remains to this day a figure of prime importance for several scholarly fields, the history of technology most especially.[226] Only with *The Pentagon of Power* (1970), however, did Mumford achieve the book-of-the-month-club middle-brow success that made his tirades against 'the megamachine' and 'science as technology' seem to spokespersons for science as among the most serious attacks they had to confront in that mutinous time.[227] Mumford was widely taken then, and he has been taken by historians of every stripe since then, as placing little value and much blame on science, science both as a mode of thinking and science as shaper of the contemporary life-world. Not without some reason: all Mumford's writing was imbued with 'Life' rhetoric and appeals to romantic-*lebensphilosophisch* values, a claptrap with distinctly anti-scientific filiations.[228] Thus Mumford's view of science, and more particularly of the relationship between science and technology, is both a necessary test of my contention of the continued primacy of science in Western culture generally through the 1970s and a necessary preparation for discussion of the exclusion of science from the history of technology over the course of the 1970s. To anticipate: Mumford, for all his exceptionality, was in this respect—affirming the preposterous primacy of science relative to technology—no exception.

Born in 1895 in New York City, Mumford was of a generation of US artists and intellectuals who came to conceive themselves as in revolt against Victorian values, including, in particular, that full faith in science on which Deweyan pragmatism was founded.[229]

> Roughly speaking, most of my generation began as pragmatists. I studied Pragmatism and A Pluralistic Universe before I had read a word of Plato … and the very first biographic note appended to an article of mine in 1914 proudly stated that I was a pragmatist.[230]

At the date mentioned, and for almost a decade after, Mumford identified himself primarily with science and as scientist.[231] Now, however, early in 1927, addressing Dewey in the third person with some scorn and sarcasm—'where on earth has he been these last ten years, not to have felt the sting of this criticism before?'—Mumford proclaimed his generation's recognition of the insufficiency of Dewey's 'belief that simply by understanding science and technology conceived as instruments we are in a better position to fulfill the ends of life itself'.

> Acting alone, without a counterpoise in the creative imagination, our narrow instrumentalism has left us impotent: all the 'agencies and instrumentalities with which natural science through technologies equips mankind' will not in this situation help us much more than the king's horses and men helped Humpty-Dumpty.[232]

Mumford was not taking issue with the subordination of technology to science implicit in the phrase 'natural science through technologies', already quoted in discussing Dewey. There Mumford could not have imagined disagreement. What Mumford deplored was pragmatism's 'preoccupation … with science and technology' and 'the pragmatic acquiescence' to American materialism in the decades following the Civil War. Turning to Dewey's version of pragmatism, instrumentalism, Mumford turned *ad hominem*. Taking literary style as an indication of 'mental rhythm', Mumford judged that 'Mr Dewey's pages are as depressing as a subway ride', that 'protective coloration' was a predominant quality of Dewey's mind, and that instrumentalism was all too similar to the worldview of 'Mr Babbitt'.[233]

Strenuously though Mumford, sometime pragmatist, sought circa 1926 to distance himself from Dewey—as part of his program of exchanging his scientist persona for that of a man of letters—Mumford remained fundamentally the Deweyan: to almost every one of Mumford's dicta over the following years regarding desirable transformations of social and cultural life, one can pair a very similar Deweyan dictum.[234] As to the matter at issue—'natural science through technologies'—Mumford's appraisal of science, and not merely of science's role in relation to technology, was about as close to Dewey's as was possible given that Mumford sided with the romantic artist's insistence on personality as source of creativity,[235] and deplored the hegemony exclusive of Life that the abstract, mechanistic view of the world had won over man.[236] That is, the Mumfordian romantic-*lebensphilosophisch* indictment of physical science affirmed the primacy of science *for* technology in the strongest, most categorical terms, insisting on the causal, determinative, originative role of science in relation to technology.

> The original mistake, which was responsible for all this misery, was committed when our scientists began to create a new world of steel and iron and chemistry and electricity and

> forgot that the human mind … marches from one to three hundred years behind the small group of courageous leaders.[237]

Only if science is supposed to have such a role in relation to technology could Mumford's critique of the world view of physical science constitute an exposure of the basis and reason for our life world, the modern industrial–technical world, being as miserable as it is. Mumford's romantic rejection of scientism not only bespoke his presumption as modern of the primacy of science, but, ironically, led him, when he turned to the history of technology, to exaggerate science's putative primacy to and for technology.

The earliest version of Mumford's repeatedly retold dramatization of the historical process by which the worldview of physical science became hegemonic, became modern man's incontestable ontology, appears in the opening chapter of *The Golden Day* (1926):

> Once the European, indeed, had abandoned the dream of medieval theology … he turned to what seemed to him hard and patent reality: the external world. … he took refuge in abstractions, and reduced the rich actuality of things to a bare description of matter in motion. Along this path went the early scientists, or natural philosophers. By mathematical analysis and experiment. … weighing, measuring, timing, decomposing, isolating—all operations that led to results. … A new view of the universe … was accepted … because it was accompanied by so many cogent proofs of science's power. … science was ready, not merely to bake the bread, but increase the yield of the wheat, grind the flour, and eliminate the baker.[238]

The protagonists in this 'drama … of the coming of the machine into modern society', as Mumford would come to call it, are not technicists but philosophically engaged physicists. And the image of the scientist latent here is the well-familiar metaphor of the pathfinder—pathfinder in the realm of the mind, first of all, and then, consequentially and decisively, in the material world of technology and industry.

In 1930 in an essay in *Scribner's Magazine*, Mumford, then in his brief philotechnic phase, expanded his account of the coming of the machine and strengthened it with further metaphors—notably, scientists as a group constituting a military general staff:

> [T]he army of machines could not take possession of modern society until every department had been trained; above all, it was necessary to gather a group of creative minds, a general staff, who would see a dozen moves beyond the immediate strategy and would invent a superior tactics. These are the physicists and mathematicians; without their abstract descriptions, the useful habit of isolating certain movements and sequences would not have been adopted, and invention would probably have sought to reproduce—as in fact it first did—cumbrous mechanical men or mechanical horses, instead of their abstract equivalents, namely, steam-engines, locomotives, rifles, cranes. Behind the scientific advance-guard came the shock troops, the miners, the woodmen, the soldiers proper, and their inventive leaders. Five centuries were needed to set the stage for the modern world.[239]

All these metaphors and mechanisms by which scientists take precedence over technicists—that is, precede modern technologies and technologists temporally, causally, and honorifically—reappear in *Technics and Civilization* (1934):

> A series of thinkers, Bacon, Descartes, Galileo, Bacon, Pascal, defined the province of science, elaborated its special technique of research, and demonstrated its efficacy. … At the end [of the 17th century] despite the relative sterility of invention itself during this century, there existed a fully articulated philosophy of the universe, on purely mechanical lines, which served as a starting point for all physical sciences and for further technical improvements: the mechanical Weltbild had come into existence.[240]

> The general staff of science had worked out the strategy of the campaign long before the commanders in the field had developed a tactics capable of carrying out the attack in detail. … Leonardo, Andreae, Campanella, Bacon, Hooke … and Glanvill … wrote down in outline the specifications for the new order: the use of science for the advancement of technics, and the direction of technics toward the conquest of nature were the burden of the whole effort.[241]

That precedence of science to technology which Mumford ascribed there to the 16th and 17th centuries he then affirmed still more emphatically in describing the unfolding of this drama in the 19th and 20th centuries. Directly out of physical research and the extension of its methods to other subject matters was emerging a new and better phase of technological history, the neotechnic:

> With the neotechnic phase, two facts of critical importance become plain. First, the scientific method, whose chief advances had been in mathematics and the physical sciences, took possession of other domains of experience: the living organism and human society also became the objects of systematic investigation, and … the extension of science here was to have a particularly important effect upon technics.[242]

Moreover,

> In the neotechnic phase, the main initiative comes, not from the ingenious inventor, but from the scientist who establishes the general law: the invention is a derivative product. … The translation of the scientific knowledge into practical instruments was a mere incident in the process of invention.[243]

We have already heard from Dewey such denigration, relative to the abstract and disinterested scientist, of those many engaged in making things that work. Mumford, to the end of his life an unreconstructed cultural elitist, was much less inhibited than was Dewey in affirming a continuing adherence to a hierarchy of values deriving through Plato and Aristotle from antiquity:

> … the fact was that a liberated scientific curiosity might at any moment prove as valuable as the most factual pragmatic research. Indeed, this freedom, this remoteness, this contemplative isolation, so foreign to the push of practical success and the lure of immediate applications, began to fill up a general reservoir of ideas … it [science] came during the nineteenth century to act as a counterweight to the passionate desire to reduce all existence to terms of immediate profit and success. The scientists of the first order, a Faraday, a Clerk-Maxwell, a Gibbs, were untouched by pragmatic sanctions: for them science existed, as the arts exist, not simply as a means of exploiting nature, but as a mode of life: good for the states of mind they produce as well as for the external conditions they change.[244]

In just this regard Mumford's view of science changed drastically in the following decades—not Mumford's view of the ideality, nor the ultimate reality, of science so described, but his view of what in fact the pursuit of science had largely come to be in

the course of the 20th century. Yet in all of Mumford's subsequent railing against 'the expansion of science as mass technology' through which science has 'become demoralized by its very success as an agent of technology' and 'the scientist has forfeited the qualities that were exalted in the past as his special hallmark: his detachment from worldly gains and his disinterested pursuit of truth', there is no mistaking either the primacy that science continued to hold over technology in Mumford's mind—primacy in rank as well as in role—or the antipathy toward technologic activity as such that runs through all of Mumford's later writing.[245]

The Mumford emerging from the Second World War increased still further the primacy that in the 1930s he had attributed to science relative to technology. This came about in two ways, oddly antithetic but both typical for that period. On the one hand, this followed for Mumford, just as for his contemporaries generally, from the success of the wartime mobilization of scientists as technologists, especially their achievement of nuclear weapons.[246] The scientists' creation of new military technologies confirmed for Mumford his earlier view that the neotechnic technologies, those emerging in the late 19th and early 20th centuries, were to be ascribed almost wholly and solely to science. On the other hand, Mumford, like so many others, came out of the Second World War even more strongly committed than he had been in earlier decades to 'the primacy of mind'.[247]

With Mumford this late-modern mentalism expressed itself as a further diminishment of the importance to be attributed to 'tool-technics' and to those occupational roles engaging directly with them. Already in 1944 Mumford was half apologetic for *Technics and Civilization* having been 'wholly devoted' to technics.[248] In the remaining three decades of his writing life, Mumford down-graded 'tool-technics' and increased the importance, throughout all recorded history, of theoretical science and of the persons creatively engaged with it. Regarding material technics, generally, and tool-technics, more especially, as 'but a fragment of biotechnics: man's total equipment for life', Mumford insisted that 'man's overdeveloped and incessantly active brain' was far and away the most important part of that equipment.[249]

In the first volume of *The Myth of the Machine* (1967)—and one of the intended allusions in that title is doubtless to his new big idea of the small importance of 'nuts and bolts' machines—Mumford moved the epoch crucial for inception of mechanization back from the 16th and 17th centuries to the 4th millennium BC. The decisive technical transformation at that dawn of civilization was the creation of the 'megamachine', a 'radically new type of social organization', that is, a merely 'abstract mechanical system'. Composed wholly of men and ideas, 'a product of myth, magic, religion, and the nascent science of astronomy', the megamachine 'sprang directly' from 'astronomical observations and scientific calculations'.[250]

Evidently, then, from early to late, Mumford affirmed emphatically the primacy of science for technology, elaborating accounts of how technology, ancient and modern, originated from science. More than that: Mumford affirmed not only the modern presumption of the primacy of science *for* technology, but also the presumption, both modern and premodern, of the primacy of science *to* technology. Worse than that:

Mumford, as romantic, had a consistently unfavorable view of technology, and of engineers more especially—excepting, but only partially, the years from 1927 to 1935.[251]

In 1927, Mumford quite suddenly acquired, and then around 1934, almost as suddenly lost, a belief in functional architecture and the machine aesthetic as embodying and symbolizing the emergence of a new cultural synthesis.[252] Yet far from that brief upward saltation in Mumford's valuation of technology being accompanied by a diminishment of the value Mumford placed on science, just the opposite was the case: given Mumford's presupposition of the primacy of science for technology, any elevation of technology could serve only to elevate Mumford's valuation of science that much more.[253]

Even as Mumford was writing *Technics and Civilization* (1934), his philotechnic phase was waning. The *telos* of Mumford's narrative in that book, and the prospect that loosed his effusions there were of a life-fulfilling 'neotechnic' era in which man, society and culture once again form a beautiful whole, as they had in medieval Europe.[254] Yet by 1934 Mumford was no longer able, as he had briefly been a few years earlier, to be enthusiastic about the engineer, i.e. about those persons professionally engaged in the creation, deployment and maintenance of neotechnics. Notwithstanding that the neotechnic is exactly coincident with the period in which the professional engineer becomes the characteristic, indeed nearly exclusive, operative and improver of technics in all its aspects, Mumford gave the credit for neotechnics only to scientists, plus a few 'distinguished individual inventors like Edison, Baekeland, and Sperry'.[255] In so doing, Mumford expressed disdain for 'the specialized, one-sided, factual education of the engineer', and adduced as its typical product Hans Castorp, the 'half-baked nautical engineer' in Thomas Mann's *The Magic Mountain*.[256]

As Mumford downplayed 'tool-technics' in the following decades, he found no occasion to elevate the engineer, not even the 'systems engineer'. In all of those later books—and not least in Mumford's biggest, *The City in History* (1961)—even where the subject itself demands attention to the engineer, Mumford pays almost none. The accomplishments of engineers, if they are described at all, are treated cursorily and grudgingly.[257] Thus the engineer and engineering are almost absent from *The Pentagon of Power* (1970)—are not to be found in the index—notwithstanding that the book's temporal focus is the 20th century. The valuations underlying that absence of the engineer and engineering were made explicit in an essay, 'Prologue to Our Time' (1975), that among Mumford's late writings comes closest to being a testament. Looking back on the 19th century—for that century was the prologue to *his* time and was the last century whose art he approved—Mumford found:

> Ranged alongside Rodin were the many mighty spirits of the nineteenth century—scientists no less than artists—who were equally exalted by a fresh vision of life. … If all the technical triumphs since 1815 had been wiped out the instant they occurred, the exuberant creativity of the Western mind would still have made this one of the most notable moments in human culture.[258]

Science and art flourishing and enduring, while technology is obliterated. That, for Mumford, was ideal.

Germany, because of its romantic tradition, presents a special case—a case that remains special even disregarding the National Socialist period as extra-special. On the one hand, romantics denigrated physical science because of its close connection with technology, denigrated it as inseparable and indistinguishable from technology. Of this the later Heidegger is the most pertinent example.[259] On the other hand, German romanticism was always a *Lebensphilosophie*, a grounding of knowledge and value in 'life'—'life' as the fundamental, irreducible, not-further-explicable, ground of the good.[260] To the extent that purposiveness was presupposed in this appeal to 'life' as grounding, *Lebensphilosophie* was a form of pragmatism. As such, German thought from the early 19th century to the late 20th century should, logically, have attributed primacy to technology. Nonetheless, as we saw with Marx and with Sombart (and with Veblen too, whose outlook was close to theirs), even the most principled proponents of the primacy of practice were prevented from being so logical by equally fundamental prejudices regarding the moral and cultural value of disinterested understanding.

Thus little originality or independence of mind was required to proceed from the romantic animus against technology and physical science, if joined with a deep cultural pessimism, as it usually was in Germany, to a 'discovery', such as Heidegger's, of the primacy of technology.[261] What required originality—or, more than originality: radical culture change—was to break sufficiently with the romantic devaluation of technology, on the one side, and with the exaltation of the idea/ideal on the other side, in order then to be able to view technology without disdain and to view science without animosity as technology. No such un-antagonistic, non-punitive categorization of science as technology was ever advanced prior to postmodernity.[262] However, on the threshold of postmodernity, in the wake of the 1960s revolt against value-free science—which naturally found an especially strong resonance in Germany due to the romantic-*lebensphilosophisch* coloring of almost all German thought[263]—there appeared contentions regarding the real or required relations between science and technology that in some manner or degree assert the primacy of technology. It is then to sharpen the distinction between the modern and the postmodern views of the relation between science and technology, and to justify my taking the difference between those viewpoints as demarcation criterion between modernity and postmodernity, that I consider the positions developed on this issue in the 1970s by the Erlangen school and by the Starnberg school.

In 1962 the metamathematician–logician Paul Lorenzen, age 47, was appointed to a chair of philosophy at Erlangen University, joining there his friend, the philosopher–theologian Wilhelm Kamlah.[264] Their principal collaboration and the work that drew wide attention to 'the Erlangen school', was the anti-Heideggerian *Logical Propaedeutic: Pre-School for Talking Sense* (1967), which in 20 years sold some 50,000 copies.[265] I surmise, however, that the book was written almost entirely by Kamlah, as Lorenzen, far from wishing to defame Heidegger, had much sympathy for at least the early

Heidegger's *lebensphilosophisch* pragmatism. Thus Lorenzen, in his principal programmatic essay, published in both English and German in 1965, quoted Dilthey's

> … remarkable statement: 'Knowledge cannot go behind Life'. (Hinter das Leben kann die Erkenntnis nicht zurückgehen.) … as a formula in which a fundamental change in the direction of philosophy is contained, beginning in the nineteenth century and slowly prevailing in the present time.[266]

Lorenzen then pointed to Heidegger (and Georg Misch) as having 'made clear what it means that thought must begin with life'.[267] Lorenzen was, namely, a metamathematician in the intuitionist direction and his program as philosopher was to carry the transcendental pragmatism underlying intuitionist constructivism over to natural science. Through the construction of a 'protophysics' of length, mass and time, on the basis of elementary physical operations, secure and indubitable foundations for natural science would be created in a manner analogous to the intuitionists' founding of mathematics wholly and uniquely upon elementary numerical operations.[268] Not yet in 1965, but eventually, Lorenzen would describe those fundamental physical operations as 'technical'.[269]

It is one further illustration of how close together, epistemologically, *Lebensphilosophie* brought the political left and the political right in Germany, that leftist Lorenzen credited Hugo Dingler, a radically rightist philosopher of physics of the previous generation, with originating this foundationalist program and adopted Dingler's label 'methodical'. Beginning with the edition of Dingler's unpublished 'Aufbau der exakten Fundamentalwissenschaften' (1943), Lorenzen devoted much effort to the resuscitation of Dingler's reputation as epistemologist.[270] Down to the present day tributes to Dingler have continued to be a distinguishing mark of the work of the Erlangen school and its prolongation at Constance University.

The principal elaborator and standard-bearer of Lorenzen's 'protophysics' program, and curator of Dingler's legacy, was Lorenzen's student, Peter Janich (PhD 1969). Advancing rapidly to a professorship at Constance University (1973), Janich, a product of the 1960s revolt against value-free science, urged in his inaugural lecture the desirability of a reformation of physics through which, 'in place of the musty ideology of the researcher who unravels nature's secrets, the physicist will understand himself to have just one task: *enabling technology*'.[271] From that typical late-1960s new left 'ought'—an 'ought' that remains modernist in its presupposition of technology's dependence upon physics—Janich advanced five years later to a seemingly postmodern epistemic 'is':

> doing experiments is more an activity to produce *technical effects*, which can be described appropriately as engineering rather than as a scientific activity, properly speaking, as a construction of machines rather than as an inquiry into nature, as an attempt to produce artificial processes or states rather than as a search for true sentences.[272]

Then two pages later, from that epistemic 'is' to a seemingly postmodern 'ought':

> Consequently, the relation between natural science and technology will have to be defined in a new way. Natural science is to be understood as a secondary consequence of technology rather than technology as an application of natural science.[273]

Janich's epistemic 'is' parallels that of Felix Auerbach, whose 1923 book he probably knew.[274] In that first quoted passage Janich denied experimental science the dignity of true, truth-seeking science, demoting it to the level of engineering. Janich, however, reveals through his rhetoric that, unlike Auerbach, he is not merely restating but actually sympathizing with this long-familiar romantic canard directed against those sciences not content to contemplate Nature but desiring rather to master Her. Thus between that 'is' statement and the following 'ought' statement there is a difference of worldview: not only has Janich dropped the distinction between observational and experimental sciences on which he had built his case, he has also reversed the difference in rank between science and technology.

Though his arguments lacked logic, Janich did unquestionably deny science's primacy to and for technology, indeed reversed primacy as between science and technology, and did so at a date, 1978, that must be regarded as not yet over the threshold to postmodernity.

In this radical form, Janich's 1978 claim remained an anomaly. So far as I have seen, neither Lorenzen nor any other member of the Erlangen school made a science-as-technology claim that went nearly so far.[275] True, in the late 1970s and early 1980s *Technik* was a key word for them, but it remained nothing more than that: a key word to which no content or thesis was attached.[276] Janich was a prolific writer, but so far as I have seen he made the statements I have quoted only that once—and that once was only in English. Far from this being his entrance into postmodernity, by the late 1980s Janich had quite backed away from this contention.[277] When it appeared again in his writings in the late 1990s it was no longer a demotion of science to the level of technology, but the elevation of technology as model for *all* higher cultural constructions, with science one among the others.[278] In this later form the asserted primacy of technology is quite in the spirit of postmodernity: pragmatic, pluralist, and free of the romantic animosity and punitive intent that are so evident in Janich's contentions two decades earlier.

With an institutional prominence and intellectual–political controversiality far greater than any product of the Erlangen school was the 'finalization theory' proposed by the Starnberg school. This 'process through which external goals for science become the guide-lines of the development of scientific theory itself' was the conception of the task-group on 'Alternatives in Science' at the Max Planck Institute to Explore the Life-Conditions of the Scientific-technical World.[279] That institute had been brought into existence on January 1, 1970, to enable Carl Friedrich von Weizsäcker to pursue on a larger scale the analyses, initially of nuclear weapons policies, that had engaged him strongly ever since he put together the 1957 'Göttingen Declaration' of German atomic scientists foreswearing work on nuclear weapons. The institute's title reflected the fact that von Weizsäcker, leaning out of his chair as professor of philosophy at Hamburg University, had been overseeing such extra-university analytical work, with extensions to environmental preservation policies and third-world development policies, quite in accord with the general extension of interest in the 'implications of science' among scientists throughout Europe and America in the course of the 1960s.[280]

Situated in the lovely town of Starnberg, on the large and largely unspoiled Starnberg Lake, 30 minutes on the S-train south of Munich, the institute afforded von Weizsäcker and collaborators a sufficiently distanced view of the scientific–technical world, yet ready access to the Munich headquarters of the MPG: the creation of the institute had been largely on the initiative of its Executive Director and its President, who looked to von Weizsäcker for advice on science policy and hoped that he would develop a solid scientific basis for government policies supportive of the MPG. As von Weizsäcker himself had relatively little interest in *science* policy and as he held no reins on his collaborators, his 'Alternatives in Science' task-group on science policy, made up largely of left-leaning students and assistants brought along from his Hamburg philosophical seminar, turned from practical science policy questions to world-historical theorizing with the intention of providing a foundation for the 1960s demand for 'relevance' in scientific research.[281]

Quite in contrast with the home-grown hermeneutical tradition that inspired the Erlangen school, the principal inspiration of the 'Alternatives' enterprise came from their discovery of the writings of Edgar Zilsel on the social origins of modern science, and, still more, from the recent, Kuhn-dominated, Western historical–philosophical literature theorizing scientific development. Thus while the Starnberg group recognized in the Erlangen school its most pertinent, and largely similarly intentioned discussion partners, and shared the Erlangen animus against discipline-directed, for-its-own-sake science, the Starnberger emphatically rejected the Erlanger's demotion of science to technology (and, likewise, the Erlanger's 'Dingler-Komplex').[282] Gernot Böhme, the Starnberger's leading spokesman, insisted that:

> Yes, the protophysicists maintain that the rules of thumb of the handworker reappear to a certain extent in science in a 'highly stylized' form. … [However] the high stylization that transforms manual rules into regulative ones giving a compulsory form to scientific behavior consists namely in this: that the determinative objects or conditions of the manual craft are transgressed, becoming ideals, and consequently the craft rules become criticizable and correctable on the basis of those ideals.[283]

Thus the Starnberger, no matter that they devoted themselves to Zilsel-inspired essays on the origins of pre-paradigmatic science in craft techniques and practices, had no doubt that science, being science, necessarily transcended its technological origins; that the creation of a science, any science, involves 'detaching the scientific experience from its prescientific–technical origins'.[284]

Far from challenging the epistemic presuppositions of modern disciplinary scientific realism, the Starnberger saw the world as comprising just so many natural kinds, to each of which there corresponded a science, and to every science a three-stage process of development. That is, to Kuhn's pre-paradigmatic and paradigmatic stages in the development of every science they added a third stage of post-paradigmatic or 'mature' science. The concept and warrant for this final stage they drew, ironically, from the overreaching claims of the theoretical physicists of that day, specifically the notion of 'a closed-off theory' promoted by Heisenberg: 'Closed-off theories are valid for all time'.[285] Thus the Starnberger, using Heisenberg against Kuhn, had the paradigmatic stage of scientific development end with the attainment of the 'closed-off

theory' specific to that science. With its intrinsic truth realized, there remained no future for the now mature science except the acceptance of its 'finalization', that is, the determination of its future course of development by extrinsic, socially defined ends in place of the internal logic of disciplinary development characteristic of the paradigmatic stage:

> The finalization thesis postulates that in the course of its development a science (discipline, or field of knowledge) reaches a state of maturity as a result of which it can be said to have completed its work. The fundamental problems of a discipline are resolved when a general theory for its subject matter is formulated.[286]

Indeed,

> we assume 'theoretical maturity' to be the property by which the integration of external goals into the theoretical research program of a field is made possible and is required.[287]

Thus,

> the paradigmatic phase of scientific development, often presented as the 'Golden Age' of science, must eventually come to a close in all disciplines. This does not, however, mean the termination of theoretical developments within those fields; rather, it signifies the beginning of the possibility of theoretical development in pursuit of social goals.[288]

—in other words, reorientation of research from disciplinarily defined goals to technologically defined goals. In this final stage of development of a science, any science, every science, 'scientific advance becomes goal-oriented and technologies are planned in conformity with theories'—or should be.[289] Thus, far from intending to suggest an end of science as such in creating the term 'finalization' for that post-paradigmatic stage of reconvergence upon technology, the Starnberger conceived that a science not only retained its elevated position relative to technology, but greatly increased its leadership role in relation to technological innovation—and did so quite specifically through the predictive power of its 'closed' theoretical apparatus.[290]

That there is in the theory of finalization an array of unreflectively modernist preconceptions—among which the primacy of science to and for technology is only the most pertinent—becomes especially clear in the proposals made in the late 1990s for the revival of that theory by two of its original proponents.[291] In the face of all the evidence of postmodernity, Krohn and van den Daele were still insisting on the internal–external distinction and on the autonomy of science when scientists are being scientists. 'We believe it'—the internal–external distinction—

> all the more important and necessary in order to account adequately for both the involvement of scientists in the networks of innovation, on the one hand, and the functional differences that remain between doing research and making money.[292]

Krohn and van den Daele are perhaps even more out of touch with culture and scholarship in postmodernity in continuing to conceive that 'science offers its mode of operation' as a model for modernizing societies to emulate: 'social and political modernization assimilate the modes of scientific operation'.[293] The notion that science, as institution and as a mode of operation, embodies in the highest degree the virtues of liberal, democratic, progressive societies, and hence any society aspiring to

such virtues ought to take science as their model, was indeed widely accepted in the middle decades of the 20th century. It disappeared, however, and rather abruptly, at just about the time that the Starnberg institute was shut down, on 30 June 1980.[294]

The 1970s Were Still Modern: Bell and Lyotard

Examination of the Erlangen and Starnberg schools has shown that even in Heidegger's homeland the 1970s remained on the whole very modern as regards the available conceptions of the science–technology relation. An anti-modern, romantic view of that relation was possible, but a postmodern view was not. It is, then, all the less surprising that to the West, in France and in the USA, through the 1970s the primacy of science to and for technology remained firmly in place in the minds of even the most forward looking. As exemplary of this persistence of modernity, right up to the big break *ca* 1980, I offer the two principal late-modern conceptors of the future of the economy–society–culture complex, Daniel Bell, and Jean-François Lyotard.

Daniel Bell was the broadest, brightest, most imaginative and insightful sociologist analyzing culture and society in the USA in the 1960s and 1970s. He was also the most widely known and widely influential—worldwide.[295] His 500-page *The Coming of Post-Industrial Society: A Venture in Social Forecasting* (1973) was the fruit of a dozen years of active reflection on the likely characteristics of an anticipated transformation of the conditions of production and of social life, in the USA in the first instance and eventually in the world at large. The venues for his forecasting efforts were chairs of sociology—at Columbia University (1959–69) and then at Harvard—the National Commission on Technology, Automation, and Economic Progress (1964–6), and, more especially, the Commission on the Year 2000, which he founded in 1964 under the auspices of the American Academy of Arts and Sciences (Cambridge, MA).[296]

Those transformed conditions of production and of social life that Bell saw becoming established in America and Western Europe, and that he projected as becoming fully realized in the course of the next three decades, constituted what Bell called 'post-industrial society'.[297] Without any of Mumford's ambivalence, Bell saw historical stages as technological phases.[298] Nonetheless, for Bell, just as for Mumford, technology was always only an epiphenomenon, a by-product of the codification of theory. Thus, at the core of Bell's anticipations of the coming post-industrial society is the primacy of science—and more generally, the primacy of 'theoretical knowledge': as the 'axial principle' of post-industrial society Bell postulated 'the centrality of theoretical knowledge as the source of innovation and policy formulation for the society'.[299]

Bell's implicit reliance upon his American Academy colleagues for his examples of 'the centrality of theoretical knowledge' as 'the basis of innovations in technology' appears in categorical assertions regarding physical–technical connections:

> The computer would not exist without the work in solid-state physics initiated forty years ago by Felix Bloch. The laser came directly out of I. I. Rabi's research thirty years ago on molecular optical beams.[300]

Bell's acceptance and exhibition of such exaggerated claims—claims typical of the self-understanding and self-representation of physicists in the 1960s, claims whose

scientific substance Bell himself only half understood—is indicative of how fully Bell shared the physicists's presumption of the primacy of their science.[301]

Behind and beneath Bell's postulation of the primacy of theoretical knowledge, forming the bedrock of his imaginings, was an ideology: modernist, meritocratic, mandarinism—a Deweyanism minus the faith in democracy.[302] To this ideology Bell gave voice toward the end of his big book in answer to its most basic question (one might say its 'axial' question): 'Who Will Rule?'

> If the dominant figures of the past hundred years have been the entrepreneur, the businessman, and the industrial executive, the 'new men' are the scientists, the mathematicians, the economists, and the engineers of the new intellectual technology. … In the post-industrial society, production and business decisions will be subordinated to, or will derive from, other forces in society; … they will be based upon the government's sponsorship of research and development, … . The husbanding of talent and the spread of educational and intellectual institutions will become a prime concern of the society; not only the best talents but eventually the entire complex of prestige and status will be rooted in the intellectual and scientific communities.[303]

Bell's 'venture in social forecasting', being in this regard so largely the wishful thinking of a modernist intellectual, appears now, from postmodernity, sheerest fantasy. Well before the year 2000 it was clear that in anticipating continued growth of the power and prestige of scientists, and the triumph of meritocracy more generally, Bell's forecast was dead wrong.[304]

For the 1999 reissue of *The Coming of Post-Industrial Society*, Bell provided a lengthy foreword. Where previously the primacy of theoretical knowledge had been his 'axial principle', Bell titled this updating of his thesis 'The Axial Age of Technology'. Bell did not consciously intend that title phrase to be an acknowledgement that he had been wrong in the 1970s in subordinating technology so largely and fundamentally to science. Blind to his past blindness, Bell's adoption of that title is another instance of our unreflectively falling in with postmodernity's presuppositions, another example of our erasure in postmodernity of all memory of modernity's presuppositions and of our earlier attachment to them.[305]

Bell was already well known in France when *The Coming of Post-Industrial Society* appeared in 1973. He had resided for a year in Paris as director of international seminars for the Congress for Cultural Freedom, 1956–7, and much of what he gathered into *The End of Ideology* (1960) had first been presented at conferences and seminars in Europe.[306] His new book drew even wider attention there than the earlier one and, notwithstanding its great length, a French translation appeared in 1976.[307] Bell was thus a thinker of first importance for Jean-François Lyotard as he approached his commission from the Quebec government to prepare a report on 'the condition of knowledge in the most highly developed societies', published in 1979 as *La condition postmoderne*.[308] That there is a great deal of Bell's conception of post-industrial society in Lyotard's conceptualization of the postmodern condition alerts us to just how modern this principal promulgator of postmodernity and of postmodernism remained.[309]

Lyotard shared with Bell the pre-postmodern belief in the primacy of ideas, including the centrality of the intellectual and social legitimation of ideas, and the intrinsic importance of those making and shaking ideas. Moreover, Lyotard, exceptionally among the postmodernists, followed Bell in giving cultural primacy to science and special importance to scientists. Thus the main theme and thesis of *La condition postmoderne* is the emergent 'crisis of narratives', a growing disbelief in 'grand narratives', i.e. in those comprehensive, 'universal' social, cultural, and cognitive goals whose goodness, rightness, and inevitability we conveyed to ourselves through narratives of their progressive realization. Science is centrally involved in this crisis because the grand narrative, the characteristically modern mode of legitimation, is itself legitimated so largely by a narrative of legitimation about science, a narrative then being discredited through widespread acceptance of anti-progressive conceptions of science.[310]

Yet for Lyotard himself the incredulousness towards grand narratives that demarcates postmodernity from modernity did not deprive science of its primacy. On the contrary, as Fredric Jameson wrote in his foreword to the English translation, Lyotard

> transfer[s] the older ideologies of aesthetic high modernism, the celebration of its revolutionary power, to science and scientific research proper. Now it is the latter's infinite capacity for innovation, change, break, renewal, which will infuse the otherwise repressive system with the disalienating excitement of the new … .[311]

For all that the still half-modern Lyotard, much like the late Mumford, continued to look to science for mould-breaking novelty, for the truly revolutionary, the postmodern Lyotard recognized that the failure of credence in grand narratives, in collective goals, and in transcendent ideals implied a far-reaching pragmatic utilitarianism. More than that, the postmodern Lyotard recognized that such utilitarianism brings with it a fundamental revaluation of science, of technology and of their relationship.

Lyotard himself acted out this revaluation through that now familiar line of fallacious reasoning subsuming science into technology: because experiment decides scientific issues, and experiments depend on technology, technology is prior to and inclusive of science.[312] Most probably, Lyotard, like most others making use of this argument since the late 1970s, had it from Heidegger—in Lyotard's case, most probably quite directly.[313] For Lyotard, however, the function and application of this argument was different: not metaphysical, but historical. Rather than being a romantic put-down of all modern science, from the 17th century forward—as it was for Heidegger—for Lyotard it served to distinguish the postmodern from the modern era: now, only now, in postmodernity, 'The relationship between science and technology is reversed'. ('Le rapport de la science et de la technique s'inverse.'[314]) Lyotard did not elaborate on this thesis—nor could he, while placing his hopes for a 'saving power' in science—but his intuition was very good, his statement prescient. As the 1970s ended, Lyotard was becoming distinctly postmodern—and so also, unconsciously, was all the Western world.

III. The Primacy of Ideology among Historians of Technology

> *SHOT is a tightly knit organization, and, among its members,*
> *the distinction between outsiders and insiders remains highly salient.*[315]

It is, I have argued, a defining characteristic of postmodernity that technology's putative role in the production of culture has risen enormously above and beyond that which modernity ascribed to it. Technology has now replaced science not only as the principal model for knowledge production, but has also replaced science as principal model for all those 'ordering' activities that constitute culture. Thus, as Lyotard presciently said, in postmodernity the relation between science and technology has reversed: technology has now not only the cultural primacy that science had enjoyed in modernity, but technology has also assumed the leadership role in the science–technology relationship. Whereas in modernity science received most of the credit (and blame) for significant technologic change, and scientists were regarded as the most competent anticipators of the technologic future, in postmodernity science is reduced to technology's servitor, while scientific advance is itself conceived to be, hence made to be, primarily the result of the application of new technology.

Among the first to recognize this epoch-making change in the cultural significance and valuation of technology should be the historians of technology. They should have the historical perspective enabling this recognition, and, one might think, their self-interest should support this recognition. While philosophers of technology, generally taking Heidegger's writings as their prophetic texts, have been claiming prime importance for technology—and, as is their wont, absolutizing, not historicizing, this postmodern revaluation of the science–technology relation—historians of technology, while wisely avoiding Heidegger, have also largely ignored this epochal elevation of technology, or, if noticing it, have generally deplored it and dismissed it as a failing of the popular mind.[316]

Such unwillingness to recognize or accept an epochal cultural reorientation that affects so favorably the general regard of their subject, technology, and hence, potentially, of their discipline, the history of technology, is contrary to expectation and demands explanation. The explanation lies, as it seems to me, in the peculiarly ideological character of that historical discipline and in the specific ideology adopted by it, as it took its present shape between the late 1950s and the early 1980s. Ambivalence toward technology, verging on antagonism, was characteristic of most of that discipline's founding figures, leading to a consensus that the mission of the discipline was to keep technology down. At the same time, the predictable resentment over inferiority to science and to the history of science that modernity imposed upon their subject and their discipline was dealt with through a consensus among historians of technology to keep science out—and, where it could not be kept out, to put *it* down. Ideologically set against recognizing that in postmodernity technology has 'gotten up', way up, in cultural rank and in cultural role, and similarly set against allowing science to figure in *their* history of technology—or, if it must figure in some way, then as villain—the historians of technology have rendered themselves effectively incapable of recognizing the reversal in recent decades of the rank and role relations between science and technology.[317]

Ever since its institutionalization at the end of the 1950s, the US division of the history of technology discipline has attributed enormous importance to *Technics and*

Civilization and to its author, Mumford.[318] Kranzberg, the principal entrepreneur of that disciplinary formation, considered Mumford's work as a model of the 'integrative' scholarship that his projected disciplinary association and journal should strive toward.[319] Three of the eight the articles that Kranzberg assembled for his first number of *Technology and Culture* referred to *Technics and Civilization*, two of them offering highly complimentary notices of it in their texts.[320] Over the next 6 years Kranzberg published some five pieces by Mumford in *Technology and Culture*, partly by printing what Mumford had already published elsewhere, had promised for publication elsewhere, or probably could not have published elsewhere.[321] Kranzberg was by no means alone among the founding figures in his admiration for Mumford. Hughes has been an even greater Mumfordolater, finding Mumford 'arguably America's pre-eminent twentieth-century public intellectual', taking Mumford as 'mentor', and taking as his own goal the thoroughly Mumfordian endeavor 'to change our way of thinking about technological and scientific change from the mechanical and analytical to the systematic, organic and holistic'.[322] Moreover, the high rate of citation of Mumford in articles and reviews in *Technology and Culture* from its earliest to its most recent volumes testifies to the sustained esteem for Mumford among historians of technology.[323] Thus Mindell, opening *Between Human and Machine* (2002) with several pages on Mumford, explained that, 'I begin this history of control systems with *Technics and Civilization* because historians of technology consider it a foundational text'.[324]

Mindell's senior colleague at MIT, Williams, at the time of this writing president of the Society for the History of Technology (SHOT), sought in 1990 to articulate just why Mumford was so important to her field:

> What is so untraditional … is the way Mumford 'enlarges the canon of culture' to include technology. … In Mumford's own words, to see technics as 'an integral part of higher civilization' represents 'a shift in the whole point of view'. This is his fundamental and lasting contribution.[325]

Taking on this task again in 2002, Williams then found, rather, that:

> His singular innovation and enduring contribution is his insistence on technics as an expression of human personality.[326]

Each of these assertions of an innovative and enduring elevation and humanization of technology sounds a bit forced, and, when the two are juxtaposed, even a bit desperate.[327] This is not surprising when we bear in mind just how little support for such an elevation of technology turned up in our examination of *Technics and Civilization*—and still less in Mumford's earlier and later writings. Surprising is, rather, that Williams and Hughes, and all those other historians of technology praising Mumford for his signal contributions to the elevation of the subject of their study, could have read Mumford in that way.

Indicative in this regard is Molella's construction of 'Mumford's enthusiasm for technology', of a Mumford 'fascinated by technology since childhood'. According to Molella,

> Mumford's interest in the history of technology originated in part in his fascination with machines … . His youthful interest in technical devices, technical processes, and industrial practices endured, ultimately informing his masterwork.[328]

To see Mumford so, it is necessary to ignore Mumford's almost invariably pejorative references to every form of enthusiasm for, or fascination by, technology—and that disdain is especially evident in Mumford's references to his own youthful interest in a career in electrical engineering.[329] Similarly, one must ignore the clear and constant message of every one of Mumford's five pieces in those early volumes of *Technology and Culture*: 'because our own society is in fact over-determined by its excessive and almost exclusive preoccupation with technology', and because 'as a consequence we tend to read our own assumptions and habits into the behavior of other societies', all histories of early man and early civilizations have mistakenly emphasized tools rather than ideas.[330]

Ignoring Mumford's derogations of technology has a curious correlative, viz. ignoring Mumford's emphasis on the importance of science and scientists for technological advance over the preceding four centuries, indeed over the preceding six millennia.[331] Molella, after doing so through the course of his 1989 and 1990 essays on Mumford just quoted, finally acknowledged on his penultimate page Mumford's attribution of primacy to science for technology throughout history. Molella adduced this discreditable fact only in order to explain why Mumford held to the, obviously false, belief 'that technology *itself* advanced':

> Accepting the conventional wisdom, he [Mumford] saw technology essentially as the product of prior scientific discovery working itself through successive levels of practice, a process of discovery and development that had gone on throughout history. Such beliefs, at least at the writing of *Technics and Civilization*, left him basically receptive to technological advance and optimistic about the future.[332]

To anyone not initiated into the true beliefs of the historians of technology the quoted sentences are perplexing. To initiates, as we shall see, there is no uncertainty about the intellectual, even characterological, failing that Molella means to be pointing out in writing of Mumford, 'left him basically receptive to technological advance and optimistic about the future'. Nor is there any doubt about the direct and necessary connection between regarding 'technology essentially as the product of prior scientific discovery' and the belief 'that technology *itself* advanced': because it is impossible for any modern to deny that science advances, 'the conventional wisdom' that technological advance results from the advance of science must be rejected if historians of technology are to be free to deny that technology does in fact advance.

Consider Williams's handling of this same matter of the significance attributed by Mumford to science. In Williams's 2002 essay on *Technics and Civilization* there is, in truth, nothing to consider: the word 'science' never appears.[333] She manages to describe Mumford's conception of the neotechnic without any reference to science. Though she quotes and affirms Mumford as having presented technics as 'an integral part of higher civilization', she explicates this not by referring to any higher elements of civilization but to 'social and physical realities'. When she comes to Mumford's 1930 essay, 'The Drama of the Machines', she refers only to 'miners, monks, soldiers, financiers', notwithstanding that, as was evident in the long quotation from that essay given

above, Mumford put scientists forward as the primary actuators of this drama, as well as the most prominent actors in his portrayal of it.[334]

In her 1990 essay Williams had a more extended exposition of this drama, and with it her antagonism toward scientists found more overt expression. Mumford, as we saw, used a military metaphor for machine civilization's conquest of the West, describing 'the physicists and mathematicians' as 'a group of creative minds, a general staff, who would see a dozen moves beyond the immediate strategy and would invent a superior tactics'. Paraphrasing Mumford, Williams used a metaphor that inverted Mumford' affirmative valences and rendered the scientists' role sinister, while also diminishing its distinctiveness.

> Mumford suggests that industrialization is a sort of conspiracy, a takeover by hostile forces within, a revolutionary coup. Even more precisely, he implies a Leninist theory of revolution. The footsoldiers are workers …; they are directed by a 'general staff', a party of ideologists (physical scientists and the like) 'who would see a dozen moves beyond the immediate strategy and would invent a superior tactics'.[335]

In Williams's, as in Molella's, 'handling' of Mumford we are encountering the antagonism toward science and the ambivalence toward technology characteristic of those professing our contemporary history of technology orthodoxy—which antagonism and ambivalence have rendered the orthodox unprepared to recognize the reversal of primacy of between science and technology in postmodernity, and have, more generally, rendered them unprepared and unwilling to recognize the epochal cultural shift from modernity to postmodernity. Thus alerted to the ideologies and strategies that have disabled that recognition, let us consider them more generally and genetically.

With the same purely symbolic concreteness characteristic of other creation myths, the source of the animus among historians of technology against science and the history of science is identified in the creation myth of the American division of the history of technology discipline. The story of the origin of their clan that every American historian of technology knows and the great majority still believe, is set as epigraph to the first chapter of the book about the intellectual constitution of their discipline that every American historian of technology reads.[336] In 1957, as that story goes, a small group from among the still few practitioners of the history of technology in the USA approached the History of Science Society, through the person of its then president, Henry Guerlac, with a request that the history of technology be accorded a place within that society, on the program of its meetings, and in the pages of its journal, *Isis*. Receiving the group at his home on a hill above the Cornell University campus, Guerlac, the highly cultured historian of French chemistry, rebuffed them: the History of Science Society would not 'condescend' to include the history of technology, and '*Isis* is not going to publish any articles dealing with it'. Walking back down the hill, that group, led by Melvin Kranzberg, resolved there and then to create a society, a journal, and a discipline of their own.[337]

This story continues to be generally believed and often repeated, even though historical research by Seely, published more than 10 years ago in *Technology and Culture*, has shown it to be exactly the opposite of the truth.[338] Seely found that officers of

the History of Science Society had made overtures to the historians of technology then being assembled by Kranzberg under the auspices of the American Society for Engineering Education. Offering increased emphasis upon the history of technology in the meetings of the History of Science Society and in the pages of *Isis*, Guerlac conveyed the society's enthusiastic interest in cooperation. But Kranzberg, ambitious, entrepreneurial and confident that there were large constituencies with only marginal interests in history that would be recruitable to a history of technology banner, already had firmly in mind to create a society and a journal of his own.[339]

Inversion of circumstances seems to be the rule in the formation of disciplinary creation myths, but rarely is the psychodynamics of the inversion—'they rejected us first'—so transparent. Yet such inversions can arise and take hold only at some distance from the events requiring to be misremembered. Thus although, as Seely observed, 'One way to distinguish between the history of science and the history of technology was to distinguish science from technology', prior to the late 1960s that task of differentiation seems to have been pursued without much evident animosity toward science and the history of science.[340] Staudenmaier, in his systematic examination of the material appearing in *Technology and Culture*, 1959–1980, found that 'No theme, in SHOT's early years, seemed as well focused, as important, or as interesting as the relationship between science and technology'.[341] Not until the late 1960s, fully a decade into the life of their new discipline, as it appears from my limited reading, did the historians of technology begin to express antagonism toward science—at first while still deeply engaged with the conceptual problem of relating technology to science.[342] Only a decade later, roughly about 1980, did historians of technology turn their backs on that problem and, simply cutting the knot, declare the divorce of technology from science. Therewith the disciplinary creation myth, with its 'they rejected us' thrust, acquired greater importance and currency.

This change in outlook—from an unquestioning affirmation of the fact and the desirability of technology being imbued with and inspired by science, to an antagonistic attitude toward science (and historians of science) in conjunction with an endeavor to establish disciplinary autonomy for the history of technology—is traceable especially clearly in Layton's writings.[343] Contrary to the reading that historians of technology today incline to give *The Revolt of the Engineers*, Layton does not there contemplate a revolt against subordination to science. Published in 1971, but begun almost two decades earlier as doctoral dissertation, the book opens, page one, line one, with a flat statement of fact: 'The engineer is both a scientist and a business man'.[344] The revolt that Layton there had in mind—the revolt that Layton found, and favored, among American mechanical engineers in the first decades of the 20th century—is of the engineer as scientist against his pecuniarily interested alter ego, the business man. Here in this book—which when it finally appeared no longer fully represented its author's views in this regard—Layton, whose original orientation was emphatically social-scientific, was still full of admiration for science, was in favor of whatever of science could be found in the engineer, and fully in favor of the engineer thinking of himself as 'an applied scientist':

> The cement binding the engineer to his profession was scientific knowledge. All of the themes leading toward a closer identification of the engineer with his profession rested on the assumption that the engineer was an applied scientist. It was the cumulative character of scientific knowledge that gave weight to engineers' claims to be the agents of progress and enlightenment. Similarly, the self-image involved transferring to the group attributes of science such as logic and impartiality.[345]

Without a suggestion of ideological distance from the scientizing of that era, Layton was at once describing and affirming the perspective of the progressive engineers whose cause Layton made his own.

While the book was still in press, Layton was writing 'Mirror-Image Twins: The Communities of Science and Technology in 19th Century America', presented in 1970 at an important conference on science in 19th century America, and published in *Technology and Culture* in 1971.[346] In that paper Layton remained, fundamentally, the admirer of science that he had always been.[347] Overlaid upon adherence to the traditional conception of science's moral superiority to engineering was Layton's insistence that the 'technological sciences', or 'engineering sciences'—created in the course of the 19th century by what 'might be termed "the scientific revolution in technology"'—were to be regarded as *the* essentially autonomous and exclusive producers of new technology in the 20th century.[348] Withal Layton's exposition remained temperate; there were no villains; no one had yet denied that engineers have minds and think. The farthest Layton there went was to complain that:

> The significance, indeed the very existence, of the scientific revolution in technology has been obscured by a commonly accepted model of the relationships between science and technology. In essence, this holds that science creates new knowledge which technologists then apply.[349]

As exemplary articulation of this commonly accepted model, Layton adduced, in an entirely neutral way, that passage in *Science, The Endless Frontier* in which it is asserted that 'New products and new processes … are founded on … research in the purest realms of science'. No antagonism was yet evident in the exposition.[350]

Moving on three years to Layton's 'Technology as Knowledge', appearing in *Technology and Culture* in 1974, we find again this same quotation from *Science, The Endless Frontier*.[351] Now, however, Layton is on the warpath; now the quoted assertion provokes an outburst—principally, however unfairly, against historians of science:

> Clearly, if basic science is the source of all new technical knowledge, then technology itself produces no new knowledge, and the technologist's role becomes that of applying knowledge generated elsewhere. And this is precisely the theory we find in Singer, Holmyard, and Hall's *History of Technology*. Indeed, it was through the work of Hall and certain other historians of science that this theory was introduced into the writing of the history of technology.[352]

Two years later still we come in *Technology and Culture* to Layton's 'American Ideologies of Science and Engineering'. There he exhibited once again this same quotation from *Science, The Endless Frontier*, continuing it to twice its previous length to include text whose moderated claims of science's primacy ought to have been somewhat mollifying.[353] But Layton was unappeasable. His summary of the entire quotation

was that, 'In short, the scientific ideology interprets a symbiotic relationship as a case of intellectual parasitism'.[354]

There was something oddly retrograde in Layton's ideological development in the early 1970s, turning away from the issue of social responsibility just when his milieu, and the engineers too, were turning toward it; advocating an epistemic conception of technology just when history of science was turning away from the intellectual to the social, and well after history generally had made that turn.[355] The logic lay of course in the issue of autonomy for the history of technology, for in modernity all (science-like) knowledge was entitled to autonomy. By much the same implicit reasoning that had shaped the history of science in previous decades—but by then no longer—there could be no autonomy for the history of technology without autonomy for technology. Thus Layton was far from alone in conceiving emancipation of the historians of technology in cognitive terms. In *Technology's Storytellers* (1985) Staudenmaier adopted and extended Layton's analysis, taking it that big step farther to the conclusion of a divorce of technology, and of its history, from science. He reasoned as follows: the popularity of the theme of the science–technology relationship among historians of technology contributing to *Technology and Culture*

> … is due to their concern to establish the irreducibly distinct nature of technological knowledge against a claim that modern technology is nothing more than an application of scientific knowledge … .

For:

> the claim that science is the only objectively valid form of knowledge leaves technology 'mindless', bereft of its own intellectual method.

However:

> … if technological knowledge is irreducibly distinct in its own right, then science cannot claim the role as technology's sole source of knowledge … .[356]

From which Staudenmaier, moved by something more than logic, concluded 'that the science–technology relationship is an inadequate frame of reference', indeed an 'inappropriate frame of reference'.[357] Behind this non-sequitur was an emerging consensus: the science–technology relationship was 'a thematic dead end in our field', was a problem better ignored than addressed.[358] Thus if, as Seely said, 'One way to distinguish between the history of science and the history of technology was to distinguish science from technology', another way was intentionally and systematically to ignore science in writing the history of technology.[359] Following Staudenmaier, historians of technology generally, and as a corporate body, have taken that latter course—at the cost of misrepresenting the mentality of modern generations past and of failing to apprehend the mentality of our present postmodern generation.[360]

Yet if there is any respect in which the historians of technology are today definitely postmodern in their assumptions and their practices, it is in their abandonment of that earlier program of fashioning the history of technology around the conception of technology as a specific sort of knowledge, i.e. in their ceasing to seek an epistemic

basis for the asserted autonomy of technology, and hence their autonomy as historians of technology.[361] Nonetheless, their policy of ignoration of science, which policy they originally rationalized by an allegedly distinctive cognitive essence of technology, remains very much in place today.[362] One ground for this continued ignoration of science and the history of science is the perennial appeal of a 'they-rejected-us-first' disciplinary identity.[363] That is, if one considered only how very many protests by historians of technology against the primacy of science were being published at about the time that *Technology's Storytellers* (1985) appeared, and have continued to be published in the following two decades,[364] one might be inclined to regard those publications as manifestations of the postmodern reversal in primacy then under way. However, taking into consideration the fact of their being protests, angry protests, against a primacy that science was improperly thought to hold, they must be regarded as still fundamentally modern.[365]

A case in point is Wise's chapter on 'Science and Technology' in *Historical Writing on American Science* (1985), the first volume of the new series of *Osiris*, published by the History of Science Society. Wise opened with an account of the then recently published article by William Broad in the *New York Times* on Derek Price's last publications, in which Broad found a rejection of the 'remarkably widespread wrong idea that has afflicted generations of science policy students … that science can in some mysterious way be applied to make technology'.[366] As stated by Wise, that 'wrong idea' of 'policymakers'

> … depicts science and technology as an assembly line. The beginning of the line is an idea in the head of the scientist. At subsequent work stations along that assembly line, operations labeled applied research, invention, development, engineering, and marketing transform that idea into an innovation. A society seeking innovations should, in the assembly-line view, put money into pure science at the front end of the process.[367]

So far the argument could be a manifestation of the postmodern reversal in primacy. Then, making 'science policy-makers' the bad guys, 'historians' the good guys—lumping 'historians of science and technology' together as 'historians' without distinction (perhaps as the condition of publication of this protest under the auspices of the historians of science)—Wise's principal contention and conclusion was that:

> Refuting the assembly-line model stands as a main contribution of the historians to the discussion of the relation of science and technology in modern America. In its place, most historians have asserted the autonomy of technology in relation to science … . All knowledge is not science; technology is knowledge, too.[368]

The breadth (and lack of depth) of this search for autonomy is evident in Wise's observation that:

> Historians of science and technology … have put forward metaphors depicting science and technology as mirror-image twins, a married couple, a lemon and lemonade, opposing armies, opposing meteorological fronts, or sovereign states. The key idea behind all the metaphors is autonomy.[369]

So much stronger was Wise's concern to establish as fact the autonomy of technology from science than to inquire into the factual relations between science and technology,

that he proposed definitions of science and of technology specifically constructed so as to 'make the assertion that science provides the knowledge base for technology meaningless'.[370] Thus it is hardly surprising that in his review of the historical literature on technology in relation to science in the 19th and 20th centuries—what forms the second half of his paper—Wise quite ignored the primacy given science by nearly all writers in that period, as also the view, almost universally held through that period, of technology as applied science.

As cohesive principles for the discipline of the history of technology, the animus against science and the commitment to ignoring science have been inseparable—with animus generally pointing the way. There is, of course, a certain incompatibility between that motive and that modus: one cannot ignore science entirely while still giving expression to the animus against it. Typically—and we have seen this already with Williams' representation of Mumford—the animus can be glimpsed only here and there, while the exclusion of science holds almost everywhere. Thus the greater distortive effect upon the representation of the past results from this exclusion of science from the history of technology. That distortive effect is the grosser the more important a place science did in fact have in the minds of technological thinkers and actors. In modernity that was always a highly important place, indeed a preposterously important place, as this study has shown.

'Marx and the Machine' (1984), MacKenzie's often-cited essay, is a good example of this motive and this modus working effectively together to distort the views of—and thus our view of—one of modernity's main men. Nominally examining the question whether Marx was a technological determinist, MacKenzie proceeded from the conviction, now a matter of course among historians of technology, that no good guy could be a technological determinist. Since Marx was, without argument, a good guy, MacKenzie found it unnecessary to make out a case that Marx was not a technological determinist. Quoting *Capital* over and over on the awful meaning of machine production for the production worker, MacKenzie simply avoided the question of Marx's view of the cause and source of technological advance—even on the page or two where he said he was discussing it.[371] Instead, MacKenzie focused upon Marx's definition of machine. Marx defined a machine as a mechanized hand-tool, a definition which MacKenzie seemed to find quite insightful (but even Mumford knew better).[372] Marx, according to MacKenzie,

> rejected definitions which saw a continuity between the 'tool' and the 'machine', definitions typical of 'mathematicians and experts on mechanics'. While it is true that any machine is analyzable as a complex of more basic parts 'such as the lever, the inclined plane, the screw, the wedge, etc.' that 'explanation is worth nothing, *because the historical element is missing from it*'.[373]

This is the first of just two, quite brief, 'by the way' passages in MacKenzie's 30-page text that address Marx's views on the contribution of science to modern industry. With science essentially excluded from MacKenzie's analysis, the slur upon 'mathematicians and experts on mechanics' is entirely gratuitous: even if Marx intended one—and he did not—it is irrelevant to anything that MacKenzie was arguing about Marx

and technology that in Marx's time mathematicians and experts on mechanics typically employed another definition of 'machine'.[374]

What, it seems, was important to MacKenzie was to draw out from among Marx's many references to science one that lent itself to an insinuation of a hostile and dismissive attitude toward scientists on Marx's part. This is borne out by the one other passage in MacKenzie's text relating to science:

> The theorist of this waging of class struggle by technical means was Andrew Ure. His 1835 *Philosophy of Manufactures* concluded that 'when capital enlists science into her service, the refractory hand of labour will always be taught docility'.[375]

Among MacKenzie's numerous references to Ure—all of them, apparently, second-hand from *Capital*—this is the first, longest and the most strongly emphasized. Presenting Ure's thesis as though obviously correct, MacKenzie immediately provided evidence of Marx's whole-hearted endorsement: 'Marx cited inventions discussed by Ure as means of doing this', etc. It is only later in MacKenzie's paper, in conjunction with another quotation of Ure to this same effect (briefer and without reference to science), that MacKenzie revealed that scholarly inquiry did not support Ure's conclusion.[376] Marx had given MacKenzie a choice of a dozen juicy quotations from Ure's *Philosophy of Manufactures*, almost any one of which would have served MacKenzie's (impertinent) purpose of emphasizing that mechanization was obviously not in the interests of the working class.[377] MacKenzie chose that one containing a jab at science—though science was no part of MacKenzie's argument—one whose 'conclusion' MacKenzie knew he would eventually have to question, but yet presented as unquestionably true.

With both his references to science—his quotation of Marx and his quotation through Marx of Ure—MacKenzie had broken his otherwise strict silence regarding the importance for technology that Marx attributed to science. In doing so MacKenzie adroitly insinuated into Marx an animosity toward science that was, presumably, MacKenzie's own.[378] Furthermore, the MacKenzie case suggests that animosity toward science and ignoration of science were not specific to the development of the American division of the history of technology discipline, that, on the contrary, by the early 1980s they were, or were becoming, international orthodoxies.

Twenty years earlier, while the historians of technology were still seeking their disciplinary identity through distinguishing technology from science, their future policy of treating technology without reference to science was being put into practice in the field of American Studies.[379] Through its emphasis upon 'material culture' and its conception of the cultural history of American technology, American Studies would exert a major influence on the history of technology at large. In particular, *The Machine in the Garden: Technology and the Pastoral Ideal in America* (1964), Leo Marx's description and critique of technological enthusiasm in the United States from the 1790s to the 1850s, had become by the early 1980s one of the half-dozen works most frequently cited by American historians of technology.[380] Marx's romantic concerns, so like those of the Mumford of *The Golden Day*, are amply evident in

his title, and his view of the machine was and remained quite as negative as Mumford's had been in that early book. However, where Mumford, in discussing the Emersons and Hawthornes, the best who thought and wrote in the *ante-bellum* period, stressed the high value they placed on science, Marx ignored whatever those admired writers may have thought and written about science—and not only they, but all their contemporaries too. Not that Marx had a good appreciation, let alone made a good case, for the Jacksonian populist insurgency against the primacy of science and other forms of high culture that was occurring in the midst of his period. He cited a couple of contemporary contestations of science's primacy, but, withal, seems to have remained largely unaware of just how far and just why the era that he examined was exceptional in that regard.[381] Rather, in line with the anti-science ideology emerging in American Studies in the early 1960s, Marx simply ignored his sources' references to science, or, if taking notice of them, construed them as references to technology.[382]

Marx's extended exposition of Timothy Walker's 'Defense of Mechanical Philosophy' (1831) is a striking example of willful ignoration of science.[383] The very title of Walker's essay, 'Defense of Mechanical Philosophy', indicates clearly enough that he took the primacy of science to and for technology entirely as a matter of course. Exactly that allowed Walker to shift the issue from the machinery of modern civilization *up* to the higher ground of mechanical philosophy and to do so without the need for even a line of justification.[384] Marx, however, represented Walker as holding a 'theory of the technological basis of culture' and simply ignored everything that Walker said to the contrary, even in the displayed quotations.[385]

Another still more striking example of Marx's ignoration of science is his lengthy exposition and discussion of Daniel Webster's address at the opening of the Northern Railroad in 1847. Touting Webster as 'keen politician, his ear nicely tuned to the prevailing mood', Marx displayed a long quotation from that address, of which the first half is Webster's declaration that:

> It is an extraordinary era in which we live. It is altogether new. The world has seen nothing like it before. I will not pretend, no one can pretend, to discern the end; but every body knows that the age is remarkable for scientific research into the heavens, the earth and what is beneath the earth; and perhaps more remarkable still for the application of this scientific research to the pursuits of life.[386]

One might think it hard to overlook the importance, indeed the primacy, that Webster here gives to science, both as such and as source of all the technological advances that Webster then points to. Marx managed not to mention science, and not only here, but also in his repeated reuse of this quotation in his subsequent writings.[387]

Although by the late 1970s Marx's interpretive approach had been severely criticized from the point of view of historical method and his topic had been redone much more thoroughly, rendering unsustainable his opposition between the popular technological enthusiasm and the sensibilities of that era's most sensitive writers, his reputation among historians of technology continued to grow. Meikle thus found some difficulty in his recent 'Classics Revisited' review of *The Machine in the Garden* in *Technology and Culture* with 'the question of why this book has enjoyed such a major scholarly

reputation, especially among those who address the cultural history of technology'.[388] In explanation of this unreasonably high regard Meikle suggested that:

> Even an internalist historian of technology could applaud a literary scholar who, however uncertain his technical knowledge, had chosen to situate the subject of this formerly marginalized area of study at the very center of American culture.[389]

Doubtless, this must be part of an explanation—and, even as far as it goes, it is not much to the credit of the historians of technology as scholars. Yet it is insufficient—most obviously insufficient in failing to explain an acclaim that is indifferent to Marx's manifest anti-technological bias, which bias led him to restrict *ante-bellum* technological enthusiasm to the less sensitive strata of the population. Or is it that Meikle could and did take for granted that Marx's anti-technological bias was consensual among historians of technology generally, but 'especially among those who address the cultural history of technology'—that that is the unspoken understanding behind Meikle's explanation?

Meikle is also silent about science; nowhere does that word appear. It was obviously important to Meikle that Marx was wrong to underrate the pervasiveness of technological enthusiasm in America in the early 19th century, was wrong to belie that enthusiasm in Emerson, Hawthorne *et al*., for Meikle points Marx's error out in several places.[390] From his silence about science should we infer that Meikle thought Marx right in ignoring the great extent to which, in that technologically enthusiastic era, science got the credit? Was that again the expression of an unspoken understanding with the readers of *Technology and Culture*?[391]

To be sure, there are exceptions to prove this rule of ignoration.[392] That exception most pertinent to this study is Kline's 'Construing "technology" as "applied science"' (1995)—published, oddly, not where the historians of technology who most needed to see it would see it, but in *Isis*, the journal of the History of Science Society. Upon Kline's deep and detailed knowledge of the ideological literature of engineering I relied heavily in order to put in evidence the far-reaching affirmations by American engineers and industrialists of the primacy of science to and for technology, affirmations reaching even to professions of subservience to science.[393] Kline heard those engineers exceptionally well. Manifestly, he did not like what he heard. Not liking it, Kline sought to delegitimate it in the usual way, namely by imputing to those engineers inestimable motives: self-interested considerations of social status, of public image, of professional politics. Over and over, in one way and another, Kline asserted that,

> … engineers, and industrial researchers construed … the relationship between what we now call *science and technology* under the umbrella label of *applied science*, in order to promote their … self-interests regarding status and financial support.[394]

For this 'self-interests' interpretation of engineers' and industrialists' construals of technology as applied science, Kline offered no evidence of his own—none whatsoever. The one study, the only study, that Kline cited for such evidence, the study that remains to this day conventionally cited for this evidence—Layton's *Revolt of the Engineers*

(1971)—provides none. Nor should it have, for Layton himself held no such view of 'his' progressive, science-emulating engineers.[395] To my knowledge, substantial evidence supporting this 'self-interests' interpretation of engineers' and industrialists' construals of technology as applied science has never been brought, neither before nor since Kline's writing.[396]

Unwilling to allow 'their' technologists sincerely to believe what, in modernity, everybody believed—viz. technology's subordinate rank and role relative to science—historians of technology regularly dismiss such affirmations by technologists as being political, if not cynical, tactics to gain social or economic advancement. This way of explaining such affirmations away has become a canonical trope among historians of technology, who would thus seem to believe that the dignity of their subject is most effectively revidicated by attributing self-interested motives and disingenuous discourses to their subjects. Such is their animus against science and their need to exclude it from the history of technology, that rather than allowing their technologists sincerely to depreciate themselves relative to science and scientists, they prefer to disparage their technologists' motives.

How can it be that historians of technology are *so* preoccupied with freeing their subject from subordination to science and its history that they take it as an acceptable means to that end to impute motives to their subjects that are narrowly, even basely, self-interested? How can the passion to liberate their subjects from subjection to science bring the historians of technology so to disparage and delegitimate their subjects' earnestly professed beliefs? For this there is no adequate explanation without invoking a constitutive ambivalence toward technology in the discipline of the history of technology. How else could the founding figures have been such Mumfordolaters, soliciting for publication in the early volumes of *Technology and Culture* his diatribes against technology? How else could *The Machine in the Garden*, clearly deploring the enthusiasm for technology that it described, be taken up with such enthusiasm by the historians of technology?[397] Nor has that ambivalence generally been absent from the (ever-shrinking) sector of the discipline inclined to take the essence of technology as something technological, and whose very mission, therefore, was to vindicate that special and specific form of knowledge. Layton, for example, although vehemently championing the independence, autonomy and self-sufficiency of the engineering sciences in the creation of new technologies, remained from first to last highly critical of the engineers' social values and social role.

The categorical denial of technological determinism, and anathematization of anyone entertaining that heresy—'No greater crime could be imagined'[398]—is surely the clearest and most consequential manifestation of that ambivalence about technology as fact and factor. Still, the route by which 'SHOT's antipathy to the ideology of technological determinism' became established as the 'official posture' of historians of technology was roundabout.[399] Within the diverse assemblage of ambivalent attitudes generally to be found in any adherent to the Society for the History of Technology in the first decades of its existence, horror at the reality of technological determinism was more frequent than was denial of it. Even in the late 1980s, Hughes' stated oppositions

to technological determinism alternated with evidences of his continuing belief in technological determinism.[400]

The younger generation gave greater importance to disciplinary ideology, thus entangling itself in quite a different inconsistency: by the end of the 1970s there was a strong consensus against 'autonomous technology',[401] but there was also, as we saw above, an almost equally strong consensus *for* the autonomy of technological knowledge. So, for example, Wise placed immediately following his claim, quoted above, that 'most historians have asserted the autonomy of technology in relation to science', the parenthetical qualification: '(at the same time as they have been emphasizing that technology itself is not autonomous in relation to economics, politics, and international relations)'.[402] This contradiction remained irresolvable while historians of technology directed the thrust of their liberation movement towards the reconception of technology as an autonomous form of knowledge.[403] Since the mid 1980s, however, as the discipline lost interest in the quest for the cognitive essence of technology, and came more and more to agree with Daniels that 'the big questions for us, then, will all have to do with technology as a social phenomenon', radical denials of both technological determinism and of the autonomy of technology have become an orthodoxy.[404]

With one significant exception: the loudly asserted 'autonomy of technology in relation to science', to be accomplished by the necessarily tacit policy of ignoration of science in relation to technology. For this exception there is even logical reasoning:

> Since 'Technology' is defined as the application of modern (Western) science, and since Science is understood to operate free from all bias in its pursuit of objective truth, technology must not be impeded … .[405]

If historians of technology do not all reason so themselves, so at least they see their contemporaries as reasoning: science is too autonomous, its progress too indubitable, to allow technology to be considered as the application of it while still denying technological progress and/or technological determinism. Science must be kept out if technology is to be kept down.

Thus there is an anti-determinist orthodoxy, and anyone visiting the SHOT website will find posted a notice stating it and warning against violating it:

> While the notion that technology marches of its own predetermined accord still has a strong hold on popular sensibilities, specialists in the interaction of technology and culture now understand that it cannot do anything of the sort. Technology is not autonomous; rather, it is impelled by choices made in the context of circumstances in ambient realms, very often in the context of disputes over political power.[406]

In keeping with this doctrinal tenet, for the past quarter century SHOTites have taken as their mandated mission combating the mistaken 'popular' belief that technology is an autonomous agent in the ceaseless transformation of our life worlds.[407] That their message was more ideological than empirical, could not be admitted. Hence the almost wholly unearned importance attached to the social construction of technology (SCOT) and the ritual citations of *The Social Construction of Technological Systems* (1987), as though the mere statement of such a program constituted a conclusive proof of the falsity of technological determinism.[408]

The fact is, however, that postmodernity has disempowered most of the forces that in modernity were supposed capable of exercising some governance over technology. Among them, science, as we have seen. Consequently, technology is today not only widely, but correctly, regarded as 'unleashed'. Only by willfully blinding themselves to the very real liberation of technology in, and in consequence of, postmodernity is it possible for the historians of technology to regard the belief in technological determinism that they encounter at every hand as merely a persistent popular misconception.

A constitutional ambivalence toward technology, and not merely an ignoration of science, must underlie this unwillingness to see how largely technology has risen in recent years. The effect of that ambivalence is aggravated by the preoccupation of historians of technology with their 'official posture', with the collective project of defining and policing an ideological stance. From the time that project got seriously under way around 1970, the historians of technology have been unusual in the extremity of their demands for doctrinal uniformity and for exclusivity of 'ownership' of the broad field of technology in history.[409] Although priding themselves upon their receptiveness—'We talk a lot in SHOT about the generous-spirited nature of our annual meetings'[410]—the historians of technology have distinguished themselves rather by their concern about 'sociological boundaries' and 'bedrock principle', about who has and who has not 'closely adhered to established disciplinary precepts', about what they must do 'in order to maintain their disciplinary identity', about the 'task of taking command over all the high ground of our subject', and about 'who is best suited to tell the story of technological change'.[411] Never realistically attainable, and never in the interest of more and better historical understanding, that quest for ideological orthodoxy seems today, with modernity abdicating in favor of postmodernity, not merely exorbitant but antediluvian.[412]

IV. Conclusion

It is now fully 30 years since Mayr urged his fellow historians of technology to set themselves the task 'not to discover what the science–technology relationship actually has been in history but what previous eras and cultures thought it to be'.[413] It is more than 10 years since Kline, introducing his exposition of 'Construing "Technology" as "Applied Science"' by quoting Mayr, observed that, 'few … have heeded Mayr's injunction':

> Although [a] large body of literature has discredited the simple applied-science interpretation of technology—at least among historians and sociologists of science and technology—little attention has been paid to the history of this view and why it (and similar beliefs) has been so pervasive in American culture. … why historical actors described the relationship between science and technology the way they did and … what this may tell us about the past.[414]

Yet this deficiency of the historiography of US technology to which Kline rightly drew attention is also a deficiency of what Kline himself there added to it. 'Construing "Technology" as "Applied Science"', so creditable for its demonstration of the pervasiveness of this technology-as-applied-science view among US engineers, did not

pursue the question *why* US engineers held that view, and did not address at all the larger and more fundamental questions: why that view 'has been so pervasive in American culture', and 'what this may tell us about the past'. Indeed, Kline deprived himself of the opportunity to address those questions when he opted to delegitimate the engineers' discourse by stigmatizing it as a parole intended to advance social and economic self-interests.[415]

To be sure, delegitimating discourse is what the history of technology, and the history of science too, has so largely been about in these past three decades. It is also, largely, what this exposé of the preposterous primacy of science is about, but here with an important difference. Whereas it has been a common conceit in our disciplines since the 1960s that reduction to 'the social' provides the most satisfactory explanation of whatever it is that we pull out from the past—a conceit now increasing passé[416]—this paper casts further doubt on the explanatory adequacy of the 'the social' and makes a case for reference to 'the cultural' as a more satisfactory explanation for *beliefs* held by historical actors.[417] Thus, in order to explain discourse diverging so flagrantly from the demands of social self-interest as that found among late-19th and early-20th century American engineers, or from the demands of systematically elaborated intellectual positions as we found with Marx, and Veblen, and Dewey, the historian must needs invoke a power greater than social interests and logical consequence. That power, the power that makes an historical era, is the power of 'the cultural'.

It is 'the cultural' that in large measure decides what the social interests of an era's actors are. Where it does not determine those interests themselves, it largely determines the extent to which an era's actors act in fact in accord with their interests. To suppose, on the contrary, that we know those interests *a priori*, without first grasping the constellation of presuppositions constituting a cultural epoch, must necessarily lead the historian into anachronistic imputations—as has been the case with the 'social interests' explanation of the American engineers' self-subordination to science.[418] The proper role of social and, especially, institutional explanations is then ancillary, with their greatest service lying in the explanation of departures from the pattern or norm created by and answering to the reigning cultural values of the historical era.[419] Foundational is the existence of that norm, of those preposterous, but yet taken-for-granted, prejudices shared by persons so socially, politically, and philosophically opposed to one another as a Marx and a Ure, as a Dunn and a Dewey, as Thorstein Veblen and the National Association of Manufacturers.

'The cultural' can perform this 'epoch making' service only if it is not postmodernized into a multiplicity of coexisting incoherent cultures; can perform this service only if, as modernity would have it, culture is conceived as a constellation of presuppositions integrating the outlooks of actors in diverse social situations over an extended period of time; can perform this service only if the historian's task is understood to be delineating that constellation of cultural values by connecting the 'dots' of temporally, socially, and intellectually 'scattered' articulations. Without this, admittedly quite old-fashioned, conception of the historian's task we would be hard

pressed to conceive of, let alone define, distinct historical eras. If, furthermore, we reject the notion of historical eras as a modernist illusion, or, worse, oppression, we are inevitably also rejecting history as a scholarly discipline.[420]

The imperviousness to the force of facts so evident in modernity's beliefs about the relation between science and technology exemplifies a circumstance often encountered by the historian: culturally determined valuations are regarded as self-evident inferences from obvious facts. Thus today, when notice is taken of the demotion of science in cultural rank, of the loss of confidence in the trustworthiness of scientists, and more especially of the loss of belief in basic research as the key source of technological innovations—all gradually gaining more force and effect since the late 1970s—this change in our view of science is attributed to the force of facts; but to different facts by different folks. When asked for the specific grounds of our disillusionment with science, the softer heads say that science has been the source of deplorable new technologies, while the harder heads say that science has failed to deliver on its promise to spawn new technologies. As always, what is fundamental is what is common to both: a presumption that 'technology'—where 'technology' stands for the totality of real world utilities—is the only relevant point of reference. Thus the facts of the matter are of secondary importance; of primary importance are the cultural values—and specifically here, today, seeing no value in science except in reference to its productiveness of technology.

How differently matters stood in modernity! So differently that to some of us it came to seem urgent to bring into light and sight the true fact of the matter, namely, that modern science, and especially late modern science, was not really as modernity conceived it and lauded it; that only the smallest part of what governments supported under the rubric 'research' approximated to the widely accepted conception of science as for-its-own-sake pursuit of understanding. This task came to seem especially urgent where and as the scientists' confabulations regarding 'science' obfuscated the very large role that the military was playing in the making of science through the generation and distribution of that governmental support for 'research'. As Edgerton said a decade ago regarding modernity's insistence 'that science and war were antithetical to each other or, at least, radically different enterprises':

> These stories, which were intended to be taken literally, were fairy stories, but ones that have bewitched many students of the relations of science, technology and war. The divergence between this picture and the most straight-forward empirical analysis of the relations of science and war … will astonish even the most hard-bitten cynic. Although many details of the relations of science, technology and war were not known by contemporaries, and much remains to be discovered today, the divergence cannot be explained by secrecy. There was always sufficient information in the public domain to yield a very different picture … .[421]

That 'divergence' between accepted truth and actual, available, even obvious fact, which Edgerton rightly found astonishing, is but another example of the impotence of facts when pitted against widely and strongly held cultural values. As we historians, in the course of our studies of late modern science, became aware of this 'divergence' and of the more or less intentional obfuscation of science's decisive involvement with the

military, it seemed most needful to draw attention to what in modernity nearly everyone preferred to ignore.[422] Today, however, in fuller consciousness of our postmodern condition, it is, on the contrary, most needful for us as historians to remind ourselves of the strength and pervasiveness of modern values in the modern era—values so strongly held by so many as to cause Edgerton's obvious facts to be ignored. Hence the investigation that forms the core of this paper.

Asking after the putative relation between technology and science in a sample of periods, theorists, and practitioners, all biased toward technology, we have found that, prior to *ca* 1980, the primacy of science in cultural value *to* technology, and in cultural practice *for* technology, was very nearly universally presupposed, notwithstanding that the primacy of science itself as cultural value was continually challenged in modernity by a powerful romantic tradition among artists and intellectuals, and intermittently by populist upsurges. The rapid reversal about 1980 in the more than merely modern—the millennial—primacy relations between science and technology is unquestionably epoch-making. If identified with the transition from modernity to postmodernity, that reversal in the science–technology relation gives to this historical–cultural transition a greater specificity and a still greater significance.[423]

There remains, however, the question 'why historical actors described the relationship between science and technology the way they did and … what this may tell us about the past'. My endeavor here to bring clearly into view the fact—and the date—of the reversal of primacy between science and technology is therefore appropriately concluded with a brief indication of what seems to me the single best answer to that question.

The primacy of science to technology, and even the primacy of science for technology, has a long history extending back through premodernity. Consequently, in modernity the primacy of science to technology was sustained not only by specifically modern cultural values, but also by cultural values carried over from premodernity—notably disinterestedness, the high value of which in the Greek philosophical tradition was reinforced by elements of the Judeo-Christian tradition. If, however, it is modernity, specifically, that we wish better to understand, then it is the specifically modern values sustaining the primacy of science that demand our attention. Among those, the most pertinent and distinctive was modernity's 'methodism', its stress upon proper method in all its doings, its insistence that the means are prior to ends, that the end is justified, indeed sanctified, by the means and only by the means employed to attain it.

The strength of the terms of deprecation that modernity directed against the maxim 'the end justifies the means' is a good indication of the strength of the cultural value against which those adopting that maxim offended. Even in the restricted sphere of relations between states, where, as *Realpolitik*, that maxim was generally followed in practice, and had apologists for so doing, the acceptability of 'the end justifies the means' was constantly contested, and that maxim was never asserted by its apologists without stated or implied reservations. Today, on the contrary, 'the end justifies the means' is unapologetically asserted as rationale for policy and practice not only in political conflicts, but by all manner of institutions (institutions of research and higher

learning emphatically included). More, 'the end justifies the means' is the operative maxim in all personal relationships. Today, all *is* fair in love and war, what in modernity (as in premodernity) could be said only with irony.

Modernity's preoccupation with method is evident in natural philosophy already in the 16th century and over the following three centuries science in all its forms came to be identified ever more completely with method rather than with ascertained knowledge. By the mid 19th century 'the means legitimate the end' had become the axiom from which all epistemology proceeded. Similarly in modernity 'the means legitimate the end' became the overriding regulatory axiom in conceptions of the state and of its subjects, normative for, if not actually characteristic of, almost all aspects of the constitution and conduct of authority. Method was at the center of such otherwise divergent developments as British legal and parliamentary procedures on the one side, and Continental statist bureaucracies on the other side. Again, by the mid-19th century 'methodism' had become the only generally acceptable theory of the state—pre-eminently of the liberal-democratic state, but scarcely less of the bureaucratic-technocratic administered state, and even, in a perverse way, of *der Führerstaat*.

In liberal-democratic states, or in such parties within the state, methodism was the guiding conception not only of the conduct of authority *vis-à-vis* the subject but also for achieving collective social progress. Because science was also the most persuasive exemplar of collective progress through 'methodism', its cultural standing was doubly high: so high at the middle of the 20th century that science could be put forward as—and widely accepted as—a model for the functioning of a liberal-democratic society.[424] Today only unreconstructed modernists, like those seeking to revive the finalization theory of the Starnberg school, cling to so thoroughly outdated an ideal—outdated above all through the postmodern disinclination toward, and loss of faith in, methodism.

It is important to bear in mind, however, that the argument for science's high cultural standing as resulting from modernity's attachment to methodism applies with full force only to—and thus undergirds the esteem enjoyed in modernity by—'pure science', science as for-its-own-sake knowledge. What validated such knowledge as knowledge was not its serviceability to some preset end, but the propriety of the means, the method, with which that knowledge was attained. To be sure, 'pure science', while it was thought to exist, was esteemed not solely as methodist. I have repeatedly emphasized disinterestedness in the foregoing exposition, and I could have emphasized autonomy—both of these virtues, in the modern mind, being especially, if not uniquely, characteristic of for-its-own-sake science.

In all these regards, technology is the antithesis of for-its-own-sake science. Though like scientists, technologist could try to excuse themselves by distinguishing between the essence and the use of their work, that excuse never worked so well for them as it did for scientists. Technology's incapacity to create and warrant its own ends—and consequently its inherent lack of autonomy—necessarily and unavoidably implied in modernity a depreciative regard, a subordinate cultural rank. Moreover, modernity's methodist motto, 'the means legitimate the end', is inherently inapplicable to technology. For technology the deprecated inverse holds: 'the end justifies the means'. A

technology is by definition a set of means to an end or ends, and it is to the efficacy and efficiency with which that end is attained that we necessarily look in order to evaluate the goodness and rightness of the means employed (having bundled into our measures of efficacy and efficiency whatever other ends are to be taken into consideration). In postmodernity, however, when the end does indeed justify the means, there is an intrinsic coherence between technology and the perspectives guiding our thought and action, a coherence that implies a high cultural ranking of technology. More than that: in postmodernity—pragmatic-utilitarian, primacy-of-ends postmodernity—technology is simply all there is, all there is apart from our *de gustibus*, not-to-be-argued-with, ends.

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Notes

[1] Edgerton, '"The Linear Model" Did Not Exist', provides ample evidence of the present popularity of pummeling 'the linear model'. Already in 1983 Callon, 'Society in the Making', 83, thought it important to stress that 'I am not simply repeating the already countless criticisms of the notion of innovation as a linear process'. Godin, 'Measuring Science', cites numerous examinations of the historical relation between science and technology in the past 40 years.

[2] 'The Turn to Technology in Science Studies' (1991) was recognized early on by Woolgar, and appropriately reflexively: 'In recent years there has been an almost indecent rush by some sociologists of scientific knowledge (SSK) into the social study of technology (SST), this author being no exception'. Having made this observation, Woolgar could hardly avoid raising the question, 'What accounts for this move from science to technology?' However, he did avoid addressing it, turning instead to the more urgent matter of the 'danger of forgetting the strategic theoretical significance of the sociology of scientific knowledge'.

[3] I drew attention to the unwarranted confusion of postmodernism, a theory and ideology articulated by intellectuals in the 1970s and 1980s, with postmodernity, the historical era then incipient, in Forman, 'Recent Science'. Postmodernism was a manifestation of the onset of postmodernity, but by no means the whole of it, nor even an accurate description of it.

[4] Eloquent in this regard is Dear, 'What is the History of Science the History *Of*?' (2005). 'The question in my title', Dear explained, 'arises from an anxiety that the history of science as a scholarly specialty is less obviously self-defining than it once was'. Yet Dear, whom one would expect to be among the last to renounce science as natural philosophy, is *not* protesting the confusion of science with technology by historians of science. Quite the contrary. Dear is incensed by 'the basic ideology of modern science', namely, its presumption that the instrumental effectiveness of science is a result of the truth of its theoretical constructs. Dear insists that 'the instrumentality in modern science need not be seen as necessarily reliant on science's natural philosophy' (pp. 404–5), i.e. even Dear now sees science as technology.

[5] Fifteen years ago, at a meeting devoted to critical problems in the history of science and the history of technology, Laudan, 'Natural Alliance or Forced Marriage? Changing Relations between the Histories of Science and Technology', S26, stated presciently and forcefully that 'Where once technology was subsumed under science, now science is on the brink of being subsumed under technology; the old hierarchy is being turned on its head. If this inversion is adopted, we will have a version of the history of the relations of science and technology in which the history of technology—or technologies—takes the lead and in the modern period science appears simply as one more technology. Moreover, the technology of artifacts and processes that has dominated the history of technology will be seen as simply one small part of the bigger world of *techne*'. This radically postmodern prospect, deriving principally from the philosophy of technology, has largely been ignored among historians of technology. See, also, notes 29 and 31, below, and Section III generally.

[6] With the very big difference that today our ends are totally 'individualized', i.e. are relative to, and chosen by, the individual person or corporate entity (Bauman, *The Individualized Society*), and with the qualification that pre-modern Western legal traditions, both Roman and Germanic, have included a conception of justice as a means-sanctified end.

[7] Harding, 'The Melancholy of Technology'—but, as I noted above, it would be an even more serious mistake simply to equate 'Postmodernism' with postmodernity.

[8] Zierdt-Warshaw et al., *American Women in Technology*, Table A3.

[9] Kline, 'Cybernetics… The Emergence of "Information Technology" as a Keyword, 1948-1985', has accumulated evidence to test this conjecture, and his exposition includes the following quotation, dated 1985, implicitly supporting it: '"Information technology in its strictest sense is the new science of collecting, storing, processing, and transmitting information."' Yet because Kline was preoccupied with nursing a grievance over discourse 'subjugating *information technology* to *information science*' (529), he was not well prepared to see a reversal in discourse setting in at about that time—a reversal exemplified, I surmise, by his quotation.

But to so specific and far-reaching a claim—that in modernity 'technology' *never* denoted science too— counterexamples are not hard to find. For me, the nearest lying is the name—National Museum of History and Technology—given the museum which employs me as curator of its modern physics collection when that museum was created in the early 1960s. Weightier as counterexample is *The Engineering Ideal in Biology* (1987), the program of Jacques Loeb and his students identified and described by Pauly. But Loeb's conception, emphatically an aberration in its early 20th century context, aroused Pauly's interest because of the emergence in the mid 1970s of our contemporary conception of molecular biology as engineering. That conception, arising out of the recombinant DNA controversy and first appearing in the term 'genetic engineering', was initially opprobrious (e.g. George Wald, 'The Case Against Genetic Engineering', *The Sciences*, Sept. 1976, as reprinted in Watson and Tooze, *The DNA Story*, 110). The opprobriousness of 'genetic engineering' as label for a

research program bespeaks its origin in a scientific milieu still modern. The neutralization of the negative connotations of 'genetic engineering', and the wide acceptance of that appellation by about 1980, reflect the incipient postmodernization of the general cultural milieu.

[10] Capshew and Rader, 'Big Science: Price to the Present'. The reference is, of course, to Derek J. de Solla Price (Price, *Little Science, Big Science*), the most ingenious of the historians of science of his generation.

[11] Thus in 1992 Galison, writing 'Introduction: The Many Faces of Big Science' for the volume he edited with Hevly, treated 'big science' as an entirely serviceable term, the intent of their volume being to 'explore the many kinds of activities that are subsumed under the term "big science."' Five years on, in *Image and Logic*, 553, Galison was contemptuous of that term: 'My view is this: as an analytic term, "big physics" is about as helpful to the historian of science as "big building" would be to the historian of architecture'.

[12] Collins and Pinch, *The Golem* and *The Golem at Large*. The displacement of 'science' by 'technology' is not the only postmodernization apparent in the title of their sequel: the displacement of 'everyone' by 'you' bespeaks, and appeals to, the postmodern individualization of society. Similarly, Hård and Jamison, *Hubris and Hybrids*, insist on the inverted order, 'technology and science', and in their chapter titles speak not at all of science but only of 'technoscience'. However, Jacob and Stewart, *Practical Matter*, like Jacob's earlier work, Appleby *et al.*, *Telling the Truth about History*, is a defense of modern presuppositions—here the primacy of science—under a half ironic, half postmodern title. A contrived slap (p. 45) at Shapin and Schaffer, *Leviathan and the Air Pump*, without naming them, let alone citing them, is indicative both of Jacob's anti-postmodern position and of the way in which she here conducts her argument for keeping science ahead of technology.

[13] For example, the 1992 parole of Nicholas Metropolis quoted in Forman, 'Recent Science', 119: 'Since World War II the discoveries that have changed the world were not made so much in lofty halls of theoretical physics as in the less-noticed labs of engineering and experimental physics. The roles of pure and applied science have been reversed.' An early, indicative, and influential example in the history and philosophy of science is Hacking, *Representing and Intervening* (1983), with its 'anti-theoretical' intent stated in his preface. Kwa, 'Interdisciplinarity and Postmodernity', 339–41, draws attention to high theory's loss of primacy in the environmental sciences in the past two decades. Shinn, on the contrary, has argued for the central role of 'research technologies' throughout the 20th century, and consequently argued against the existence of a postmodern discontinuity: Shinn, 'New Sources', and earlier publications cited there.

[14] Typical for the period is Gilpin, *France in the Age of the Scientific State* (1968). Between his own preconceptions and French 'science' rhetoric, Gilpin remained oblivious to how little his quoted sources were in fact speaking about science. That capacity of 'science' to cover technology long remained especially strong among the political scientists and science policy scholars, in good part because they have thought of themselves as scientists. Their discourse too began to shift in the mid 1980s, but only slowly. So, for example, although Rouban, *L'État et la science: La politique publique de la science et de la technologie* (1988), gets technology too up there in his subtitle, but in his very detailed table of contents 'science' appears 23 times, 'technologie' only once. Needless to say, by rights the ratio should have been reversed, for this work too is concerned with science only secondarily, with technology primarily.

[15] Mirowski, 'Caveat emptor', drew attention to the acuity of David Noble and David Dickson who so early as 1981 ('By Force of Reason') were able to recognize the fact of a fundamental shift having taken place in the USA in the late 1970s in the assumptions about what government science policy was for, and was to do. That long paper and also Dickson's *The New Politics of Science* (1984) are impressively perceptive, informed and documented descriptions of the reversed relation between science and technology. It is thus the more striking that Noble and Dickson were not then yet able to unthink the assumption of the primacy of

science, which assumption enabled 'science' to stand for technology too, not only in the title of Dickson's book but through the text as well. (As noted below, note 73, a Marxian perspective strongly *supported* that assumption.) By 1985 Elzinga had recognized that 'The science policy of the 1960s underwent a transformation into what is more properly called technology policy in the 1970s and thereafter innovation policy in the 1980s. The end result has been the end of science policy. Science policy—in the sense of polity for science—ceased to exist'. Elzinga, 'Research, Bureaucracy', 194, to which my attention was drawn by Kwa, 'Interdisciplinarity', 337–8, again making that point. Likewise, Kwa, 'Programming', abstract: 'Prior to 1980, funding agencies spoke in the name of science to the national states, articulating the needs of science. They now speak to science'.

[16] Already in 1979 Lyotard, *La condition postmoderne*, 77 (*The Postmodern Condition*, 47), had asserted that 'Le rapport de la science et de la technique s'inverse' ('The relationship between science and technology is reversed'). Beginning in the early 1980s, Lyotard began to use the term 'technoscience'—which he neither invented, nor claimed to have invented. In another paper I will explore the origins and progress of 'technoscience' in some detail, likewise as index of the course of the modern–postmodern transition.

[17] Latour and Woolgar, *Laboratory Life*; Latour, 'Give Me a Laboratory', 159–69.

[18] Latour, *Science in Action*, 131–2, 168–9. (My attention was drawn to Latour's two very brief discussions of the science–technology relation by Radder, 'Technology and Theory in Experimental Science', 158.) Probably important for that reversal in Latour's conception of primacy in the science–technology relation, but certainly important for the introduction of the primacy of technology into the history of science, science studies, and humanistic studies more generally, were Shapin's earlier publications: Shapin, 'Pump and Circumstance: Robert Boyle's Literary Technology' (1984) and, with Schaffer, *Leviathan and the Air-Pump* (1985).

[19] The scholarly reviews of *Science in Action* that I have seen are by Amsterdamska, Bijker, Etzkowitz, Goldman, Hacking, Jardine, Myers, Oldroyd, Overman, Pinch, Rocque, Rose, Shapin, Shrum, Star and Yearly. Those taking notice of Latour's equation/conflation of science and technology are Etzkowitz, Goldman and Shrum.

[20] It received its highest praise in the *Times Literary Supplement*, where Jardine concluded his review: 'This is no mere bricolage, but a coherent and powerful framework for research. I predict that Science in Action will have an impact comparable to that of Thomas Kuhn's Structure of Scientific Revolutions both as a provocation to philosophers and as an inspiration to sociologists and historians of science'.

[21] A good indication of the organized resistance to the conflation of science with technology is Lecourt's prefatory material to *Dictionnaire d'histoire et de philosophie des sciences* (1999). And a good indication of what is being resisted is *Tekhnema: Journal of Philosophy and Technology*, http://tekhnema.free.fr, or, still better, the career of Bernard Stiegler, the second successor to Pierre Boulez as director of the Institut de Recherche et Coordination Acoustique/Musique (Ircam) at the Centre Pompidou. For biographical background and interviews, see: http://www.rouge.com.au/3/ister.html; accessed 16 November 2004.

[22] In *Science in Action*, 29, Latour claimed to be coining the term and the Anglophone world of science studies has taken him at his word; so, for example, Shapin in his review of the book in *Social Studies of Science*. Then, in a note (n. 1, p. 548) added in proof, Shapin reports a personal communication from Latour admitting that he had not in fact invented the word and alleging that he had it from Heidegger. That too is an invention—and not on Latour's part only (as I will show in another paper)—an invention indicative of the importance given Heidegger by Latour and by the philosophic milieu in France. (On which see note 37, below.)

[23] Pestre and Krige, 'Some Thoughts', 94, 93.

[24] Sibum, 'What Kind of Science'; Auerbach, *Entwicklungsgeschichte der modernen Physik*, 4. Numerous examples of such rhetorical concessions by physicists and mathematicians to the post-First World War cultural milieu of German Central Europe are given in Forman, 'Weimar Culture'.

[25] This invidious distinction between observational and experimental sciences, i.e. between those attentive to nature and those tormentive of nature, was quite generally maintained by German romantics from Schiller and Goethe onward. Rothschuh, 'Bedeutung apparativer Hilfsmittel', 161–2, quoted Schiller on Alexander von Humboldt's experiments on animal electricity: 'It is the naked knife of reason that shamelessly wants to measure out Nature, who is always ungraspable and in all her points honorable and unfathomable'. Höpfner, *Wissenschaft wider die Zeit*, 58–62, arguing the essential agreement between Novalis (Friedrich von Hardenberg) and Goethe regarding experimental science, and more especially physics, displays parallel quotations deploring such science as 'tearing phenomena out of their context', 'tearing off Nature's limbs on the torture rack' or 'with sharp knife cuts'. (The image goes back of course to Francis Bacon, but not the animus.) And Riese, 'The Impact of Romanticism on the Experimental Method', 19, referring to the Goethe-idolizing physiologist Johannes Müller: 'The text of Müller's inaugural academic lecture, delivered in 1824 and printed in 1826, contains a chapter entitled "Observation and Experiment" ("Beobachtung und Versuch"). Whereas Müller believed observation to be natural, patient, assiduous, faithful, unprejudiced, he called experimentation artificial, impatient, eager, digressive, passionate, unreliable. Nothing is easier, Müller said, than to make a series of so-called interesting experiments. All you need is to tempt nature by force; in her emergency she will always make a painful reply.' See also note 40, below. Chemical experimentation was regarded more favorably by late 18th century German romantics as standing between the mechanical and the organic: Kapitza, *Die frühromantische Theorie der Mischung*, 166–9, *et passim*. I could find no discussion of the above in Richards, *The Romantic Conception of Life*.

[26] Auerbach, *Entwicklungsgeschichte*, 4–5: 'In diesem Sinne ist die Physik Iher Methode (nicht dem Ziele) nach streng genommen überhaupt keine Naturwissenschaft, wie es die Astronomie, die Geologie, die Botanik usw. sind; sie behandelt gar keine Naturerscheinungen, sondern künstlich und nach Willkür des Forschers hergestellte Phänomene; und in diesem Sinne kann man sie geradezu als eine technische Wissenschaft bezeichnen. Wohlverstanden: der Methode nach; denn dem Ziele nach ist und bleibt sie eine reine Naturwissenschaft, insofern sie nicht (oder doch nicht in erster Linie) auf technische Anwendungen ausgeht, sondern der reinen Erkenntnis dient.'

[27] An unwillingness to understand that, how, why, in modernity a distinction was drawn between experimental physics and technology appears also in Sibum, 'Experimentalists in the Republic of Letters', 107.

[28] Lecourt, *Contra la peur*, 144. Lecourt's appraisal is quoted by Janicaud, *Heidegger en France*, 1: 442.

[29] Ihde, *Instrumental Realism*, 140, 55. (The grammar is all Ihde's.) Similarly, Kaplan, *Readings in the Philosophy of Technology*, 431, writes: 'Science is embodied in its technologies, and technologies determine what is science'.

[30] Lovitt and Lovitt, *Modern Technology*, 265. Wm. Lovitt, a blind scholar, had been one of the first to reach this insight, and the first to translate Heidegger's principal primacy of technology texts.

[31] Historians and sociologists of science and also, more surprisingly, of technology have remained largely unaware of just how important Heidegger has become, and consequently have become themselves only unconsciously Heideggerian. Again, Laudan, 'Natural Alliance', S25–6, pointed out rather early on that this unprecedented primacy of technology 'derives from from an old tradition, but it is not the analytic; rather, it is the tradition of Continental philosophy, of hermeneutics stemming ultimately from Husserl and Heidegger. It is this that has supplied the underpinning for most recent sociology of science. The insistence on the centrality of praxis (which has been transmuted into the talk about practices that is so popular in contemporary history of science), the talk of mediations and transparency, and the very broad interpretation of *techne* as purposive action all come from outside the analytic tradition that has informed history of both science and technology for most of this century'. She

continued to observe (S27): 'But historians of technology have tended to stay with their traditional definition of technology as having to do with artifacts and have not embraced the wider definition of *techne* that is becoming popular among historians of science'. Earlier still, T. P. Hughes noticed the Heideggerian affinities of actor-network theory: Hughes, 'The Seamless Web' (1986), 288, and the unsigned but obviously Hughes-authored introduction to Part I in Bijker *et al.*, *The Social Construction of Technological Systems*. Hughes, *American Genesis* (1989), 6, offered a definition of technology that he himself regarded as Heideggerian. More recently, however, Hughes excluded Heidegger entirely from his reflections on 'how to think about technology and culture': Hughes, *Human-Built World* (2004). Latour, who 'laundered' so much of that hidden influence of Heidegger into science studies in recent decades, now argues openly for Heideggerization of science studies as the way from 'critique' to 'concern': Latour, 'Why Has Critique Run out of Steam?'.

[32] Borgman, 'The Question of Heidegger and Technology' (1987), 107–8. Alderman, 'Heidegger's Critique of Science and Technology' (1978), exemplifies the inability prior to the 1980s to grasp Heidegger's subordination of science to technology.

[33] Grene, 'Heidegger, Martin', 462. Heidegger, 'The Age of the World View', 269 (translated by Grene). 'Zu den wesentlichen Erscheinungen der Neuzeit gehört ihre Wissenschaft. Eine dem Range nach gleichwichtige Erscheinung ist die Maschinentechnik. Man darf sie jedoch nicht als bloße Anwendung der neuzeitlichen mathematischen Naturwissenschaft auf die Praxis mißdeuten. Die Maschinentechnik ist selbst eine eigenständige Verwandlung der Praxis derart, daß diese erst die Verwendung der mathematischen Naturwissenschaft fordert. Die Maschinentechnik bleibt der bis jetzt sichtbarste Ausläufer des Wesens der neuzeitlichen Technik, das mit dem Wesen der neuzeitlichen Metaphysik identisch ist'. Quoted from 'Die Zeit des Weltbildes', in Heidegger, *Holzwege*, 75. The best translation of this work that I have seen is Heidegger, 'The Age of the World Picture' (translated by Young and Haynes).

[34] A more forceful deprecation of the notion that technology is applied science occurs in Heidegger's 'Die Frage nach der Technik': Heidegger, *The Question Concerning Technology*, 21–3. Interestingly, Heidegger's is exactly contemporaneous with the first and most forceful deprecation of that notion by Ellul, in *La Technique* (1954), translated as *The Technological Society*, 7–8: 'Everyone has been taught that technique is an application of science. … This traditional view is radically false.' Never again, however, would the prolific Ellul, though he published lots more on technology, issue anywhere near so strong a denial that technology is applied science.

[35] 'Ist die neuzeitliche Naturwissenschaft—wie man meint—die Grundlage der modernen Technologie oder ist sie ihrerseits schon die Grundform des technologischen Denkens, der bestimmende Vorgriff und der ständige Eingriff des technologischen Vorstellens in die ausführende und einrichtende Machenschaft der modernen Technik?', Heidegger, 'Neuzeitliche Naturwissenschaft'. Wolf, *Hermeneutik und Technik*, 11, opens his learned book on Heidegger by quoting this sentence and surmising that this text was Heidegger's last. Wolf, however, missed the admonitory point of Heidegger's 'greeting', misconstruing the meeting as that of 'eines Kolloquiums über "Neuzeitliche Naturwissenschaft und moderne Technik"'—what Heidegger would certainly have welcomed, but what was in fact still unthinkable at that date. In 2001, Michael Kelly introduced his report of the 35th annual meeting of the North American Heidegger Society with the observation that 'participants both commemorated the twenty-fifth anniversary of the death of Martin Heidegger (May 26, 1976), and breached a twenty-five year rejoinder to his letter to the tenth convening of the society, April 11, 1976. In this letter, Heidegger called us to consider "The Relation of Modern Science to Modern Technology," which became this years theme.'

[36] '… jeder der Teilnehmer dieser Frage auf seine Weise eine Beachtung schenkte und sie als Anregung für seinen Arbeitsbereich aufnähme'. Heidegger, 'Neuzeitliche Naturwissenschaft'.

[37] Foucault, *Politics, Philosophy, Culture*, 250; Rockmore, *Heidegger and French Philosophy*; Janicaud, *Heidegger en France*. See also note 22, above.

[38] Against my characterization Heidegger would have objected that 'Ich habe nie *gegen* die Technik gesprochen, auch nicht gegen das sogenannte Dämonische der Technik. Sondern ich versuche das Wesen der Technik zu verstehen', quoted by Wolf, *Hermeneutik und Technik*, 11, from Heidegger, *Gesamtausgabe*, 16: 706. Indeed, in 'Die Frage nach der Technik', his fullest statement, Heidegger had said—and the statement is often quoted—that 'Die Technik ist also nicht bloß ein Mittel. Die Technik ist eine Weise des Entbergens. Achten wir darauf, dann öffnet sich uns ein ganz anderer Bereich für das Wesen der Technik. Es ist der Bereich der Entbergung, d.h. der Wahr-heit' (Heidegger, *Vorträge und Aufsätze*, 20). However, as Heidegger's exposition there and on the following pages makes clear, he was toying with his audience, knowing that those who came to hear him would find that 'implication' of his argumentation uncomfortable. To his audience's relief, Heidegger then explained the difference between good *Entbergung* and the bad *Entbergung* by modern technology. Some part of Heidegger's denial can be accepted by distinguishing between the early Heidegger, the son of a village cooper, who expressed a romantic attachment to traditional handicrafts, and the later Heidegger, who was dominated by an equally romantic antipathy toward modern technology. As for Heidegger's categorical denial quoted by Wolf, a historian is unwise to accept without question anyone's denials, especially a philosopher's, and most especially Heidegger's. So, to take an example whose pertinence will appear in Section II, Heidegger anathematized *Lebensphilosophie*, but, as Krell, *Daimon Life*, and Wolf himself, *Hermeneutik und Technik*, have shown, he was emphatically within that tradition. More generally, from the romantic tradition, and it alone, came Heidegger's conviction that in poetizing he was philosophizing. E.g. Schaeffer, *Art of the Modern Age*.

[39] The romantic antipathy to technology was not nearly so strong, or at least so uniform, in the British–American context, but tended to approximate to the German attitude as the 19th century advanced. A text of huge importance in that tradition, creating the concept of 'the age of machinery' along with its romantic indictment, was Thomas Carlyle, 'Signs of the Times', published in 1829—though Carlyle himself was quite ambivalent, for modern industry was also proof of the prowess of the British. See Sussman, *Victorians and the Machine*.

[40] Gode-von Aesch, *Natural Science*, 24–31, is both description and expression of the romantic antagonism toward technology and, therewith, forms of science aiming at mastery of nature. See, also, note 25.

[41] In this regard, as my only half ironic epigraph suggests, postmodernism was right about postmodernity. So, for example, Lecourt, *The Mediocracy*, 72–3, saw Lyotard and the postmodernists generally as 'technophiliac'. Similarly Kroker, *The Possessed Individual*, 1: 'Read the French, therefore, to learn a language for thinking anew the empire of technology'. Thus it seems to me that Ezrahi *et al.*, *Technology, Pessimism and Postmodernism*, have the case just backwards in linking postmodernism with technological *pessimism*.

[42] Robert Frosch, physicist, sometime head of NASA, in Frosch, 'The Notes', reviewing the two *Golem* books, said 'I am in the peculiar position of agreeing with the message of the golem books, often disagreeing with how it is said, and thinking that it is dangerously incomplete'. Thus Frosch took no notice of, let alone issue with, Collins and Pinch's conflation of science and technology, himself writing of 'processes of science/technology' without commenting on the union. Historians of technology are generally leery of defining technology, just as historians of science are leery of defining science, but for opposite reasons: historians of technology are afraid of including too much, historians of science of including too little. A satisfactory definition of technology is easily formulated, but would include far too much for a discipline that is preoccupied with its own coherence, as the history of technology has been. Though a satisfactory definition of science is not to be found, science is inseparable from working definitions of science, as Gieryn, *Cultural Boundaries*, has steadily emphasized: science is

constituted by an ongoing process of drawing boundaries between what is and what is not included in science, or in a particular science.

[43] De Gennes, *Soft Interfaces*, 104–5.

[44] Cech as quoted by Blackman, 'The Right Research Mix'. Similarly, Kaiser, *Science*: 'Janelia Farm director Gerald Rubin says he wants to recreate the close-knit feeling of legendary labs such as the Laboratory of Molecular Biology in Cambridge, UK, where well-funded investigators free of grant-seeking pressures work in small groups. There will be at least one difference: Janelia will emphasize technology.' There is also Wade, 'New Hughes Haven', who, as usual, distorts as he reports.

[45] Alan Leshner, the then new CEO, addressing the Council of the AAAS, 17 February 2002, on the revised statement of the mission of the Association ('Advancing science and promoting innovation …'), as recorded by me.

[46] Joseph Henry, in his retiring address as the Association's second president, 1850, admonished its members to 'Hold stubbornly to the essentials of our faith in science. Avoid clashes but give not an inch', promising them that 'Ours is the future'. (And for a hundred years it was.) Reingold, 'Joseph Henry', 159, 167.

[47] Weintraub *et al.*, 'Through the Glass Lightly'. Not one of the biological scientists among the 45 contributors pointed to other than technologically defined goals. Indicative in this regard is the study by Klevorick *et al.*, 'On the Sources', Tables 1 and 3, showing that as early as the mid 1980s—i.e. before molecular biology had become a leading factor in the technical and commercial orientation of biology—academic biological research was far less like that in any other academic science, far more like that of academic engineering research, in the degree to which industrial R&D directors rated it as important to their own interests—ten times more likely to be so rated than academic *physics* research! By the early 1970s, Erwin Chargaff, *Heraclitean Fire*, saw this coming. Indeed, Peter Medawar was advocating it as early as the 1960s: Medawar, 'Two Conceptions of Science', 38–9.

[48] For example, Greeley and Hout, 'Americans' Increasing Belief in Life after Death'; Walter and Waterhouse, 'A Very Private Belief: Reincarnation in Contemporary England'; Kaminer, *Sleeping with Extra-terrestrials*; Fogel, *The Fourth Great Awakening*; Frank, *One Market Under God*; Rifkin, *The Age of Access*; Schiller, *Irrational Exuberance*; Fuller, *Spiritual*; Jenkins, *The Next Christendom*; Clark, *From Angels to Aliens*; Forman, 'From the Social to the Moral to the Spiritual'.

[49] More particularly, it seems to me unwarranted to construe the development of the enormous and enormously complex multi-mode particle detectors used in conjunction with the highest energy accelerators as *postmodern* manifestations, as did Galison, *Image and Logic*, 553–5, 684–5, and 'Three Laboratories'. Nothing, on Galison's showing, compels us to regard the conception of such 'hybrid' instruments—'hybrid' only if one accepts Galison's themata of detector types, and invests those themata with archetypical powers—as other than obvious responses to the exigencies of observing just those special sorts of processes that the high-energy accelerators were being built to produce. (It is so much easier to impose upon the development of particle detectors stylistic analogies to postmodern architecture if one ignores, as Galison did, the development of particle accelerators.)

[50] Galison, 'Three Laboratories', 1127, 1148, keeping postmodernization dammed back in the innocuous realm of style, declined to see any loss of scientific identity in postmodernity, insisting on 'a picture in which scientific and engineering cultures are treated on a par, but not in any way homogenized'. A more up-to-date statement of this inoffensive postmodernism is Johnson, 'Revisiting Technology as Knowledge': 'Technology is neither subsumed under science nor completely outside of it; communities of technologists and scientists constitute interdependent, parallel, epistemologically equivalent bodies, what Layton calls "Mirror Image Twins." These views, while perhaps radical in the 1970s, have come to be accepted without argument in the twenty-first century.' (Johnson's misrepresentation of Layton's thesis is the common one. See notes 346–8, below.) If, however, I am anywhere near right that

in the postmodern laboratory technology subsumes science, no such separation is sustainable—and all the pressure to merge is exerted upon the scientists. In this connection, it is interesting to see that there, in the high-energy particle-accelerator laboratories, where a generation earlier just such a separation between the scientific and engineering 'cultures' had arisen as a modernist functional differentiation between the (higher status) experimental physicist and the (lower status) accelerator designing, building, and operating physicist, now, today, the experimental physicists are seeking to repossess the technologic role, seeing a value in the definition of themselves as inventors that simply did not exist for them 30 years ago: Tigner, 'Does Accelerator-based'.

[51] Cao and Schweber, 'The Conceptual Foundations', 71.

[52] See Braun and Diospatonyi, 'A Simplistic Approach'. I am grateful to Professor Braun for communicating these results to me prior to their publication. Kwa, 'Programming ', 457 *et passim*, draws attention to another characteristic form of postmodern science in which 'disciplinary "little science" structures seemingly continue to exist', even as the work of those little scientists is continually being reprogrammed by science policy officials steering their '"distributed megascience"'.

[53] Baird *et al.*, *Discovering the Nanoscale*; McCray, 'Will Small Be Beautiful?'. There is already a superabundance of 'science studies' literature on nanotechnology, and a tsunami in the offing, for one aspect of the US National Nanotechnology Initiative's emulation of the Human Genome Project has been the reproduction of the HGP's set-aside of a small percentage of its budget for studies of the social and ethical import of its research program. Characteristic is that even the most insightful critical examinations of nanotechnology—for example, Milburn 'Nanotechnology'—elide completely the distinction between science and technology and use the terms interchangeably.

[54] 'This Month in Physics History'. See also the quotation of Nicholas Metropolis in note 13.

[55] Again, the contrast with the founding purposes of this turn of the 20th century institution could hardly be greater. Thus, Henry A. Rowland's address as first president of the society, Rowland, 'The Highest Aim', 826: 'Above all, let us cultivate the idea of the dignity of our pursuit so that this feeling may sustain us in the midst of a world which gives its highest praise, not to the investigation in the pure ethereal physics which our Society is formed to cultivate, but to one who uses it for satisfying the physical rather than the intellectual needs of mankind'.

[56] For example, Taylor, *Laser: The Inventor, the Nobel Laureate, and the Thirty-Year Patent War*, which takes as protagonist a lotsa living, laying, and liquor physicist ('the inventor') who quits his failed scientific career to devote his efforts to an entrepreneurial patent suit, hoping to make a fortune out of his early anticipation of how a laser might work. 'The inventor' finally wins big, overturning in the courts the claims of the patent holder—'the Nobel Laureate'—a physicist of the highest distinction, both in his discipline and in public service, and a paragon of Protestant ethics.

[57] Gross and Levitt, *Higher Superstition*, 48–9. The emphasis is Gross and Levitt's. In their discussion (57–60) of Latour's *Science in Action*, no notice is taken of, let alone exception to, Latour's conflation of science and technology. Similarly, the very title of the follow-up book, Levitt, *Prometheus Bedeviled*, bespeaks conflation of science with technology—surely unconsciously, for Levitt writes (104) that 'Some critics [of science] have taken to using terms like "technoscience" to signal their commitment to the idea that science and technology not only move in lockstep, but are merely different names for the same thing'.

[58] Rorty, 'An Antirepresentationalist View', 125, 128. As Rorty points out elsewhere (Saatkamp, *Rorty & Pragmatism*, 211, n. 6), he there remains in the most authentic tradition of pragmatism: 'In a famous footnote to *Pragmatism*, James quotes [physicist] W.S. Franklin as saying that "the healtiest notion" of physics is as "the science of the ways of taking hold of bodies and pushing them."' But Rorty the postmodern, by taking the value of any enterprise as a function only of the moral intentions of those engaged in it, and setting science at zero (its practitioners intending neither good nor ill), makes most of technology rank above all of science.

[59] So, for example, physicist Srdjan Lelas, *Science and Modernity*, 274, professedly intending to 'provide a powerful basis for our belief in the reliability and truthfulness of science', finds the basis for that belief, as well the means for achieving the believed-in knowledge, 'physical interaction with nature by way of technology and its kin—experiment'. Similarly, Lelas, 'Science as Technology'.

[60] Mirowski, 'The Scientific Dimensions', 311, and 'Caveat Emptor' presenting his concept of 'the global privatization regime' with inception about 1980, cited publications by half a dozen other scholars locating a restructuring of knowledge production at about that date. To these I would adjoin Kwa, 'Interdisciplinarity' and 'Programming' and Pestre, *Science*, 98–104, 144–50, 154–5, where the number of aspects of the restructuring circa 1980 is so great as would warrant labeling as a new historical epoch.

[61] Mumford, *Technics and Civilization*, 52. Or, without the ellipsis, 'Technics is a translation into appropriate, practical forms of the theoretic truths, implicit or formulated, anticipated or discovered, of science'.

[62] Dunn, 'The Relationship'. The '*temple* of science in Washington' to which Dunn refers is the then recently constructed building housing the National Academy of Sciences. Http://www4.nationalacademies.org/nas/nashome.nsf/(leftnav)/AboutTheNAS_NASBuilding?OpenDocument (accessed 17 March 2005).

[63] Mayr, 'The Science–Technology Relationship', 671. And in so saying Mayr admonished his fellow historians of technology to 'Consider how they'—science and technology—'have risen and fallen, both absolutely and relative to each other', from century to century. Hevly, 'Afterword', 358, similarly stressed that the alleged relation between science and technology is to be taken as an ideology that 'itself needs to be understood historically, rather than accepted as historical explanation'.

[64] Dunn, 'The Relationship'. In 1912 Dunn had told the members of the American Institute of Electrical Engineers, as its president, that 'Engineering is Science's handmaid following after her in honor and affection, but doing the practical chores of life'. As quoted by Kline, 'Construing "Technology"', 204. See my discussion of 'Sons of Martha' at note 163, below.

[65] Wien, 'Aufruf zum Beitritt'. Forman, 'The Helmholtz Gesellschaft', Appendix. Wien's reference is to 'Hüttenkunde, Maschinenbau für alle Industriezweige und Elektrotechnik' and 'wissenschaftliche Führung'. This parole was intended for industrial circles only. Wien would have said the same to any audience until just a couple years earlier, but the romantic reaction following Germany's defeat in the First World War made it more than unwise to emphasize before an academic audience physics' close connection with technology. Forman, 'Weimar Culture', 40–4, 49, 55. Regarding romanticism's reprehension of the physics–technology connection, see the discussions of Heidegger, above, and of the Erlangen School, below.

[66] Thus Georg Klingenberg, one of the principal lieutenants to AEG chief Felix Deutsch, wrote his boss, who had accepted the position of treasurer of the Helmholtz-Gesellschaft, a scathing critique so soon as the documents of the founding meeting came to hand: 'I can't possibly assume that the expression "applied physics" is supposed to include technology [*Technik*] proper, since the expression "applied physics" is just about the falsest and most erroneous designation which can possibly be found for technology [*Technik*]'. The original German is quoted in Forman, 'The Helmholtz Gesellschaft', 115, from letter in Werksarchiv, Farbenfabriken Bayer AG, Leverkusen, signature 46/8. Klingenberg's protest was to no effect—in part, it must be said, because the industrial leaders were at this juncture much interested in making political alliances with the leaders of science and scholarship: Forman, 'The Financial Support'.

[67] Pestre, 'The Moral and Political', 245–6, introducing several articles on Marie Curie, underscored 'the leading role she attributed to scientists themselves. Science being the mother of progress—and scientists being by function disinterested—they had to play the dominant role and be leaders'.

[68] Exemplary for the modernist adulation of abstraction (the epitome of the theoretical) is Whitehead, *Science in the Modern World*, 25, 31–2, and all of ch. 10—the more indicative for the wide range of politico-epistemic positions from which this book was praised when published in 1925 and in following decades. Such adulation of abstraction in modernity did not prevent, but only trumped, high regard for the material and practical, as Whitehead himself testified at other points in this book, e.g. the passage in the fourth paragraph of the seventh chapter underscoring the centrality of instruments in the progress of science, quoted below, note 120, as quoted by Dewey.

[69] Gillispie, 'The Natural History of Industry' (1957), took this stance 50 years ago in one of the first historical studies to examine, without prejudice, the relation between science and technological advance. More generally, historians of science pointed out the preposterousness of the traditional assertions of the primacy of science relative to technology well before historians of technology found it important to do so. Thus as late as 1973, Molella and Reingold, 'Theorists', 129 (1973), 139 (1991), attacked Gillispie and likeminded historians of science for surrendering science's claims to primacy, 'usually [with] no evidence whatsoever'.

[70] 'Desto prakischer hat die Naturwissenschaft vermittelst der Industrie in das menschliche Leben eingegriffen und es umgestaltet'. As quoted by Habermas, *Erkenntnis*, from MEGA [Marx-Engels, *Gesamtausgabe*], I, 3: 122; Habermas, *Knowledge and Human Interests*, 45 (found under Habermas, *Erkenntnis*). Marx was certain that science was the highest of all possible cultural forms, and consequently his own highest aspiration was to be a scientist, like, but more completely than, any natural scientist. Habermas, *Erkenntnis*, 62–3, 66–8 (*Knowledge and Human Interests*, 45–6, 50), pointed out this aspect of Marx's thought, but found it incomprehensible.

[71] Marx, *Grundrisse*, 594 (*Grundrisse: Foundations*, 706).

[72] *Ibid*., 587–8 (700), and again, 591 (704): 'Es ist … direkt aus der Wissenschaft entspringende Analyse and Anwendungen mechanischer und chemischer Gesetze, welche die Maschine befähigt dieselbe Arbeit zu verrichten, die früher der Arbeiter verrichtete'. While this insistence that (only) fully developed capitalism is to be equated with technology as applied science ought to have led Marx to give some attention to the question of the sources of technological advance in earlier periods, only here in this one passage in *Grundrisse* does he do so: 'the development of machinery enters upon this path only as and when large-scale industry [grosse Industrie] has reached a higher stage and all the sciences have been caught up and pressed into the service of capital. … But this is not the road by which machinery, by and large, arose, and even less the road by which it progresses in detail'. That road is 'the division of labor, which gradually transforms the workers' operations into more and more mechanical ones' so as then to be replaceable by mechanisms. Evident here in the anti-mechanical animus is the romantic anti-mechanism that would trap Marx into regarding the machine as a mechanized hand tool. See, also, note 372, below.

[73] What Marx took to be true of the machine technology of his time, Braverman, writing in 1974, made the result of the more nearly science-based technologies of his time: 'The key innovation is not to be found in chemistry, electronics, automatic machinery … or any of the products of these science-technologies, but rather in the transformation of science itself into capital'. As quoted by Noble, *America by Design*, 6. Similarly, Dickson, *The New Politics of Science* (1984), 6: 'decision making over science—the key to the development of the forces of production'. Likewise, Schmidt, *The Concept of Nature in Marx* (1971), where 'science' stands so completely both for itself and for technology that 'technology' is never mentioned.

[74] Müller, 'Materialismus', xli–lxii. Marx, *Die technologisch-historischen Exzerpte*. Frison, 'Technical', 305 *et passim*, has argued that Marx took from Poppe the cameralist concept of *Technologie* as the science that systematized artisanal knowledge, and that Marx employed this perspective in conceptualizing modern industry. It seems to me, however, that, *contra* Frison,

Marx's use of *Technologie* in the cameralist sense of the science of artefaction is exceptional in *Das Kapital*, where the word is used more often as a synonym for *Technik*. So, for example, in the following passage Marx continues from one of his rare cameralistic uses of *Technologie* in the first sentence ('the modern science of technology') to an equation of *Technologie* with the 'application of natural science' in the immediately following sentence: 'Ihr Prinzip [i.e. that of 'Die große Industrie'], jeden Produktionsprozeß, an und für sich und zunächst ohne alle Rücksicht auf die menschliche Hand, in seine konstituierenden Elemente aufzulösen, schuf die ganz moderne Wissenschaft der Technologie. Die buntscheckigen, scheinbar zusammenhangslosen und verknöcherten Gestalten des gesellschaftlichen Produktionsprozesses lösten sich auf in bewußt planmäßige und je nach dem bezweckten Nutzeffekt systematisch besonderte Anwendungen der Naturwissenschaft'. Marx, *Das Kapital*, in Marx-Engels, *Werke*, 23: 510, available at http://www.mlwerke.de/me/me23/me23_483.htm, (accessed 23 June 2006). Marx, *Capital*, 1: 616-7. Recently, Schatzberg, '*Technik* Comes to America', 494—who kindly communicated this paper to me prior to its appearance—accepting and extending Frison's contention, has alleged that in Germany during the second half of the 19th century '*Technik* and *Technologie* were the focus of independent discourses and almost never compared'. I can readily imagine that '*Technik* and *Technologie* were … almost never compared', but only because, as Marx's usage implies, they were *not* 'the focus of independent discourses'.

[75] Marx maintained into his last years his preoccupation with the merely conceptual: *Mathematical Manuscripts of Karl Marx* (1983). There is, of course, Engels' oft-quoted private letter disparaging the notion of science's primacy for technology: 'If technology is chiefly dependent on the extent to which science is advanced, as you say, then [I must insist that] the latter is far more dependent on the degree to which technology has advanced as well as its requirements. If society has a need for a certain technology, then this does more to advance science than can ten universities' (quoted, in translation, from Marx–Engels, *Werke*, 39:205, by Rürup, 'Historians', 186; also by Jamison, 'Technology's Theorists', 514). Against this unique spontaneous expression is Engels' public declaration, in which he claimed to speak for Marx as well, that 'knowledge of mathematics and natural science is essential to a conception of nature which is dialectical and at the same time materialist', and in consequence of that conviction Engels too, on retirement from business, turned his attention immediately and for years to mathematics, physics, and chemistry. See the preface written in 1885 to Engels, *Herr Eugen Dühring's*, 15.

[76] Marx, *Das Kapital*, 'Erstes Kapitel: Die Ware', in Marx–Engels, *Werke*, 23: 54: 'Die Produktivkraft der Arbeit ist durch mannigfache Umstände bestimmt, unter anderen durch den Durchschnittsgrad des Geschickes der Arbeiter, die Entwicklungsstufe der Wissenschaft und ihrer technologischen Anwendbarkeit, die gesellschaftliche Kombination des Produktionsprozesses, den Umfang und die Wirkungsfähigkeit der Produktionsprozesses, und durch Naturverhältnisse'. Available at http://www.mlwerke.de/me/me23/me23_049.htm#Kap_1_1 (accessed 23 June 2006). Marx, *Capital*, 1: 130: 'the productivity of labour.…is determined by…the workers' average degree of skill, the level of development of science and its technological application….'

[77] Marx, *Das Kapital*, 'Dreizehntes Kapitel: Maschinerie und große Industrie', in ibid., 486: 'Das Prinzip des Maschinenbetriebs, den Produktionsprozeß in seine konstituierenden Phasen zu analysieren und die so gegebnen Probleme durch Anwendung der Mechanik, Chemie usw., kurz der Naturwissenschaften zu lösen, wird überall bestimmend'. Available at http://www.mlwerke.de/me/me23/me23_483.htm (accessed 23 June 2006). Marx, *Capital*, 1: 590: 'The principle of machine production, namely the division of the production process into its constituent phases, and solution of the problems arising from this by the application of mechanics, chemistry and the whole range of the natural sciences, now plays the determining role everywhere.' Similarly, *ibid*., 407: 'Ersetzung der Menschenkraft durch Naturkräfte und erfahrungsmäßiger Routine durch bewußte Anwendung der Naturwissenschaft'. Available at

http://www.mlwerke.de/me/me23/me23_391.htm (accessed 23 June 2006). Marx, *Capital*, 1: 508: 'the replacement of human force by natural forces, and the replacement of the rule of thumb by the conscious application of natural science.'

[78] *Ibid*., 527: 'In der Sphäre der Agrikultur wirkt die große Industrie insofern am revolutionärsten, als sie das Bollwerk der alten Gesellschaft vernichtet … . An die Stelle des gewohnheitsfaulsten und irrationellsten Betriebs tritt bewußte, technologische Anwendung der Wissenschaft'. Available at http://www.mlwerke.de/me/me23/me23_483.htm#Kap_13_10 (accessed 23 June 2006). Marx, *Capital*, 1: 637: 'A conscious, technological application of science replaces the previous highly irrational and slothfully traditional way of working.'

[79] Marx—exactly like Ure, whom he quotes as leading apologist for machine production, and whose views on the proletariat Marx therefore exhibits to despise—is so firmly of the view that technology is applied science that he finds no contradiction, no calling of this presumption into question, in the fact that, 'Die Wissenschaft kostet dem Kapitalisten überhaupt "nichts," was ihn durchaus nicht hindert, sie zu exploitieren. … Dr. Ure selbst bejammerte die grobe Unbekanntschaft seiner lieben, Maschinen exploitierenden Fabrikanten mit der Mechanik, und Liebig weiß von der haarsträubenden Unwissenheit der englischen chemischen Fabrikanten in der Chemie zu erzählen'. *Ibid*., 407, note 108. Available at http://www.mlwerke.de/me/me23/me23_391.htm#M108 (accessed 23 June 2006). Marx, *Capital*, 1: 508-9, note 23. Rosenberg, 'Karl Marx on the Economic Role of Science', 126, complaining that 'Many of the most interesting aspects of Marx's treatment of technological change have been ignored, perhaps because of the strong polemical orientation which readers from all shades of the political spectrum seem to bring to their reading of Marx', drew attention to none of the above quoted passages. Rather, Rosenberg himself polemicized for the perfect correctness of Marx's (correctly understood) views on the relation between science and technology/industry, without even bothering to distinguish between what Marx wrote and what Engels wrote. That Rosenberg 'argues that Karl Marx should be the foundation of all thinking in the history of technology' was noted also by Roland, 'What Hath Kranzberg Wrought?', 705, citing *Inside the Black Box*, 34.

[80] For Veblen, even more the romantic than Marx, historical materialism was even less a matter of belief or preference: it was a working hypothesis, the obligatory working hypothesis for a scientist. Veblen had not only more concerns in common with John Ruskin and William Morris than he cared to admit, but even also values. So, e.g. Veblen, *The Theory of the Leisure Class*, 162, and 'Arts and Crafts'. Diggins, *The Bard of Savagery*, emphasized the romantic Veblen with that title phrase taken from Perry Miller's discussion of Veblen. For the enormous influence of Ruskin in Veblen's milieu, see Stein, *John Ruskin*.

[81] Veblen, 'The Preconceptions', 143; 'The Place of Science', 598; *The Higher Learning*, 6. As the quotations suggest, Veblen's use of the noun 'technology' as well as the adjective 'technological' was quite as loose as Marx's, i.e. used sometimes in the cameralist sense of a science of productive activities, but more often used to designate the productive activities themselves. To repeat re: Veblen what I have already said re: Marx in note 74, above, I can see no basis for the claims in Schatzberg, '*Technik* comes to America', 504–5, that Veblen employed a specific and sophisticated concept of technology. Still less can I see as empirical Schatzberg's contention there that 'Veblen's analysis of the *science–technology* relationship was possible only because of his *Technik*-centered definition of *technology*. There were no discussions of this relationship in the nineteenth century, when *technology* was clearly understood as a field of science and the relationship was simply that of part to whole'. The articulation of so insupportable a proposition would not, I think, be possible but for the hegemony of an anti-science ideology in the discipline of the history of technology.

[82] Veblen, 'The Place of Science'; Veblen, 'The Evolution'. The second of these papers elaborates and qualifies the historical materialist thesis that is advanced, still quite summarily, in the first—the broader, more balanced essay.

[83] Veblen, *The Instinct of Workmanship*, chs 6 and 7. A similar but briefer discussion is Veblen, *Imperial Germany*, 110–20, where Veblen refers the reader to *The Instinct of Workmanship* for fuller discussion.

[84] Veblen, 'The Place of Science', 585. This thesis, which we now commonly associate with Max Weber, was a commonplace at that time. Diggins, *The Bard of Savagery*, 114–18, compares Veblen and Weber.

[85] Quotations from Veblen, 'The Place of Science', 595, 608, 586, 608. This 'ubiquitous presence of the machine technology' is, however, characteristic only of the period since 'the so-called industrial revolution' (Veblen, 'The Evolution', 54). In the early-modern period, that being an era of handicraft, 'the concepts of the scientists came to be drawn in the image of the workman' (Veblen, 'The Place of Science', 595), but in the period since the early decades of the 19th century 'the technological ascendancy of the machine process brought a new and characteristic discipline into the cultural situation' (Veblen, 'The Evolution', 54). In this later phase, 'whether in technological work or in scientific inquiry … men have fallen into the habit of thinking in terms of process rather than in terms of the workmanlike efficiency of a given cause working to a given effect'. In physics and chemistry this produces 'a highly impersonal interpretation of phenomena in terms of consecutive change … with the disappearance or dissipation of all stable and efficient substances', i.e. Veblen offers a historical materialist interpretation in place of contemporary positivist polemics against the older, cause–effect conception of scientific explanation.

[86] Veblen, 'The Place of Science', 598. To be especially noted, for it is essential to Veblen's assignment of high cultural rank to science, is his insistence here that the scientist, as scientist, cannot aim at technological ends. Cf. the discussion of Dewey, below. In this connection notice should also be taken of an indicative inconsistency in Veblen's exposition of 'the reason why scientific theories can be turned to … technological purpose'. Having first given the quoted explanation for this 'applicability' of science—the fact that 'the canons of validity under whose guidance he [the scientist] works are those imposed by the modern technology'—Veblen, evidently uncomfortable with the consequences of the primacy he has attributed to practice, restates the issue in the next paragraph in such a way as to demote technology from primacy to parity: 'Modern technology makes use of the same range of concepts, thinks in the same terms, and applies the same tests of validity as modern science. … Hence the easy copartnership between the two. Science and technology play into one another's hands' (Veblen, 'The Place of Science', 598).

[87] Heidegger, 'The Question', 21–2.

[88] Veblen, *The Instinct*, 253. Veblen's discussion in *Imperial Germany*, 110–20, 268–70, involves the same inconsistencies. Andrew Jamison, 'American Anxieties', 83, quoted from *Imperial Germany*, 268: 'the most characteristic habit of thought that pervades this modern civilization, in high or low degree, is what has, in the simplest terms hitherto given it, been called the mechanistic conception. Its practical working-out is the machine technology, of which the intellectual precipitate and counterpart is the exact sciences'. Although Jamison exhibited this quotation as illustrative of the primacy that Veblen allegedly gave to technology, it exemplifies, rather, Veblen's vacillation between attributing primacy to technology ('precipitate') and attributing it to economics ('counterpart').

[89] Veblen, *Absentee Ownership*, 261, 255. The chapter, X, is titled 'The Technology of Physics and Chemistry'. Diggins, *Bard of Savagery*, 24–6, appraised this book as Veblen's 'most somber work, his final indictment'.

[90] Veblen, *Absentee Ownership*, 265–6.

[91] *Ibid.*, 259. It is an indication of how far Veblen had bought into the ideology of primacy of science that he cites Edwin Slosson, *Creative Chemistry* for evidence of this dependence of industry upon science. That Veblen had then only very recently accepted this view of the science–technology relationship is suggested by the fact that science is very nearly absent from Veblen, *The Engineers*; an isolated exception is the reference on p. 52 to 'the material sciences' as a resource upon which 'the industrial system of today' 'constantly draws'. Likewise,

although Veblen could refer in *Imperial Germany*, 198, to 'the range of applied science called the machine technology', that too remained an isolated exception.

[92] Veblen, 'The Place of Science', 587. The phrase, 'the increase and diffusion of knowledge among men', which Veblen quotes here (and he is quoted below quoting it again in 1918) will be recognized as the operative mandate in James Smithson's bequest establishing the Smithsonian Institution. The history of that institution over the past thirty years can be understood as manifold manifestations of the diminished power and allegiance of its modernist mandate as postmodern presuppositions have taken ever firmer hold.

[93] *Ibid*., concluding paragraph; James, *The Will to Believe*.

[94] That ambivalence made James an enthusiast for Bergson and rendered Veblen half sympathetic to Bergson: Veblen, *The Instinct*, 332–6.

[95] David Hollinger, 'Justification by Verification', 127, while recognizing the disinterested research scientist as a cultural ideal widely shared in the late 19th and early 20th centuries, excepted William James. Hollinger there described James as having 'parodied' the contemporary conception of science in referring to the edifice of science as having been built by and upon '"thousands of disinterested moral lives"' (quoting James from pp. 17–18 of the 1979 critical edition of *The Will to Believe*). Hollinger cited no evidence for imputing a parodic intent to James, a reading not consistent with the text, its context, or the alterations that James deliberated in preparing it for publication (pp. 405–6).

[96] Veblen, 'The Socialist Economics', 576. Other examples of Veblen's wholly positive usage of 'science' words: 'The Preconceptions'; 'Gustav Schmoller's'.

[97] Veblen, 'The Higher Learning', 6.

[98] *Ibid*., 46–7; Diggins, *The Bard of Savagery*, 180–1: 'Thus in *The Higher Learning* Veblen acknowledged what he had slighted in his previous works—the eminence of the academic calling even in a capitalist culture. … Even under the corrosive influence of business principles the ideals of scholarship could survive'.

[99] Veblen, *The Higher Learning*, 8; Diggins, *The Bard of Savagery*, 29–30, not grasping that what Veblen said is what everyone thought, was astonished and perplexed that Veblen 'attempted to turn utilitarianism on its head by claiming, against every canon of empiricism, that science progresses only to the extent that it serves no immediate useful purpose. … It was, perhaps, the residue of Kantianism in Veblen that led him to see science as practically liberating only so long as the mind remained theoretically free from the demands of practicality'.

[100] Veblen, *The Higher Learning*, 10–11.

[101] Proctor, *Value-free Science?*, 85–92, 122–9; but for the *lebensphilosophisch* Sombart, see Sieferle, *Die conservative Revolution*, 86: 'Der Kern seiner Theorie folgte … dem lebensphilosophischen Paradigma'.

[102] Hård, 'German Regulation', 56–60; Dietz *et al*., 'Der "Kulturwert der Technik"'. While historians of technology have seen in Sombart's 'Technik und Kultur' a celebration of the value of *Technik* for *Kultur*, that requires ignoring Sombart's expression there of quite antithetic views, as on pp. 340–1.

[103] 'Die moderne Technik … ist eine Zwillingsschwester der modernen Naturwissenschaft'. Sombart, *Der moderne Kapitalismus*, 78.

[104] *Ibid*., 79: It is 'eine müssige, ja falsche Frage (die ich selbst einst gestellt habe): welche von beiden genetisch die frühere sei, welche die andere erzeugt habe. Sie sind eben eins, und dadurch ist ihr Entwicklungsgang derselbe'.

[105] *Ibid*.: 'Wir können deshalb die Etappen der modernen Technik in grossen Zügen aus den Etappen der Ausbildung naturwissenschaftlicher Erkenntnis bestimmen'.

[106] *Ibid*., 81: '*Denkt* die Naturwissenschaft die Welt as Maschinismus oder Chemismus, so *schafft* die Technik künstlich eine Welt, die nach den von der Naurwissenschaft für das Weltganze aufgestellten Formeln abläuft'.

[107] Cohen, *Bukharin*, 353, et passim.

[108] There is, however, Joravsky's counter-thesis, developed to explain the extraordinary interventions by Soviet political leaders, beginning with Stalin, into scientific and scholarly questions and controversies—what Joravsky identified as 'the central mode of self-justification in the mentality of communist leaders. Since practice is the ultimate criterion of truth, and the political leaders of the historically progressive class are the supreme readers of the lessons of practice, they are the ultimate arbiters of truth. In short, one learns in many ways, but most of all by bossing. The higher up the boss, the greater his realm of practical mastery, the more his 'big truth' supersedes the 'little truths' perceived by the lesser creatures down below, scholars and scientists included. That was probably Stalin's most significant innovation in Marxist theory'. See Joravsky, 'The Stalinist Mentality', 583; also Joravsky, *The Lysenko Affair*. Yet I think it is not impossible to see an implicit attribution of primacy to science even also in this very compulsion for self-justification by the political leaders, Stalin in particular.

[109] Bukharin, 'The Methodology'. This event and speech are the subject of Graham, 'Bukharin', which is largely incorporated in Graham, *The Soviet Academy*. For all that Bukharin was in strong disagreement with Stalin on policies for Soviet economic development, nowhere in his discussion does Graham suggest that Bukharin's views on the primacy of science for technology could be other than consensual in the Soviet Union. Likewise, Andrews, *Science for the Masses: The Bolshevik State*, 154–6, in arguing that the Great Break at the end of the 1920s brought a new emphasis upon the application of science to industry and to the creation of new technology implicitly confirms that the presumption of primacy of science for technology remained unchanged.

[110] Bukharin, 'The Methodology'.

[111] *Ibid.*

[112] *Ibid.*, end of pt. 2. Bukharin's positions here in 1931 are not essentially different, but more strongly expressed than those taken ten years earlier in his book *Historical Materialism.* There, in ch. 6, Section g. 'The Significance of the Superstructure', Bukharin allowed that 'Thousands of examples prove that a true scholar, or artist, or theoretical jurist, loves his vocation as he loves himself, without regard to its practical phases. But … man's view of his labor is not identical with the role, the significance, of his labor for *society*. … Knowledge formerly served practice, even in men's minds; it still serves practice, but the minds of the closeted specialists represent knowledge as entirely divorced from practice. … the superstructure is not "child's play". We have shown that a destruction of the capitalist state would make capitalist production impossible, that a destruction of modern science would involve also that of large-scale production and technology; … Science likewise (let us suppose we are speaking of the natural sciences) ultimately serves as a guide for the process of production, increases its effectiveness and regulates its operation'.

[113] Bukharin, 'The Methodology', opening paragraph.

[114] *Ibid.*, end of pt. 2.

[115] Kojevnikov, *Stalin's Great Science.*

[116] White, *The Origin of Dewey's Instrumentalism*.

[117] Ihde, 'Editor's Foreword', viii. More: 'even before Wittgenstein and Heidegger, [Dewey] moved philosophy into the postmodern period'. Hickman himself has been a principal perpetrator of this misrepresentation of Dewey, foisting upon him a postmodern conflation of science and technology in which science loses its primacy to technology. See, Hickman, 'Pragmatism', 72–87; Hickman, *Philosophical Tools*.

[118] Dewey, *Reconstruction*, 12 (in Dewey, *The Middle Works*, 12: 86). Similarly, but rather stronger, Dewey, 'By Nature and by Art' (1944), as reprinted in Dewey, *The Later Works*, 15: 88: 'The scientific revolution may be said to have been initiated when investigators borrowed apparatus and processes from the industrial arts and used them as means of obtaining dependable scientific data'. It is likely that when writing these lines Dewey was aware of Edgar Zilsel's publications, specifically, 'The Sociological Roots of Science' (1942).

[119] Dewey, *Reconstruction*, 42, 126 (in Dewey, *The Middle Works*, 12: 103, 152).

[120] Dewey, *Logic*, as reprinted as Dewey, *The Later Works*, 12: 388. Dewey there gave, as illustrative, a list of instrumental advances in astronomy during the preceding half-century, and added a footnote: 'The following passage [from Whitehead, *Science in the Modern World*] is worth citation as one of the comparatively few instances of recognition, from the side of theory, of the importance of this point: 'The reason why we are on a higher imaginative level today [in science] is not that we have finer imagination, but because we have better instruments. In science, the most important thing that has happened in the last forty years is the advance in instrumental design. … These instruments have put thought on to a new level'. [I have corrected some small inaccuracies in Dewey's quotation from the fourth paragraph of the seventh chapter.]

[121] Dewey, *The Quest for Certainty*, 84 (in Dewey, *The Later Works*, 4: 68).

[122] Dewey, 'Science and Society', here quoted from Dewey, *The Later Works*, 6: 57. So far as I have seen, Dewey's strongest and most fully theorized assertion that modern science is 'a mode of technology' is in 'By Nature and by Art', where (89, note 3) he credits the Veblenist economist Clarence E. Ayres with being the first explicitly to say this. Dewey founds this contention on the one hand on the historical fact of science originating in artisanal knowledge, and on the other hand on an extremely broad definition of art, broader even than that employed by cultural anthropologists, namely as including the responses of even 'low-grade organisms' to external stimuli (88). From this definition of art it follows necessarily, but vacuously, that science too is art. Dewey then proposes to use the word 'technology' for 'the art which is science', thus making science not 'a mode of technology', but tautologously technology—and, of course, technology science.

[123] Dewey, *Reconstruction*, 12–13, 110–11, 170–1 (in Dewey, *The Middle Works*, 12: 86–7, 142–4, 177–8).

[124] The opposite is commonly asserted today. So, Depew, 'Pragmatists', 11: 'Dewey assigns *techne* primacy over *praxis*, and *praxis* over *theoria*'; Rorty, 'Philosophy as Science', 13, as quoted by Nevo, 'Richard Rorty's', 285: 'pragmatists such as Dewey turn away from the theoretical scientist to the engineers and the social workers'. In a somewhat softer form by Westbrook, 'Lewis Mumford', 311: 'Dewey idiosyncratically (for his time if not ours) considered science to be, above all, practical reason, and he regarded the abstraction of modern natural science to be a methodological move made in the interest of gaining control over natural processes'.

[125] Dewey, *Reconstruction*, 168–9, xl (in Dewey, *The Middle Works*, 12: 176, 276). Note that the first quotation is from what Dewey wrote in 1920, while the second quotation is from the long introduction that Dewey wrote in 1948 for the republication of that book, i.e. note the constancy of this prejudice.

[126] 'Philosophers in setting experience down as inherently inferior to rational science were truthful', *Quest for Certainty*, 82 (in Dewey, *The Later Works*, 4: 66).

[127] Dewey, *Quest for Certainty*, 84–5 (in Dewey, *The Later Works*, 4: 68).

[128] *Ibid*. The passage continues with Dewey insisting that 'the technique of modern industry, in commerce, communication, transportation and all the appliances of light, heat and electricity, is the fruit of the modern application of science'.

[129] So, with increasing pejorativeness, 'the privileged pecuniary classes', Dewey, *The Middle Works*, 10: 253; 'our pecuniary oligarchy', Dewey, *The Later Works*, 5: 67; 'publicity agents of the dominant pecuniary group', Dewey, *The Later Works*, 8: 63.

[130] See, Dewey, *Reconstruction*, 41 (in Dewey, *The Middle Works*, 12: 102); 'Philosophy', 323–4 (in Dewey, *The Later Works*, 6: 57); *The Later Works*, 11: 143, 311–12.

[131] Dewey, 'Needed—A New Politics', as reprinted in *The Later Works*, 11:274–81, on 279.

[132] Dewey, *Reconstruction*, 42, 145–8, 156 (quotation; in Dewey, *The Middle Works*, 12: 170). Dewey's vehemence here shows his awareness that reconciling his instrumentalism and his deprecation of individualistic motivations was problematic.

[133] The point is made quite briefly by Habermas in opening his 1965 inaugural lecture at Frankfurt University (Habermas, 'Erkenntnis und Interesse').

[134] And this requires Dewey to walk a rather narrow line—though in fact he rarely notices the necessity—for it follows from his principles that 'What is sometimes termed "applied" science, may then be more truly science than is what is conventionally called pure science. For it is directly concerned with not just instrumentalities, but instrumentalities at work in effecting modifications of existence in behalf of conclusions that are reflectively preferred. … Thus conceived, knowledge exists in engineering, medicine and the social arts more adequately than it does in mathematics, and physics'. Dewey, *Experience*, as reprinted in Dewey, *The Later Works*, 1: 128; Jordan, *Machine-Age Ideology*, 228, exhibits this quotation and asserts that 'This conception remains notably consistent throughout Dewey's writings of the 1920s and 1930s', for 'As a pragmatist, Dewey embraced a scientific ideal at odds with the purity of experimental investigation as an end in itself. … his version of science mirrored engineering, not merely inquiry'; but Jordan is wrong, both about Dewey's ideal and Dewey's consistency.

[135] Dewey, 'Science and Society', here quoted from *The Later Works*, 6: 57–8. Dewey said the same more concretely and less concisely two decades earlier: 'The modern warship … could not exist were it not for science: mathematics, mechanics, chemistry, electricity supply the technique of its construction and management. But the aims, the ideals in whose service this marvelous technique is displayed are survivals of a pre-scientific age, that is, of barbarism'. Dewey, 'Science as Subject-Matter', 127. Dewey is, I suppose, influenced by Veblen here. See note 58, above, for Rorty's antithetic position as *postmodern* pragmatist.

[136] Dewey, 'Authority and Social Change', 188 (in Ratner, *Intelligence in the Modern World*, 359, and in Dewey, *The Later Works*, 11: 142–3).

[137] An early instance of the same presupposition of the incompatibility of pecuniary interest with behaving scientifically (and morally): Dewey, 'Logical Conditions' (1903), as reprinted in Dewey, *The Middle Works*, 3: 19.

[138] Dewey, 'Introduction: Reconstruction', xxviii (in Dewey, *The Middle Works*, 12: 269).

[139] How hard it is for an intellectual historian today—especially today—to recognize and allow the primacy that Dewey ascribed to science is shown by the unwillingness of Dewey's best biographer to do so: Westbrook, 'Lewis Mumford', 321, 311, otherwise entirely on Dewey's side in his clash with Mumford in 1927, is unable to accept Dewey's highly affirmative view of science. There and only there he declares Dewey wrong and Mumford right.

[140] Dewey, 'The Pragmatic Acquiescence' (1927), concluding lines. Dewey's title is that of ch. 4 of Mumford's *The Golden Day*, which work, then recently published, contained harsh indictments of pragmatism—to be considered below, at notes 230–34, in examining Mumford's view of the science–technology relation. The traitorousness of Mumford's attack was institutional as well as intellectual: Mumford had been a junior editor on *The Dial* in 1919 when this new journal, with Dewey at the head of its editorial board, was at the center of pragmatism-inspired theorizing of social reconstruction following The Great War. Dewey and Mumford were both regular contributors in the following years to *The New Republic*, a magazine that likewise carried the reputation then, and among historians since, as inspired by pragmatism.

[141] Dewey, *Reconstruction in Philosophy*, 41 (in *The Middle Works*, 12: 102). The emphasis is Dewey's.

[142] Dewey, 'Authority and Social Change', 188 (in Ratner, *Dewey's Philosophy*, 360 and Dewey, *The Later Works*, 11: 143). And elsewhere at about this time, Dewey, *The Later Works*, 11: 182, 267, 311, 364: 'all the physical features of the present regime of production and distribution of goods and services are products of the new physical science'; 'modern industry in production and distribution of goods is the direct product of science'; 'these mechanical inventions are, of course, the product of scientific discovery'; 'the cause of the release of productive energies was the rise of experimental science and its technological application'.

[143] Dewey, 'Philosophy', as reprinted in Dewey, *The Later Works*, 3: 115–32, on 118.

[144] Dewey, *The Quest for Certainty*, 80 (in *The Later Works*, 4: 64). This fact, Dewey explained, was 'The practical reason for selecting such a technical matter as the method of physical science' as the main topic of his Gifford Lectures.

[145] This 'inconsequence' of Dewey as instrumentalist is the more noteworthy as he was in other respects remarkably consequent in his instrumentalism. Thus Howard, 'Two Left Turns', at note 38, has pointed out that Dewey was a more consistent naturalist than were the logical positivist/empiricists who refused science a role in the construction or selection of ends. More pertinent to the present issue, Dewey was so consistent a naturalist as to affirm that mere knowledge—i.e. thought as the act of knowing—not only can, but must, have physical consequences. (Howard, *ibid.*, 2nd para. before note 31, citing Dewey, *The Quest for Certainty*, 211 (in *The Later Works*, 4: 195).) This axiom that knowledge alters the real world became then the metaphysical base for the Copenhagen interpretation of quantum mechanics, Bohr having been strongly influenced by Dewey. Einstein, as is well known, opposed this axiom, and so also did Karl Popper. Indeed, as pointed out by Shields, 'Karl Popper's', 'the issue that virtually obsessed Popper [was] does human knowledge have physical effects?' If one bears in mind that to insist that mere knowing has physical consequences is to negate the possibility of any distinction in principle between science and technology—as also, as more commonly noted, between science and mysticism—it is easy to understand why this issue had to be important for Popper, whose constant concern was the demarcation of science from non-science. Cf., note 42. Yet, impressively consistent though Dewey was in regard to this matter of naturalism, as in his instrumentalism, it remained beyond the limits imposed by modernity for him to apply those principles to the relation between science and technology.

[146] Adas, *Machines … and Ideologies of Western Dominance*, took the primacy of science so much for granted—presumably because his non-Western subjects and sources, no less than his Western, did so also—that he did not find it necessary even to raise the question.

[147] Pestre, *Physique*, 285, quoting and endorsing Guerlac, 'Science and French', 491. In an important study Shinn, 'The French Science', 322–5, showed the strong influence of industrial interests in providing the funding and in setting the programs of French university science faculties in the late 19th and early 20th centuries. His exposition leaves open, however, the question whether this altogether surprising circumstance is evidence against the common view that the French scientist made the ideal of for-its-own-sake science 'very specially his own' (Guerlac), or, on the contrary, should be taken as supporting that view, i.e. supporting it to the extent—and it was, Shinn indicates, to a considerable extent—that French scientists were unenthusiastic about the influence of industrial interests.

[148] As quoted by Tessa Morris Suzuki, as quoted by Walter Grunden in Walker, *Science and Ideology*, 49.

[149] Kojevnikov, *Stalin's Great Science*, 2–5. An implicit indication of the Russian tradition of scientific purity is offered by Vucinich, *Science in Russian Culture* (1970), which, with an index containing more than 1500 entries has none for 'technology' or 'engineering'. Zvorikine, 'Technology and the Laws of Its Development' (1962), stated the official Soviet line at that time: 'The primary prerequisite for the conversion of science into a direct productive force is large-scale development of theoretical natural science'. By 1980 it was possible to say that 'An exaggerated respect for science as the motive force of change (capsuled in the phrase 'the scientific–technological revolution', or STR) has undermined the traditional Soviet concepts of what generates progress'. Valkenier, 'Development Issues', 497–8. However, as we have seen, this *was* the traditional Soviet concept of what generates progress.

[150] Kline, 'Construing "Technology"'. As Rose, 'Science as an Idiom' (1987), 4, had noted, 'the recent monographic literature suggests that scientists exercised only a modest influence within corporate and political arenas'. Thus, since 'the proposition that technology was identical with science appeared routinely in the assertions of opinion leaders', it should be taken as 'evidence of its existence as an artifact of culture among educated Americans'. In 1975,

Hughes, *Changing Attitudes*, 6, could still present evidence of this artifact without taking umbrage at it. Pursell, 'Engineering Organization' (2006), provides unintended and unwilling support: he portrays Dunn, Carty, and their peers, men of the widest experience of the world of affairs and with the closest connections with the wealthiest and most powerful in America, as allowing themselves to be bilked by scientists who exploited their inordinate admiration and respect for science. Noteworthy is the bit of evidence that Pursell brings that the rank-and-file engineer did not exalt science and scientists so greatly above himself as did the elite engineer speaking down to him.

[151] Kline, 'Construing "Technology"', 203–4.

[152] *Ibid*., 212. The parenthesis is Kline's.

[153] Carty, 'The Relation of Pure Science', 514. Conant, *Modern Science and Modern Man* (1952), 54–8—lectures containing the kernel of the views developed by his protégé Thomas S. Kuhn in the following decade—gives much space to a critique of this commonplace simile likening scientists and explorers. It is in this connection that Conant repudiates the conception of scientific constructs as successively better approximations to reality—'In short, the whole analogy between a map and a scientific theory is without a basis'. In lieu of the scientist as explorer, which was a version of the conception of the scientist as 'virtuous', Conant there advocates a conception of the (true) scientist as virtuoso, which conception was then in fact becoming the common self-conception of scientists. Forman, 'Social Niche and Self Image'.

[154] Carty, 'The Relation of Pure Science', 514.

[155] *Ibid*., 518. Kline, 'Construing "Technology"', 210, points to G. E. Hale as quoting Carty in two 1919 addresses.

[156] Jewett as quoted by Kline, *ibid*., 217.

[157] Kline, *ibid*., 211, where he represents 'the … pure-science ideal' as 'the NRC pure-science ideal'. ('NRC' = National Research Council, the activist instrument created for the US National Academy of Sciences in 1916.) Kline's effort so to restrict the cultural hold of that ideal is without foundation in his exposition or elsewhere.

[158] Kline, *ibid*., 194. T. H. Huxley, 'Science and Culture', 26. On the high standing of science—*pure* science—as high culture in late 19th century Britain: White, 'Ministers of Culture'; and in the USA, Kevles, *The Physicists*, 17.

[159] Kline, 'Construing "Technology"', 212.

[160] *Ibid*., 203.

[161] *Ibid*., .214, where, again, Kline wrote 'the NRC's pure-science ideal'.

[162] *Ibid*., 210; and again, 212, 'Engineering leaders who took a more moderate position [on engineering's subservience to science] often echoed Gano Dunn'. Cf., however, note 64, above.

[163] Kipling, 'Sons of Martha'.

[164] *Luke*, 10:38–42, for the incident, and *John*, 11:1–2, for the relation with Lazarus.

[165] Oldenziel, *Making Technology Masculine*, 125–31, 180. What, however, the poem says, and what engineers were affirming as self-image in their quotations of it, is scarcely discernible though Oldenziel's presentation. Rossiter, 'The Matthew Matilda Effect', 334–5, had already shown how strongly the image appealed to women engineers. Similarly, Dillon, 'Margaret Mead', 336, had emphasized how strongly the image appealed to Margaret Mead. Veblen himself quoted a couple stanzas in *Absentee Ownership*, 255.

[166] Invocations of the poem and its laudation of subservience have by no means disappeared: In 1964 the Professional Engineers Ontario (Canada) created 'the Sons of Martha Medal, to recognize outstanding contributions to the profession through the association': http://www.peo.on.ca/events/awards/OPEA/OPEA_piperarticle_celebrating.htm (accessed 25 June 2006). In 1995 the commencement speaker at the graduation of industrial engineers from North Carolina State University could still say: 'I am unaware of any comparable literary work in the English language that so honors any other profession. This poem is reprinted in the front of your graduation bulletin along with a Biblical quotation that explains the context of the poem': http://www.ie.ncsu.edu/jwilson/gradf95.html (accessed 25 June 2006). It is an

indication of the unwillingness of historians of technology to report the resonance that this poem sustained with the self-conception of American engineers that an electronic search of the entire run of *Technology and Culture* turns up not one article containing the phrase 'Sons of Martha'.

[167] Oldenziel, *Making Technology Masculine*, 128, considering that 'The import of the poem lay in its celebration and validation of the thankless, subjugating nature of hard physical labor', alleged that 'Kipling realized only later, the explicit, irreverent treatment in his poem of those who did not get their hands dirty—God and Mary's sons alike'. However, Kipling was clearly not denying the higher goodness of those whom the sons of Martha served and Oldenziel's only evidence for Kipling's alleged reconsideration is a statement by Christopher Harvie, '"The Sons of Martha"', 276, regarding Kipling's increasingly unsympathetic view of labor unions—which is evidence only if one accepts Oldenziel's unfounded identification of the sons of Martha with manual laborers. Harvie himself more rightly considered that '"The Sons of Martha" can be read as a straightforward tribute to engineers, but the values it endorses are those which most managers would advance as justifying their elite status'. Or, rather, so they would in modernity.

[168] Seely, 'Research, Engineering, and Science'.

[169] Sinclair, 'Inventing a Genteel', 11, 16. See, e.g. Maclaurin's inaugural address as president available at http://libraries.mit.edu/archives/exhibits/inaugurations/maclaurin.html (accessed 12 October 2006).

[170] Quoting Lecuyer, 'The Making of a Science Based Technological University', 180. Kline, 'Construing "Technology"', 213, says that following his appointment Compton's rhetoric changed. However, on his own evidence it changed very little. Nor should we expect it to change much, given the rhetorical obeisance to science of his industrialist supporters.

[171] Thurston quoted from ASME, *Transactions*, *1* (1880) by Multhauf, 'The Scientist', 47, note 7.

[172] Note 155, above. Earlier in his address Carty had declared, 'I consider that it is the high duty of our institute and of every member composing it, and that a similar duty rests upon all other engineering and scientific bodies in America, to impress upon the manufacturers of the United States the wonderful possibilities of economies in their processes and improvement in their products which are opened up by the discoveries in science'. Carty, 'The Relation of Pure Science', 512.

[173] National Association of Manufacturers, *News Letter*. The representatives of research on the committee were: K.T. Compton, G.B. Pegram, R.A. Millikan, Ross G. Harrison, F.R. Moulton—all pure, academic scientists—plus E.R. Weidlein (Director of the Mellon Institute), H.A. Barton (Director of the American Institute of Physics), Julius Weinberger (RCA). I am indebted to Marcel Lafollette for drawing my attention to this document, upon which she came in working through the Science Service records in the Smithsonian Institution Archives.

[174] Kline, 'Construing "Technology"', 212.

[175] Forman, 'Social Niche'. Kevles, *The Physicists*, chs II–IV, gave a good account of the pure science ideology as articulated by late 19th century American physicists. Weart, 'The Physics Business', 301–2, emphasized the rise of industrial physicists to numerical predominance in the early 20th century. For the growth of industrial research generally, see Thackray *et al*., *Chemistry in America*. Kline observed in concluding 'Construing "Technology"', 220, that 'neither the gospel of industrial research, nor the increasing respect for engineering research, nor the new terminology of the 1930s seriously challenged the established epistemological relationship between pure and applied science'.

[176] Forman, 'Social Niche'.

[177] England, *A Patron for Pure Science*, 14. Reingold, 'Vannevar Bush's', 301 (1991, 287) extended England's point, discussing in some detail 'Bush's rather idiosyncratic position' and the fact that his contemporaries 'consciously or not … were choosing a different future than Vannevar Bush'. Kevles, 'The National Science Foundation', 9–10, 17–19, 26, showed how largely the drafting committee, chaired by the very conservative Isaiah Bowman, addressing the question

'"What might the government do to aid research generally in public and private institutions?"', was crafting 'a political document, a textual weapon for the political battles of 1945 to 1950 over the shape, purpose, and choice of federal policy for research and development in the postwar era', a weapon directed specifically against the populistic proposals of Senator Harley Kilgore. But Kevles saw no need even to raise the question of agreement or disagreement between Bowman and Kilgore over basic scientific research as the ultimate source of technogical innovation.

[178] Kline, 'Construing "Technology"', 219. Kline, *ibid*., 213, suggested that the spokespersons for engineering 'deferred to an NRC ideal of pure science … probably in an effort to present a united front with the scientists', but he presented no evidence for this motive. Recently, Kline, 'Cybernetics…The Emergence of "Information Technology"', 527–9, has touched on the question of the persistence into the early 1960s of 'adherence to the ideal of "pure science", in which basic science is viewed as the fount of all new technology', again making evident his sense of grievance that those invoking 'a pure-science ideal' were 'subjugating *information technology* to *information science*'.

[179] Bush, *Science, The Endless Frontier*, 13–14. Similarly, the 'Summary of the Report' states, in its first paragraph, that 'New products, new industries, and more jobs require continuous additions to knowledge of the laws of nature … . This essential, new knowledge can be obtained only through basic scientific research'.

[180] Hounshell, 'The Evolution', 44–46. Knowles and Leslie, '"Industrial Versailles"'. The matter is stated briefly and vividly by Mirowski, 'Caveat Emptor', at note 32.

[181] *New York Times,* 'Research Milestone', recognizing the fiftieth anniversary of the founding of GE's Schenectady research laboratory. By the end of that decade this theme was part of any presidential speechwriter's stock. In the spring of 1959 President Eisenhower said: 'in our young, vigorous, and rapidly growing society, the uninformed often referred in slurring terms to what we called the "impractical scholar". Fortunately, we … have learned that the apparently visionary researcher is likely to produce unexpectedly practical results.' Eisenhower, 'Science', 137. Four years later President Kennedy said much the same, as quoted by Greenberg, *The Politics of Pure Science*, 254: 'If I were to name a single thing which points up the difference this century had made in the American attitude toward science, it would certainly be the wholehearted understanding today of the importance of pure science. We realize now that progress in technology depends on progress in theory; that the most abstract investigations can lead to the most concrete results'. Thus I was half wrong in saying of Kennedy that 'the first President who was prepared to tell them that was also the last President who would be willing to tell them that' when quoting this in Forman, 'Recent Science', 185.

[182] That in the United States in the 1950s and 1960s both the military and industry believed in the linear model, i.e. 'the notion that radical technological innovation rests exclusively on advances in basic science', is the conclusion that Asner drew from his close examination of policy and practice of the US DoD in the funding of research in conjunction with contracts for industrial production: Asner, 'The Linear Model' (2004). Asner's paper is followed in the symposium volume by Edgerton's '"The Linear Model" Did Not Exist', and Edgerton's by Hounshell, 'Industrial Research', who, drawing upon his knowledge of research policy at DuPont, rebutted Edgerton. See, further, note 422, below.

[183] Snow, 'The Moral Unneutrality', including an introduction by Warren Weaver and responses by Father T. M. Hesburgh, president of Notre Dame University, and W. O. Baker, vice president for research, Bell Telephone Laboratories. Like several other of Snow's 'moral lessons of science' lectures at about this time, this too drew wide notice and was reprinted in several places.

[184] Baker, 'Response'.

[185] That 'practical aspect' was the conflict in values between the truth-seeking scientist and the pragmatic orientation of the world of affairs, with Baker insisting on the impossibility of

the scientist making any compromise: 'Never can the scientist deal with [i.e. deal out] a half truth'.

[186] Snow, 'The Moral Unneutrality', 257. Snow had already said in his 1959 Rede Lecture, 'The Two Cultures and the Scientific Revolution', that it 'was not to our credit' that the attitude of young researchers at Cambridge in the 1930s, and he among them, 'was to take it for granted that applied science was an occupation for second-rate minds', and that the more distant one's work appeared to be from 'any practical use … the more superior one felt'. Snow there acknowledged his obvious debt to Ashby, *Technology and the Academics*, 88, 85, who had argued that 'Universities have adapted themselves considerably to the scientific revolution, but in adaptation to technology—which is one of the consequences of that revolution—they have not yet reached equilibrium', i.e. not yet taken technology as 'the core of a new twentieth-century humanism'. Snow, taking 'A Second Look' five years later, went even farther. Repudiating any attempt, including his own earlier attempts, 'to draw a clear line between pure science and technology', Snow avowed that 'The more I have seen of technologists at work, the more untenable the distinction has come to look. If you actually see someone design an aircraft, you find him going through the same experience—aesthetic, intellectual, moral—as though he were setting up an experiment in particle physics'. Snow, *The Two Cultures*, 32–3, 67. Thus Snow, the governmental administrator of scientific and technical affairs, dropping all reservations such as Auerbach had used to elevate science above technology, advanced to a very nearly postmodern position; only the retention of pure science as the standard and basis of comparison keeps it from being such. Similarly strong repudiations of the priority of purity were expressed shortly after by Medawar, 'Two Conceptions'.

[187] Baker, 'Response', 262. Baker was not exceptional: the thrust of 'the Grinter Report', the major self-evaluation of engineering education carried out in the early 1950s, was that engineering at its best was applied science, and that not all fields of engineering were doing well by that standard. Wisnioski, 'Engineers and the Intellectual', 102–4, quotes from the republication of that report in *Journal of Engineering Education*, January 1994: 'The continued growth of our knowledge of basic science has opened vast new areas to engineering endeavor and has enlarged the foundations underlying many of the existing engineering fields. Some fields of engineering have been reasonably alert in assimilating new scientific advances into their teaching programs. It is one purpose of this Report to encourage all fields of engineering education to move in this direction.'

[188] Once that faith had failed it became conventional to attribute great importance to the results of 'Project Hindsight': Sherwin and Isenson, 'Project Hindsight' (1967). Yet when those results were first published they were accompanied by strong reaffirmations of that faith in the face of the evidence presented. Declaring that 'It is clear that, on the 50-year or more time scale, undirected science has been of immense value', the authors of that report advocated that the U.S. Department of Defense devote 10% of its science and technology expenditures to such undirected research—in effect double the percentage that agency had maintained in the previous two decades. The authors of the two other investigations commonly cited as empirical disproofs of the dependence of invention upon science, Jewkes *et al.*, *The Sources of Invention* (1958; 1969), 38, 60–2, 224, and Schmookler, *Invention* (1966), 8–9, were similarly disinclined to draw from their researches general conclusions at variance with the presumed primacy of science for technological development since the late 19th century. Jewkes, himself a Polanyi-admiring conservative, was even opposed to the by then conventional view that science and scientific knowledge played a minor, even negligible, role in invention prior to the end of the 19th century.

[189] http://www.bell-labs.com/about/history/presidents.html has a portrait of each of Bell Laboratories' presidents and 75-word summary of the laboratory's accomplishments during their tenure. Only two of these summaries mention Nobel prizes: that of the first president, Jewett (1925–40), where at the end of the summary the fact of the laboratory winning its first Nobel prize is stated; and that of Baker (1973–9), where the summary begins with, and is

largely filled with, a recitation of the research fields in which Bell Laboratories' researchers won Nobel Prizes in physics. Thereafter, Nobel prizes are no longer considered noteworthy, but the summaries contain careful listings of National Medals of Technology won. J. K. Smith, Jr, 'Review Essay', 129–30, reviewing the then recent historical research on American industrial research, found it 'remarkable how similar the postwar experiences' of the several industrial research powerhouses were. Quite independently of such factors as the extent of their participation in government-funded research, in each at about the same time 'management began to lose patience with the denizens of these ivory towers' that management had itself constructed. 'What was the source of this discontent?' Smith was as ready to pin it on 'a few outspoken executives or management consultants' as on an empirically realized failure of 'the promise of science-based invention'—i.e. he saw no reasonable explanation. Which suggests that the failure of that faith in science was scarcely more reasonable than the faith itself.

[190] 'Die deutsche Industrie scheint mit ihrer grossen Wertschätzung von wissenschaftlicher Forschung einen mehr ideologisch als rational begründeten Sonderweg gegangen zu sein': Hashagen *et al*., 'Artefakte *circa* 1903', 21, speaking of 'German industry' and the past century quite generally. My contention, however, is that the German industrialists were, in this regard, not on a 'Sonderweg' but on the main route of modernity.

[191] Osietzki, 'Die Gründungsgeschichte', 49–50.

[192] 'Es ist nicht Meine Aufgabe hier eine lange Rede zu halten, aber es wird mir gestattet sein, die grosse Bedeutung der Technik aufmerksam zu machen. Die Technik ist nichts anderes als Physik und Physik nichts anderes als Naturwissenschaft. Der Unterschied besteht darin, dass die Physik uns theoretisch die Kräfte der Natur und die Verwendung derselben lehrt und die Technik die Verwendung praktisch ausführt'. Deutsches Museum, *Chronik*, 3. Prince Ludwig—born 1845; 1913–18 König Ludwig III von Bayern—restated this view less pompously, and thus more concisely and clearly, at the 3rd Ausschusssitzung, 12 November 1906: 'Die Technik ist nichts anderes als angewandte Naturwissenschaft'. Deutsches Museum, *Verwaltungs-Bericht*, 3: 26. And two years later, at the 5th Ausschusssitzung, 1 October 1908, Prince Ludwig carried this thought a step farther: 'Technik ist angewandte Naturwissenschaft, und die Technische Hochschule is nichts anderes als die Hochschule der angewandten Naturwissenschaften'. Deutsches Museum, *Verwaltungs-Bericht*, 5: 19.

[193] Hashagen *et al*., 'Artefakte *circa* 1903', 22.

[194] 'Die verschiedenen Zweige der Technik, welche mit der Wissenschaft in enger Beziehung stehen, sollen in ihrer Entwicklung durch historische Apparate, durch Modelle, sonstige typische und hervorragende Meisterstücke, sowie durch Zeichnungen und Urkunden zur Darstellung gebracht werden, dabei sollen sowohl Industriezweige Berücksichtigung finden, die ihren Anfang durch wiseenschaftliche Forschungen nahmen, als auch solche, welche, durch die wissenschaftlichen Bestrebungen in ihrer spateren Entwicklung hervorragend beeinflusst wurden'. As quoted from the protocol of the 11 May 1903, meeting of the Wissenschaftlicher Ausschuss by Füssl, 'Konstruktion technischer Kultur', 37.

[195] Füssl, *ibid*., 42; Osietzki, *Technikgeschichte*, 54, 58.

[196] For example, Harwit, *Cosmic Discovery*.

[197] Hashagen *et al*., 'Artefakte *circa* 1903', 22.

[198] *Ibid*., referring to the chapter by Hartl, 'Protuberanzenspektroskopie', 280–306.

[199] Hashagen, *Walther von Dyck*, 214–25; Hensel, 'Die Auseinandersetzungen'. For the disciplinary drive of mathematics toward increasing abstraction, see Mehrtens, *Moderne—Sprache—Mathematik*.

[200] Manegold, *Universität, Technische Hochschule*; Dietz et al., *Technische Intelligenz*.

[201] Füssl, 'Konstruktion technischer Kultur', 41–2. Riedler figures continually in Gispen, *New Profession, Old Order*, where Gispen is wholly on Riedler's side.

[202] Füssl, 'Konstruktion technischer Kultur', 41–2: 'In ihrer zeitgenössischen Wirkung werden sie in der [historical] Forschung jedoch meist überschätzt. Kaum einer von Riedlers akademischen Kollegen, geschweige denn die Mehrheit der Techniker und Ingenieure, trug seine

Kritik mit'. Further details on the replies of von Miller's advisers in Osietzki, 'Die Gründungsgeschichte', 62–4. Hensel, 'Die Auseinandersetzungen', 99–100, notes that also among engineers there was significant opposition to Riedler's anti-theoretical stance. It is an indication of how alone Riedler was in his *Technik als Kultur* stance, that Ulrich Wendt, then writing *Die Technik als Kulturmacht*, 2, could say that Roscher and Engels were the only writers in which he found even hints of the importance throughout history of 'die Technik als Kulturfaktor'.

[203] 'As to [National Socialist] ideology and rhetoric, three central elements have to be noted: the ideal of a heroic will tempered by a soldier-like character; the praise of *Anschauung*; and the utilitarian move': Mehrtens, 'Mathematics and War', 101. In 1934 the Reichsminister für Volksaufklärung und Propaganda could still write that 'working under the flag of science would bring us successfully to our goals', but by 1938 such a statement was evidently out of the question: Doel *et al.*, 'National States', 53–8.

[204] Gillispie, *Science and Polity in France*, 195–209, drawing attention to the alliance between the artisans and the artists based on a common romantic confidence in genius. Joravsky, *The Lysenko Affair*, as discussed in note 108, above, drew attention to the populistic, anti-intellectual dimension of Soviet 'science policy'. With reference to the USA, see next note.

[205] Had Perry Miller lived to complete *The Life of the Mind in America* (1965), 321–4, we would have a well-documented account of the challenge to high-culture values in the Jacksonian period. To my knowledge, no such account has been provided even to this day. Hindle, *Technology in Early America*, 87–8, noted an 'anti-intellectual and anti-scientific impulse' in such works as Amos Eaton, *Art without Science* (1830), but did not invoke Jacksonianism or otherwise suggest a connection with more general features of that period. Molella and Reingold, 'Theorists and Ingenious Mechanics' (1973; 1991), without referring to Hindle or his evidence, inferred a refusal by American mechanics to credit mechanical inventions to the mechanical philosophy from Joseph Henry's polemical counter-assertions, which they encountered in editing his papers. They cited (in their note 41) only one statement of the view against which Henry remonstrated. Their failure to find more evidence they excused with the observation that 'Actually documenting the viewpoints and alleged prejudices of these still unknown mechanics is a difficult and intricate task' (note 26). Still they were confident that a 'viewpoint like Henry's was only considered seriously by a minority. The practical labors of practical men were self-evident causes for technical improvements to most Americans of that day, judging by the evidence so far uncovered' (p. 139). The matter of this Jacksonian revolt against science is not engaged by Daniels, *American Science in the Age of Jackson*, nor by Greene, *American Science in the Age of Jefferson*, nor by Bruce, *The Launching of Modern American Science, 1846-1876*; nor by Sinclair, *Philadelphia's Philosopher-Mechanics: A History of the Franklin Institute, 1824-1865*, who has Perry Miller's book in his bibliography, but does not cite it in his notes.

[206] Reingold, 'Joseph Henry'. Kevles, *The Physicists*, 6, emphasized rather the fact that in 1850, as in 1832, Henry still saw a serious problem in the public's regard of 'abstract science' but that in 1874 Henry expressed himself in the Smithsonian's *Annual Report* as seeing a 'great change' having occurred in the previous quarter century in the public's 'appreciation of abstract science as an element in the advance of modern civilization'.

[207] Especially aviating engineers, e.g. Siegmund-Schultze, 'A Non-Conformist'.

[208] Although the opposite has been asserted of Victorian Britain by distinguished scholars, I incline to regard such claims as more ideologically than empirically founded, as I argue at greater length in Section III in regard to early 19th century America. Notable here is Sussman's contention, *Victorians and the Machine*, 6, that 'Seeing the machine as symbol of the spirit enabled Carlyle to welcome the machine while excluding intellectual mechanism. To accept technology while rejecting scientists, he brings the inventor into the context of Carlylean heroism'. A Carlylean move 'to accept technology while rejecting scientists' is simply not supported by 'Signs of the Times', on which Sussman chiefly relies. My disposition to set Sussman's thesis aside as ideological rather than empirical is strengthened by finding it quoted

approvingly, but vacuously, in a recent paper that, on my reading, contributes no evidence in support of that thesis: Miller, '"Puffing Jamie"'. On this issue of the primacy of science, which is for us the main issue, Sussman reversed himself in the latter half of *Victorians and the Machine*, stating correctly, as I think, in his concluding paragraph (233) that 'In the machine, then, the Victorian literary imagination saw incarnated the power of the scientific intellect … the deeper conflict between rationalism and intuitionism, between scientific and organic modes of thought … is the true subject of the Victorian writing on the machine'. Similarly Berg, *The Machinery Question*, 149, 155, stated that the assumptions underlying the British mechanics institutes 'fostered the views … that science was merely an appendage of technological knowledge'. However, her evidence points in the opposite direction, namely to the centrality of science in the conception of the new sort of artisan that such institutes would create. As with Sussman, but even more so, Berg's quoted contention regarding the subordination of science to technology is antithetic to her main thesis, which was that 'the scientific movement' constituted in that place and period 'a far-reaching cultural sphere' (151). If the then common notion that science stood at the center of technological progress was, as Berg so wished to emphasize, 'a mythical one' (145), 'was rhetorical only' (177), that is *not* evidence that science was viewed in Britain at that time as 'merely an appendage of technological knowledge', but the reverse: that technology was viewed as merely an appendage of scientific knowledge.

[209] The 1930s and 1940s saw publication of a considerable number of books deprecating the primacy that science had come to have in modernity, a fair fraction from the University of Chicago, among them Morgenthau, *Scientific Man*. This book, like so many such, for all that it polemicizes against science's cultural primacy, testifies to it. So, e.g. pp. 136–9, 144–5. To the extent that Morgenthau directs himself against the engineer, as in his concluding section 'The Statesman vs. The Engineer', pp. 219–231, it is only because he takes it as a matter of course that the engineer is an applied scientist—what I stress because it is overlooked by Jordan, *Machine-Age Ideology*, 9, in quoting Morgenthau.

[210] Lafollette, *Making Science Our Own*; Jordan, *Machine-Age Ideology*, 185–92, though in general leaning over backward to ignore his sources' emphasis on science, in describing the Chicago 1933 'Century of Progress' exhibition points out that the 'organizers in Chicago had conceived of the entire fair as a testimony to the power of science', and quotes the Official Guide Book of the Fair that 'in virtually everything we do we enjoy a gift from Science'. He includes a photograph of the towering 'Hall of Science', which building, 'initially called the Temple of Science, constituted the architectural focal point of the fair'. Likewise Rose 'Science As an Idiom', 3, though the fact displeased him, stressed that 'By the 1880s, articulate Americans'—i.e. 'leaders in politics, industry, and the professions'—'routinely confused technology with science'.

[211] Lafollette, *Making Science Our Own*, 9. Nor was this just middle-brow thinking: Dewey's philosophy was largely an elaboration of this conviction.

[212] Kevles, *The Physicists*, 180–4, quoting Charles Beard, 1930. On Beard, Jordan, *Machine-Age Ideology*, 214–21, who brings out Beard's heroizing of the engineer in the early 1930s, and his hopes for social salvation in the application of engineering methods, without, however, noticing the coincidence with Beard's taking his son, an MIT engineer, as collaborator, nor drawing attention to Beard's taking it as a given that whatever effectiveness engineering achieved was due to science underlying it.

[213] Kevles, *The Physicists*, 237, 239; Bix, *Inventing Ourselves*, ch. 6, *et passim*. That science was held responsible is not Bix's thesis, but is evident in her evidence. Meier, 'The Technological Concept', vii, quoted as indicative Roosevelt's Secretary of Agriculture, Henry A. Wallace, writing in *Mechanical Engineering* in March 1934: 'Yet science all this time has been creating another world and another civilization that simply must be motivated by some conscious social purpose, if civilization is to endure'.

[214] As quoted by Kevles, *The Physicists*, 399–400.

[215] Paul Goodman, *New Reformation*, p. 21, as quoted by Winner, *Autonomous Technology*, 4. Goodman speaks not of 'technology' but of 'science'. Nonetheless, Winner puts forward the quotation in illustration and explanation of why *technology* was then becoming a matter of such general concern.

[216] The Bishop of Ripon's brief remarks from a Leeds pulpit, and specimens of the huge uproar in the press they occasioned, are quoted in *The Literary Digest*, as reprinted in Hughes, *Changing Attitudes*, 217–21.

[217] Kevles, *The Physicists*, ch. XVII. Striking here with Roosevelt's Science Advisory Board, as with the composition of the President's Science Advisory Committee 25 years later, is its overwhelmingly 'pure' science membership. That initial cohort was supplemented by additional appointees a year later—*not* in order to remedy the absence of *applied* scientists—let alone engineers—but to remedy the relative absence of *life* scientists: Cochrane, *The National Academy of Sciences*, 652–5.

[218] National Association of Manufacturers, *News Letter* (19 November 1938). See note 173, above.

[219] Jordan, *Machine-Age Ideology*, 9, 217, 219, unreflectively conflates science and technology, doing so half in the scientistic spirit of the era that he describes, and half in technologistic spirit of postmodernity: 'The term *engineer* could generically connote inventors and scientists as well as professional applied scientists'. But though Jordan gives primacy to technology, the primacy of science for his subjects keeps popping out of his exposition. Thus in his pages on Rexford Tugwell (247–51), his leading example of a convinced technocrat empowered by the New Deal, Jordan speaks always of Taylorism and social engineering, while in his exhibited quotations Tugwell speaks of science.

[220] Needell, *Science, Cold War*. Sometimes, of course, physicists proved to be less than good technologists, as experiences in nuclear reactor design and construction ought to have shown: Hewlett and Anderson, *The New World*, 305–8; Needell, 'Nuclear Reactors'.

[221] Dickson, *The New Politics*, 28, quoting Eisenhower's Science Advisor, James Killian; Wang, 'American Science'. On the creation of the JASON consultative group of elite physicists: Aaserud, 'Sputnik and the "Princeton Three"'.

[222] Carson and Gubser, 'Science Advising', 152; Osietzki, 'Die Physik', 65–6.

[223] Carson, 'Nuclear Energy Development', 234.

[224] Ellul, La *Technique*. Kranzberg as editor of *Technology and Culture* devoted an entire issue (Vol. 3, No. 4) to materials from that conference, including Ellul, 'The Technological Order'. (Ellul sent this contribution; he was not physically present.)

[225] Hutchins in Preece, 'Ideas of Technology', 471. Also in 1962 Hutchins' Center sponsored a 'Conference on the Role and Responsibilities of Science Executives in the Federal Service' and published its report, *Science and Democratic Government* (1963). In the 4 October 1963, issue of *Science*, Greenberg, 'Reaction to Snow' quoted Hutchins, at some length, from a recent 'Occasional Paper' of his Center, developing the thesis that 'professors are somewhat worse [morally] than other people, and that scientists are somewhat worse than other professors'. Greenberg's intent was likewise to ridicule the notion that 'we should permit ourselves to be ruled by scientist-kings', but that very premise of Greenberg's piece is evidence that scientists were then being looked to as philosopher-kings.

[226] Miller, *Lewis Mumford*, is the authorized biography and a considerable achievement, though having many deficiencies, among them the want of a 'theory' of Mumford—for which see Forman, 'How Lewis Mumford Saw Science'. The side of Mumford most seriously neglected by Miller, namely Mumford's steady concern with architecture, is the subject of a dissertation by Wojtowicz, 'The Lewis Mumford Decades', and of a book he extracted from it: Wojtowicz, *Lewis Mumford and American Modernism*. Wojtowicz, *Sidewalk Critic*, 11, judged Mumford 'the most important architectural critic produced by the United States in the twentieth century', and that judgement is affirmed by Mallgrave, *Modern Architectural Theory*, 293. Wojtowicz has also been responsible for the revision and the updatings of the

bibliography of Mumford's publications: http://www.library.upenn.edu/collections/rbm/mumford/index.html.

Marx, 'Lewis Mumford, 164–5, underscores the uniqueness of Mumford's career of unequaled duration, productivity, visibility, and breadth and recognized scholarly importance of his writings. Both Marx and the editors of that volume acknowledge for the term 'public intellectual', Jacoby, *The Last Intellectuals*, where Mumford, with his 'singular oeuvre, almost unequaled in American letters' (191), figures importantly.

As regards the history of technology, Hughes has been Mumford's longest and steadiest proponent. Mumford is all through Hughes, *American Genesis*, where he figures both as historical actor and as repeatedly affirmed historical–cultural interpreter. (In addition to the many pages listed in the index of that book, Mumford appears also on pp. 354–60, 369, 453, 454, 458.) Further evidence of Mumford's high importance for the historians of technology is provided below in Section III. As regards American Studies, Mumford makes a case for himself in opening his 'Introduction to the 1957 Edition' of *The Golden Day*. As of 24 October 2005, Googling 'American Studies' + 'Lewis Mumford' produced 842 web pages, while Googling 'American Studies' + 'Alan Trachtenberg'—he being perhaps the most widely and highly regarded American Studies scholar at work today—produced 780 web pages.

[227] Mendelsohn, 'Prophet of Our Discontent', 355, 357: 'the scientific community, and especially its leaders, saw him as posing a direct challenge to them'; saw Mumford, *The Pentagon of Power* 'as an attack on the very core of their practice and their thought'.

[228] Hughes, *American Genesis*, 359, placed himself squarely with Mumford on the side of 'life'. Regarding *Lebensphilosophie*, see the notes to the sub-section below on the Erlangen and Starnberg schools. Among the seventeen contributors to Hughes and Hughes, *Lewis Mumford*, many point to the centrality of the organic and of 'life' in Mumford's thought, but only one recognizes *Lebensphilosophie* as the appropriate term, and the German intellectual tradition as pertinent context for Mumford's thought: Molesworth, 'Inner and Outer', 252.

[229] May, *The End of American Innocence*.

[230] Mumford, 'The Pragmatic Acquiescence: A Reply' (1927).

[231] Forman, 'How Lewis Mumford'. See also notes 139 and 140, above.

[232] Mumford, 'The Pragmatic Acquiescence: A Reply'. The phrase that Mumford quotes is from Dewey, 'The Pragmatic Acquiescence'. The Humpty-Dumpty simile was a favorite of Mumford's; he had used it in Mumford, 'Via vitae', and would use it again in *Technics and Civilization*, 447.

[233] Mumford, *The Golden Day*, 94, 131, 135–6. The reference to 'Mr Babbitt' was, of course, to the title character of Sinclair Lewis's *Babbitt* (1922) who had been seized upon immediately as the perfect expression of the philistinism that Mumford's generation found most antipathetic in the middle-class American. The characterization of Dewey's mental habitus as 'protective coloration' was due to Mumford's admired older contemporary, Randolph Bourne. In order to disdain Dewey's instrumentalism as Babbittry, Mumford had to misrepresent Dewey's ranking of technology on the scale of cultural values: 'Mr Dewey's instrumentalism is bound up with a certain democratic indiscriminateness in his personal standards: a Goodyear and a Morse seem to him as high in the scale of human development as a Whitman and a Tolstoi'. Mumford, *The Golden Day*, 134. This misrepresentation of what we have seen to be Dewey's views serves Mumford's endeavor to distinguish himself from Dewey by affirming the romantic orthodoxy of the artistic circles into which Mumford had thrown himself in the previous two or three years. The continuing antagonism between Mumford and Dewey from that date to the early 1940s is closely and broadly considered by Westbrook, 'Lewis Mumford, John Dewey'. (It should be noted, however, that Mumford did seek to make a generous gesture towards Dewey in 1938, one that he succeeded in making in 1946: Mumford, *Values for Survival*, 3, 9.)

[234] Westbrook, 'Lewis Mumford, John Dewey', 301–2, found Mumford's criticisms of Dewey 'often wide of the mark', and that the two 'were kindred spirits to a greater degree than either

was willing to admit'. Repeatedly through that essay Westbrook exhibited quotations of Dewey indicative of the similarity with Mumford in social philosophy and view of art (pp. 307, 308, 310, 311, 312). Likewise, Blake, 'The Perils of Personality', 287: 'Mumford owed more to Dewey than he usually acknowledged'. Dewey is absent from Mumford's autobiography of his early years, *Sketches From Life*.

[235] Thus in his man of letters phase, running from the mid 1920s to the late 1930s, Mumford insisted that contemporary man's greatest need is for 'the ability to conceive new forms and channels for life to run in, the ability to think creatively with the artist who says "I will" rather than causally with the scientist who says "It must"'. Mumford, 'The Pragmatic Acquiescence: A Reply'.

[236] A hegemony won above all through 'the very success of the physical sciences in their applications to technics': Mumford, *Values for Survival*, 83. 'Philosophy is reclaiming the place which science so confidently pre-empted on the basis of its pragmatic applications': Mumford, 'The Grain of Things' (1930), the pertinent paragraphs of which Mumford reprinted, under the heading 'The Sciences and Philosophy', in *Findings and Keepings*, 193, and again in *My Works and Days* (1978), 190, where Mumford, not insignificantly, modified it to 'its many successful pragmatic applications'. Even in these middle years of primary identification with the artist, Mumford vacillated on the question whether to deny physical–mathematical science its scientistic place as highest cultural value. So, for example, in *Technics and Civilization*, 361, Mumford judged 'creating a neutral world … the great general contribution of modern analytic science. This contribution was possibly second only to the development of our original language concepts.' In *The Golden Day*, 132, Mumford had even gone so far as to say that 'with the introduction of the scientific method, men began to think consciously as whole human beings'.

[237] Mumford, 'The Proud Pageantry of Man' (1922), and more fully at about this same time in *The Story of Utopias*, 271: 'On the basis of the precise knowledge of physical relations which became available in mathematics, physics, mechanics, and chemistry the startling changes which have been crudelly labeled the "industrial revolution" were carried through … . The actual world of machinery is at present, it seems fair to say, a parasite upon this body of knowledge, and it would speedily starve to death if the host were annihilated'. See, also, note 61, above.

[238] Mumford, *The Golden Day*, ch. 1, as reprinted in Mumford, *Interpretations*, 7–8. The corresponding pages in *The Golden Day* are 6–8. The same is restated there more concisely on p. 137.

[239] Mumford, 'Drama of the Machines', 227–28.

[240] Mumford, *Technics and Civilization*, 46.

[241] *Ibid.*, 57. It must be allowed that there are a couple of passages in the 1930 *Scribner's Magazine* essay (in Mumford, *Interpretations and Forecasts*, 229, 231) in which Mumford asserts the opposite causation, as would be expected from romantic-*lebensphilosophisch* principles of the primacy of practice, and more especially from that version of them avowed by Veblen. Thus a couple pages after developing his 'general staff' metaphor for the role of scientists, Mumford has a couple paragraphs developing the thesis that 'The woodman was the chief contributor to the precise arts: … in his creation of the engine lathe … he handed on the most useful perhaps of all machine tools, for without it accurate machines and instruments of measurement could not be made. … From the woodman's primitive distillation of tar to the thousand dyes and medicines and poisons that come from the destructive distillation of coal,' etc. There Mumford even says that the machine 'has created for the accomplishment of certain physical results a universal language: the language of exact science'.

Yet these statements are so inconsistent with his most fundamental preconceptions that Mumford just cannot believe what he finds himself saying. So, in the midst of that passage making the woodman the real source of modern technology and modern science too, Mumford reinserts 'the General Staff' to make it all work: 'Once these key inventions were

planted, once the General Staff was ready to supply a broad stream of abstract ideas and suggestions, the time had come for the machine to take possession of Western civilization: at last the derivative products of industrialism could spawn and multiply'.

Otherwise, through this essay it is, over and over, the primacy of science to and for technology that comes to expression: 'Hitherto the sole influence upon machine design had been the physical sciences' (234); 'the three greatest monuments' of 19th century engineering, the Crystal Palace, the Brooklyn Bridge, and the Eiffel Tower, were 'created with the aid of physics and mathematics and their special technologies' (235); 'the machine has given us a noble austerity of form' in consequence of 'its background of scientific concepts and abstract categories' (239). Long, 'Lewis Mumford', 171–2, points out the primacy that Mumford ascribed to scientists and his disparagement of engineers.

[242] Mumford, *Technics and Civilization*, 216. True, this subsection—titled 'The Importance of Science'—has as opening sentence: 'The detailed history of the steam engine, the railroad, the textile mill, the iron ship, could be written without more than passing reference to the scientific work of the period'. Mumford, as reader and writer about the history of technology knew this to be so, but as modern was incapable of assimilating the stated fact and reconciling it with his presuppposition of the primacy of science for technolgy. As a result Mumford stated this antithetic fact only as a rhetorical foil for an exposition of 'The Importance of Science'.

[243] *Ibid*., 217–18. Two years later, in reviewing J. G. Crowther's *Men of Science* ('Science Out of the Cloister') Mumford wrote: 'Mr Crowther picks up the thread at the point where Smiles dropped it: but instead of dealing with derivative motors of industry, he deals with the prime movers—the physical scientists who opened new fields of exploration, formulated new problems, and laid the basis for a thousand practical applications'.

[244] Mumford, *Technics and Civilization*, 218–19. The affinity with Veblen and Dewey is to be seen in Mumford's frequent pejorative use of 'pecuniary', as in Mumford, 'Toward Civilization?' (1930), 50: 'the pecuniary interests and preoccupations of the business man'.

[245] Mumford, *The Pentagon of Power*, 122–3. Mumford incorporates here in chapter 5, 'Science as Technology', in great part a paper that he had published in 1961: 'Bacon: Science as Technology'. The primary, leading, nearly exclusive, role of science in the creation of new technologies is emphasized repeatedly in this earlier paper (159, 161, 163–6) as it is in the 1970 book (65–73, 120–1).

[246] Among innumerable instances testifying to Mumford sharing the common perception of a heightened primacy of science, *The Myth of the Machine I*, 3, opens: 'The last century, we all realize, has witnessed a radical transformation in the entire human environment, largely as a result of the impact of the mathematical and physical sciences upon technology'.

[247] Mumford wrote to his friend Benton MacKaye in 1964 that 'the primacy of mind' was the concept underlying *The Myth of the Machine*, his two-volume work then in progress. Miller, *Mumford*, 510; see also 456. 'The Primacy of Mind' is the title of the opening section of Mumford's 'The Human Heritage', first published in 1972 and republished in Mumford, *My Works and Days*, 468–84. Mumford's argument there is, again, against *homo faber*, insisting that man's discovery of the capabilities of his big brain long preceded development of even the crudest tools. Already in 1951 Mumford, arguing against Bergson's *homo faber*, asserted that 'the chief source of this [i.e. man's] particular form of creativity was not fire, tools, weapons, machines, but two subjective instruments far older than any of these: the dream and the word': *The Conduct of Life*, 40. This was of course Mumford's leaning from early on, but in *Technics and Civilization*, 370, it appeared not as a thesis, but as inadvertent lapse: 'the tools and instruments society has developed through history—words, symbols, grammar, logic'. Mumford's reassertion of the primacy of mind post-Second World War was quite in line with the general shift to mentalism that appears strikingly in the history of the history of science. See Mayer, 'Setting Up a Discipline'.

[248] Mumford, *The Condition of Man*, 5.

[249] Mumford, *The Myth of the Machine I*, 7, with the volume as a whole being devoted to minimizing the role of the tool and the machine. This feature, as well as his discovery that the most important technics in early civilizations were organized bodies of human beings, were emphasized by Mumford in 1959 as the points in which his thought departed from and advanced over *Technics and Civilization*: Mumford, 'An Appraisal of Lewis Mumford's', 529–30. Long, 'Lewis Mumford', 170–1, underscores Mumford's disdain for 'the chipping of a mountain of hand-axes'.

[250] Mumford, *The Myth of the Machine I*, 11–12. Recapitulating this argument early in the second volume of *The Myth of the Machine*, Mumford emphasized that 'the priesthood of science, whose mathematical measurements had first disclosed and utilized this source of cosmic order, lacked the faintest premonitions of the possible consequences. In all innocence, astronomy and celestial mechanics laid the foundation for a more absolute order, political and industrial …'. Mumford, *The Myth of the Machine II*, 30. Miller, '*The Myth of the Machine*', 155–56: 'Mumford argues that the critical tools that led to the assembly of the megamachine were inventions of the mind: mathematics and astronomical observation …'.

[251] Up until 1927 the difference between Mumford's treatment of scientists and of engineers is drastic: the scientists generally considered admirable and their activities all-important; the engineers generally insulted, and their actions almost always injurious. Thus in *The Golden Day*, Mumford speaks of science, affirmatively, on 7–9, 14, 50, 54, 93, 98, 113, 115, 132, 141, 144, and of technology and engineers, negatively, on 49, 87–8, 118, 132, 134–7, 141. Mumford, who in his handling of technology here is heavily indebted to his admired contemporary Van Wyck Brooks, simply ignores the affirmations of it by 19th century American writers—or, rather, does so up to his antepenultimate page where he says: 'Need I recall that Whitman wrote an apostrophe to the locomotive … that Thoreau, who loved to hear the wind in the pine needles, listened with equal pleasure to the music of the telegraph wires?' So far from having previously stated anything of the sort, Mumford had earlier in that book explicitly denied Whitman and Thoreau such sympathies. In 1927/28 Mumford underwent a sea change, and could say that 'engineering shares with music the supremacy in the arts during the last hundred and fifty years'. Mumford, 'The Arts', 303, 305, 312. For evidence that a refusal to see the results of engineering functionalism as aesthetic remained Mumford's predominant attitude until about 1928: Wojtowicz, 'The Lewis Mumford Decades', 293–301; Samson, 'German–American dialogues', 372, 384–89, 407–8; Samson, '"Unser Newyorker Mitarbeiter"', 134.

[252] Hughes, *American Genesis*, 297, 300, 446, ignoring the lateness, the anomalousness, and the transience of Mumford's post-1927 philotechnic phase, represented him as having from 1921 onward 'experienced the technological enthusiasm of the interwar years'. Hughes presented Mumford as *avant garde*, where in fact his aesthetic sensibility was very conservative—close to that of Ruskin, the main influence on his aesthetic formation. More generally still, if I am right in the argument of this paper, *American Genesis* is misconceived in taking 'technological enthusiasm' as being simply or even primarily an enthusiasm for technologists. That *Century of Invention and Technological Enthusiasm, 1870–1970*, was much rather—and for Mumford, most emphatically—an enthusiasm for science and scientists, and they not merely but also as the creative agents giving birth to new technologies.

[253] This is striking in Mumford's survey of 'The Arts' (1928), where for all that he elevates engineering, he presents science as the main motor, for the fine arts as well. (One must rely upon context to construe the meaning of the word 'art' in Mumford's writings prior to the late 1930s, for he used it not only to refer to aesthetic endeavors but also at times in the archaic sense of the *industrial* arts, technics.)

[254] Prior to the early 1930s Mumford had declined to accept his mentor Patrick Geddes' conception of an emergent neotechnic era, and he would begin to turn against it again shortly after completing *Technics and Civilization*. On this point see Forman, 'How Lewis Mumford Saw Science'.

[255] Mumford, *Technics and Civilization*, 216–20; quotation on 218. Hughes, *American Genesis*, took much the same pro-invention, anti-engineering stance. I have seen one statement by Mumford regarding the actors to whom technics are due that places engineers first and scientists last: Mumford, 'An Appraisal of Lewis Mumford's', 535.

[256] Mumford, *Technics and Civilization*, 219–20. The German term is *Schiffsbauingenieur*, but had he translated it as 'naval architect' Mumford would have found it difficult to make fun of its bearer. Upon publication of *Whither Mankind?*, the volume edited by Charles A. Beard that included Mumford's 'The Arts', a group of engineers protested the neglect of their contribution to human progress, with the upshot that Beard edited a sequel, *Toward Civilization* (1930), authored by that group. Reviewing it, Mumford, 'Toward Civilization?', showed only contempt for engineers (while using a scientist as avatar of the virtue they would fain possess): 'interspersed with innumerable references to the wisdom, integrity, fairness and general aptitude of the engineer, a class of illumined minds who, according to the authors—and who would know better?—combine the chivalry of a Bayard with the intellectual resourcefulness of a Faraday … . Is there any notion, among these distinguished contributors, that their art has as its social foundation the destructive occupations of mining and warfare? … and that this mixture of mining, warfare and finance has been the specific medium in which engineering has germinated and spread … . Without this specific medium, engineering would be an important but subordinate art: while the medium exists, scarcely any other art than engineering is capable of flourishing.' Ergo, let us ignore it.

[257] Mumford, *The City in History*, a 600-page tome on the city from antiquity to the 20th century—the city especially in its physical aspects, including provision for the physical needs of city dwellers—that has scarcely a dozen pages on civil engineering! Indicative is the first entry in the index under 'Engineering', viz. 'inadequacy of Roman and American'—Mumford is contemptuous of its 'flatulence'. 'Engineers, fortifications need for' leads to the assertion that 'The development of the art of fortification shifted the emphasis in building from architecture to engineering', and that this was a 'prelude to the wider technics of the machine' (360). As this involved a shift in emphasis 'from esthetic design to material calculations of weight, number, position', Mumford will not go there. The grossly unsanitary conditions in English cities and towns in the mid 19th century are portrayed over many pages in graphic detail, but their remediation is dealt with by a wave of the hand toward 'those improvements which three generations of active legislation and massive sanitary engineering have finally brought about' (462).

[258] Mumford, 'Prologue to Our Time'; Mumford, *My Works and Days*, ch. 1. First published in *The New Yorker* in 1975; it had been drafted more than 10 years earlier. The essay concludes with Mumford exhilarated by the discussion of black holes 'at a lively luncheon of astrophysicists which I attended at M.I.T. … The astrophysicists are daringly open-minded fellows,' and it is from the stimulus of their ideas that 'my own faith blithely flourishes. Let the curtain rise on the twenty-first century—*and After*!'

[259] See Section I, notes 25, 33–40, particularly, and notes 260–70, below.

[260] Schnädelbach, *Philosophy in Germany, 1831–1933* (1984), ch. 5, particularly 142–7, asserted the *lebensphilosophisch* unanimity, right to left, but acknowledged (142) that 'no real research has yet been done into the roots in social and ideological history of this basic mood'.

[261] Although evidence of this romantic tradition of disparagement of technology and experimental science is plentiful, no scholarly account of it is known to me. Herf, *Reactionary Modernism*, who, given his topic, ought to have investigated the romantic tradition, did not. Promising explorations of the post-Second World War intellectual–political scene focusing on the appraisal of technology in relation to science are being undertaken by Beyler, 'The Demon of Technology', and 'Physics and the Ideology'. Carson, 'Science as Instrumental Reason', has extended her close attention from Heisenberg to Heidegger and Habermas in a paper that she has kindly allowed me to read in draft.

[262] To my knowledge, the closest to come to such an elevation of technology was Friedrich Dessauer, who indeed was a true original. (See, e.g. Wolfgang Pohlit, 'Friedrich Dessauer,

1881–1963', available at http://www.physik.uni-frankfurt.de/paf/paf84.html [accessed 12 October 2006].) Dessauer's *Philosophie der Technik* (1927) is unique in its claims for the value of technology in every dimension of culture, from economics and ethics to epistemology and ontology, and on to religion ('approach to the throne of God'). Dessauer made an argument there for an invention being something more wonderful than a scientific discovery (what we postmoderns simply assume): the discovered phenomenon or law was out there all along, but through 'Erfindung … neue Qualität in die Erfahrungswelt zum ersten Male hineinkommt' (61). He even presented such scientific achievements as weighing the planets as technical (3). Nowhere, however, did Dessauer explicitly assert that science as such is technology. Rather, as one expects from a physicist, for all that Dessauer put the technical object forward as the highest form of 'realization', priority remained with the laws of nature revealed by science: 'Die Untersuchung des Gegenstandes "Technik" ergibt, daß es sich um Realisation gewisser Ideen handelt, jener nämlich, die mit Naturgesetzen erfüllbar sind' (viii); 'Die Inhalte der Naturgesetze … haben Wirklichkeit im höchsten Grade' (35). Moreover, the result of Dessauer's many additions and reformulations in *Streit um die Technik* (1958) was to weaken his claims for the epistemic primacy of technology.

McCormmach, 'On Academic Scientists', 162, said that Wendt, *Die Technik als Kulturmacht* (1906), had argued 'that technology now determined the science of the time rather than the reverse', but I am not able to find that argument in the book. True, Wendt (8–10, 315), like Riedler, was annoyed that 'In volkswirtschaftlichen Werken wird zuweilen mit einer gewissen Absichtlichkeit darauf hingewiesen, daß die moderne Technik abhängig sei von der Naturwissenschaft'. However, Wendt did not *deny* that dependence of modern technology upon natural science. On the contrary, as Marxian socialist Wendt believed in it, but also believed that 'die Technik' formed 'die Ergänzung der Naturwissenschaft', rather than science being an end in itself.

[263] The West German Constitution includes a guarantee of the freedom of science—which freedom is commonly understood as guaranteeing the production of truth through the absence of any constraint on the course of scientific work due to political interests. That constitutional guarantee reflects the fact that the pre-postmodern self-conception of the scholar as necessarily disengaged from the hurly-burly of ordinary life, and more especially of political life, had found stronger cultural support in Germany than in any other country. Yet, paradoxically, even more characteristic of German culture has been the *lebensphilosophisch* consensus that 'life' is the fundamental basis of knowledge—the only legitimate basis of knowledge—thus creating a deep conviction, on the right as on the left, that value-free science was both impossible and undesirable.

[264] Mösgen, 'Wilhelm Kamlah'; Roberts, 'Lorenzen, Paul'. A couple essays by each of some eight members of the Erlangen school are translated and published in Butts and Brown, *Constructivism and Science*.

[265] Kamlah and Lorenzen, *Logische Propädeutik*. My sales figure is upped slightly from that as of 1983 given in Lorenzen's preface to his *Normative Logic and Ethics*, 3. The criticism of Heidegger in *Logische Propädeutik* is largely implicit; they criticize Heidegger directly in only one short section (pp. 111–12 in the English translation), where they—or, rather, Kamlah—show that one, the central one, of Heidegger's many cockamamie etymologies is sheer invention. Kamlah had taken Heidegger to task for his use of insupportable etymologies—'zur Erdichtung Ihrer Ihnen vollends eigentümlichen Begriffsmythologie'—in 1954 in an open letter in response to Heidegger's 'Die Frage nach der Technik'. (See Kamlah, *Von der Sprache*, 119, at Kamlah, 'Martin Heidegger'; Kamlah had studied with Heidegger *ca* 1930, but was excluded from an academic career by the National Socialist regime. He had opted for clarity in the mid 1930s, but that open letter was his first published attack on Heidegger.)

[266] Lorenzen, 'Methodisches Denken', 36–7. The capitalization of 'Life', as also the inclusion of the original German of the quotation of Dilthey, is as it appears in the English version. The German version is reprinted as title essay in Lorenzen, *Methodisches Denken*, 24–59

(quotation from p. 26), and retranslated somewhat more freely in Lorenzen, *Constructive Philosophy*. The title of Lorenzen's essay and book signals both a continuation of Dingler's program (see note 270) and what distinguished Lorenzen's program from the sheerly romantic tendency of most *Lebensphilosophie*, namely his affirmation rather than rejection of method. Grappling with this issue from a position much closer to Heidegger, was Gadamer, *Truth and Method* [original German edition, 1960], especially 214-35 on Dilthey.

[267] Regarding Heidegger as *Lebensphilosoph*, see note 38, above. What interested Lorenzen in Misch is suggested by the title of a recent publication of Misch's lectures at Göttingen in the late 1920s: Misch, *Der Aufbau der Logik auf dem Boden der Philosophie des Lebens*.

[268] Transcendental pragmatism was what Habermas advocated in *Erkenntnisse und Interesse*, and it is that which made Peirce so attractive to him. (Dewey, considered as political thinker, ought to have been Habermas's man, but was disqualified by his naturalism.) Habermas, emphatically in the tradition and spirit of *Lebensphilosophie*, observed approvingly in *Erkenntnisse und Interesse*, 185, that 'For Dilthey the category "subjective experience" ["Erlebnisse"] was from the outset a key for his theory of the Geisteswissenschaften'. In that book Habermas quoted Dilthey more often than any other writer. Among the very few references in it to contemporary thinkers there is a highly complimentary one to Lorenzen's work (90, n. 2).

[269] Lorenzen, 'Wie ist die Objektivität'. The fullest exposition of the 'protophysics' program in English is Janich, *Protophysics of Time*. A critique of the program was made by Wilhelm Kamlah's son Andreas in the *Festschrift* for Lorenzen's sixtieth birthday: Andreas Kamlah, 'Zur Diskussion'. A recitation of the main institutional loci and publications of *methodische Philosophie* = *konstruktive Philosophie* = '*Erlanger Schule*' is given by Janich in his 'Vorwort' to Janich, *Entwicklungen*, that volume being composed of papers presented at the *Festspiel* for Lorenzen's 75th birthday. An exposition entering rather more into substantive issues is provided in Janich, *Konstruktivismus und Naturerkenntnis*, 106–9, 123–8.

[270] Dingler, *Aufbau*. Here Lorenzen, 'Vorwort', 11, begins that reading back into Dingler of a reduction of science to *Technik* which will become characteristic of the Erlangen/Konstanz school (with the notable exception of Jürgen Mittelstraß). Dingler would not have been pleased with this reconstruction of his program. No more than Auerbach did Dingler intend to reduce physics to technics. On the contrary, Dingler took pride in having elevated and validated experimental physics (over against theoretical physics), what Dingler thought necessary because experimental physics 'bisher als ein unerklärliches, halb künstlerisches, halb handwerkliches Metier betrachtet worden war', Dingler, *Das Experiment*, 253. As is to be expected, Dingler was a passionate *Lebensphilosoph:* see the bouquet of brief quotations of Dingler's *lebensphilosophisch* declarations in Weiss, *Hugo Dinglers*, 356.

[271] Janich, *Zweck und Methode*, 17: 'Mein Vorschlag bedeutet, dass an die Stelle der verstaubten Ideologie vom Naurforscher, der die Welt enträtselt, auch und gerade im Selbstverständnis der Physiker als einzige *die Aufgabe der Technikermöglichung* tritt'.

[272] Janich, 'Physics', 11. My attention was drawn to Janich, and thus to the Erlangen school, through the quotation of this passage by Hans Radder, 'Technology and Theory', 155.

[273] Janich, 'Physics', 13. Janich recognizes that this contention requires that physical laws be regarded as the codification of technical experience, and he asserts this to be the case (24), but he does so entirely without argument or example.

[274] Auerbach, *Entwicklungsgeschichte*, 4. See notes 24–6, above, and the discussion in the text.

[275] So, for example, Tetens—whom Weiss, *Hugo Dinglers*, 1, identifies as 'grandson' of Lorenzen, i.e. student of a student—obviously *wants* to say in his 1984 essay Tetens, '"Der Glaube an die Weltmaschine"' that physics is from the outset technology, that the very method of experiment is technological, but he just cannot bring himself to assert it. When Edge reviewed *The Dynamics of Science and Technology*, the volume carrying Janich's 'Physics' in *The British Journal of Sociology*, largely unfavorably, he made no mention of Janich's contribution—perhaps because Edge, for all his interest in instruments, remained quite modern in his view of

them as merely instrumental. Knorr ('Review of Krohn'), however, reviewing it in 1981 in *Isis*, featured Janich's essay: 'Most removed from a historical investigation, but nevertheless quite fundamental in its implications, is Peter Janich's answer to the question whether physics itself must not be considered a technology rather than a traditionally conceived natural science. The paper, which starts off the book, criticizes the 'objectified' conception of nature held by physicists and philosophers alike and reminds us that the experience of modern natural science is structured by instruments. Doing experiments is held to be more an activity to produce technical effects than a search for true sentences, and theories are seen as norms for technical practice rather than as representation of nature'. Knorr's précis reproduces Janich's illogic, and shows her readiness—greater than Janich's own—to carry forward in that direction. Apart from Knorr's review, the only references to Janich's paper turned up in a search of JSTOR are in Brush, 'The Chimerical Cat', 446, note 148, where, after citing a series of papers on the anthropic principle in cosmology, Brush concluded 'Cf. …', and in Jamison, 'Technology's Theorists', 510, note 10: 'On the instrumental dependence of science, see …'. Thus neither takes Janich's point.

[276] In Lorenzen's lengthy 'Theorie des technischen Wissens' (1976), no form of the word *Technik* appears in the text (and *a fortiori* no explanation is given of the concept 'technischen Wissens'). Similarly, Lorenzen, *Grundbegriffe technischer…Kultur* (1985), reprints 12 essays published in the previous seven years. Four are grouped in a section headed 'Technik'. The word appears only in the first of those four, and there on only two pages (62, 74) and in two very different senses, namely the 'Technik' of generating random sequences (with the drawing of lots as prehistoric realization) and 'der "technische Nutzen"' of research (which logician Lorenzen would define so widely, through the Machian concept of mental economy construed as paedagogical technology, as to justify everything that is commonly justified by the virtue of being 'wertfrei').

[277] In Janich, *Protophysics of Time* (1985), 62–6, there are still some faint echos of his previous positions, both that in 1973 and that in 1978, but significantly Janich comes to the conclusion not that physics is technology, or even that it is a technical science, but merely that 'It thus appears more suitable today to speak of an *experimental science*, instead of a science of nature, i.e. to characterize physics not by means of the subjects investigated, but rather by means of a domain of methods'. The science-as-technology thesis is then entirely absent from Janich, *Konstruktivismus und Naturerkenntnis* (1996), a collection of essays that he had published over the preceding eight years.

[278] Janich, 'Die Struktur technischer Innovationen' (1998).

[279] Quotation from Böhme *et al*., 'Finalization in Science,' 307.

[280] Drieschner, 'Die Verantwortung der Wissenschaft'. Weizsäcker, 'Erforschung der Lebensbedingungen', written in June 1979. The 'Erklärung der 18 Atomwissenschaftler vom 12. April 1957' ('Göttinger Erklärung') is reprinted in Weizsäcker, *Der bedrohte Friede*, 29–30.

[281] Weizsäcker, 'Erforschung der lebensbedingungen', 471; Drieschner, 'Die Verantwortung der Wissenschaft', *passim*.

[282] For example, Böhme, *Protophysik: Für und wider*, in which the first essay, by Jürgen Mittelstraß, is 'Wider den Dingler-Komplex'. A further disparaging reference to Dingler is Böhme *et al*., *Experimentelle Philosophie*, 201. Both von Weizsäcker and Habermas contributed to Lorenzen's Festschrift: Lorenz, *Konstruktionen versus Positionen* (1979).

[283] Böhme, 'Ist die Protophysik', concluding lines: 'Die Protophysiker behaupten nun, daß die Handwerksregeln in der Naturwissenschaft gewissermaßen "hochstilisiert" vorkommen. … Die Hochstilisierung, die Handwerksregeln in Regulative überführt, die das wissenschaftliche Verhalten verbindlich gestalten, besteht also darin, daß die maßgebenden Gegenstände oder Verhältnisse des Handwerks auf Ideale hin überschritten werden und so selbst von diesen Idealen her kritisierbar und korrigierbar werden'.

[284] Böhme *et al*., *Experimentelle Philosophie*, 9: 'lösen die wissenschaftliche Erfahrung von ihren vorwissenschaftlich-technischen Ursprüngen ab'.

[285] Böhme *et al.*, 'Finalization Revisited', 132. Heisenberg, 'Der Begriff "abgeschlossene Theorie" in der modernen Naturwissenschaft', 93: 'Die abgeschlossene Theorie gilt für alle Zeiten'. Heisenberg, who had there immediately added that 'Die abeschlossene Theorie enthält keine völlig sichere Aussage über die Welt der Erfahrung', could not have been pleased by this use of his concept by his friend von Weizsäcker's men.

[286] Böhme *et al.*, 'Finalization Revisited', 131.

[287] Böhme *et al.*, 'Finalization in Science', 314.

[288] *Ibid.*, 10, being the concluding paragraph of the 'Authors' Introduction'. The book concludes with Wolf Schäfer, 'The Finalization Debate', an account and bibliography of the media campaign in spring 1976, directed against the Starnberg institute and specifically the finalization thesis, by a group of academics associated with and supported by the conservative Fritz Thyssen Stiftung.

[289] Böhme *et al.*, 'The "Scientification"', 225.

[290] 'The term *Finalisierung* … is derived from the traditional category of *causa finalis*, its connotations then are the goals or purposes of science not the end of science'. Böhme *et al.*, 'Finalization in Science', 326.

[291] Krohn and van den Daele, 'Science as an Agent' (1998). This article concludes a series devoted to 'Revisiting the Theory of "Finalization in Science"', in *Social Science Information,* Vols 36 and 37.

[292] Krohn and van den Daele, 'Science as an Agent', 192.

[293] *Ibid.*

[294] With this date, as in my presentation of the Starnberg institute generally, I refer to Abteilung von Weizsäcker only. Abteilung Habermas, not part of the original conception, came into existence two years later, in the autumn of 1971. However much respect Habermas had for von Weizsäcker personally, Habermas had little respect for the way in which the fundamentally social–scientific and philosophical projects in Abteilung von Weizsäcker were pursued, or for the wissenschaftlich character of von Weizsäcker's researchers. This lack of confidence was an important factor in the debacle that brought the entire institute to an abrupt end shortly after the closing of Abteilung von Weizsäcker upon his retirement on 30 June 1980: Habermas, 'Warum ich die Max-Planck-Gesellschaft verlasse'. Although Habermas here makes an exception of the finalization theorists—'Bei der von neokonservativer Seite angezettleten Polemik gegen die sogenannte Finalisierung der Wissenschaft bin ich … für Mitarbeiter von Herrn v. Weizsäcker in die Bresche gesprungen; damals konnte ich für die umstrittenen Projekte mit Überzeugung eintreten'—the bibliography relating to that controversy compiled by Schäfer, 'The Finalization Debate', intended to be complete, contains no publication by Habermas, nor is any likely-looking title to be found in Douramanis's Habermas bibliography (Douramanis, *Mapping Habermas*). On the other side, Weizsäcker, 'Erforschung der Lebensbedingungen', 464, while acknowledging that Habermas 'sowohl hinsichtlich der Rechtsstaatlichkeit, Gewaltfreiheit, und Toleranz wie hinsichtlich der unnachsichtigen Forderung wissenschaftlicher Strenge niemals zu Kompromissen bereit gewesen ist', clearly, and rightly, thought Habermas incapable of understanding natural scientists, how they think and work.

[295] Waters, *Daniel Bell*, 11–12, 16, 165.

[296] Bell, *The Coming of Post-Industrial Society*, ciii, civ, 36. Waters, *Daniel Bell*, 14–16. A helpful *Lebenslauf* is available at http://www.kfunigraz.ac.at/sozwww/agsoe/lexikon/pdfs/bell.pdf (accessed 23 July 2005).

[297] Bell and Graubard, *Toward the Year 2000*. Bell's American Academy Commission on the Year 2000 took 'it as a given that we are moving into a postindustrial society' (pp. 325–6). By the time Bell, *The Coming of Post-Industrial Society* came out Bell had already succeeded in making 'post-industrial society' the single most widely employed conception of where the world was going. Indicative of the primacy that Bell attributed to science is that into this collective effort at social forecasting, he recruited scads of scientists but only one technologist, J. R. Pierce.

[298] Waters, *Daniel Bell*, 148–55. Waters (150) characterized Bell as a 'technology freek'. Mumford, 'Prologue to our time', 7, lumped Bell with McLuhan and Arthur Clarke—'those giant minds whose private dreams all too quickly turned into public nightmares'—as still equating 'this limitless mechanical progress' with human progress.

[299] Bell, *The Coming of Post-Industrial Society*, 14; and reaffirmed in Bell's foreword to the 1976 reissue, *ibid*., xciv. Böhme *et al*., *Finalization in Science*, 9, saw their conception of finalized science as that which would realize Bell's 'forecast that "theoretical knowledge will be the strategic resource of post-industrial society"'. Waters, *Daniel Bell*, 110, summarizing Bell's theses: '*The primacy of theoretical knowledge*. This is the defining "axial principle" of the post-industrial society … . Bell stresses that in a post-industrial society this knowledge is theoretical, rather than traditional or practical, in character. It involves the codification of knowledge into abstract symbolic systems that can be applied in a wide variety of situations. The scientist displaces the inventor; the econometrician displaces the political economist'. Consequently, as Waters writes, p. 109, 'Given that the generation of information is the key problem and that science is the most important source of information, the organization of the institutions of science, the universities and research institutes is the central problem in the post-industrial society.' Regarding such expectations from where we stand today in the unfolding postmodern life world, we can only shake our heads in wonder at such preposterous conceits.

[300] Bell, *The Coming of Post-Industrial Society*, 26.

[301] A revealing indication of whom Bell was listening to is the last and most fulsome acknowledgement in his big book's 'inventory of influences': 'And finally, in this inventory of influences, I would single out an essay by the physicist and historian of science, Gerald Holton (Holton, 'Scientific Research and Scholarship'), in illuminating for me the significance of theoretical knowledge in its changing relation to technology, and the codification of theory as the basis for innovation not only in science, which Holton demonstrated, but in technology and economic policy as well'.

[302] Gilman, *Mandarins of the Future*, 8, et passim, captured very well the feel and content of that modernist, technocratic ideology, except that he overlooked the central importance of the concept and ideal of meritocracy. Forman, 'In the Era of the Earmark', points to the changing connotations of the word 'meritocracy', and its fall from grace as ideal, as indicative of the modern-to-postmodern transition.

[303] Bell, *The Coming of Post-Industrial Society*, 344–5. By 'the new intellectual technology' Bell did not mean material technology informed or directed by theory, but rather a sort of technocratic policy science; thus the 'engineers' referred to are not engineers as conventionally understood, but rather social engineers: 'What is distinctive about the new intellectual technology is its effort to define rational action and to identify the means of achieving it' (*ibid*., 30). Similarly, Waters, *Daniel Bell*, 111. Veysey, 'A Postmortem', 51, recognizing Bell's mandarinism, was offended that humanistic scholars were excluded from his ruling class; Brick, 'Optimism of the Mind', 353, simply denied that when Bell 'defined postindustrial society as "one in which the intellectual is predominant" … he meant … that intellectuals became a new ruling class'.

[304] '… the expansion of science and scientifically based technology is creating the framework for a new social order that will erode capitalism, as the activities of the merchants and the bourgeois outside the landed economy undermined feudalism, … the significant fact is that most of the activities of science are outside the business system and the organization of science policy is not, in the first instance, responsive to business demand. The necessary foundation for any new class is to have an independent institutional base outside the old dominant order. For the scientist this base has been the university'. Bell, *The Coming of Post-Industrial Society*, 232. Even as Bell was writing this, and certainly as he reaffirmed it in 1976, the changes in culture, economy, and polity that would render this obviously sheer fantasy were under way. They would be pointed out, soon after, by Dickson and Noble, 'By Force of Reason'. More generally, the thesis of Noble, *America by Design* (1977), is the antithesis of Bell's, and thus the

more prescient work, by far. Cf. however, note 15, above, re: Dickson and Noble and the primacy of science for technology—on which they had no disagreement with Bell.

[305] Similarly, Kenneth J. Arrow wrote in his blurb for this reissue, 'Daniel Bell introduced the concept of post-industrial technology in the first edition of this work, and it has now become universal'. However, Rose, *The Post-Modern and the Post-Industrial*, 29, was right that in 1973 Bell 'sets out to propose that the "axial principle" of the post-industrial society will not be technology but theoretical scientific knowledge'.

[306] Bell, *The End of Ideology*. In an 'Afterword' to the 1988 reissue of the book, Bell gave clear indication of the French postwar intellectual scene as a main point of reference for the work (410–11) as well evidence of the enormous controversy and literature that it generated (420)—much of it, to be sure, resulting from reading only the title and not the book.

[307] Bell, *Vers la société post-industrielle*.

[308] Lyotard, *The Postmodern Condition*, xxv; Rose, *The Post-Modern*. Acknowledging intellectual debts was one of those modern conventions that Lyotard consistently flouted, but this report on knowledge was an exception, with many footnotes citing many works, including Bell's. It is possible to see how central Bell's post-industrial was to Lyotard's thinking about this time in Lyotard's preparatory notes for the mammoth art-and-technology exhibition 'Les Immatériaux', as published in Centre de Création Industrielle, *Les Immatériaux*. Interestingly, Crowther, '*Les Immatériaux*', 196, found in Lyotard's rationale for the aesthetic of that exhibit 'a persistence of *modernist* attitudes'. Yet however much Lyotard's unifying conception for this exhibit reflected the continued primacy of the conceptual and thus of science, the exhibit as such reflected much rather the new alliance of art with technology, in itself a distinctly postmodern phenomenon.

[309] Although some of what Lyotard found in Bell, *The Coming of Post-Industrial Society* is inherently postmodern, specifically Bell's idea that a post-industrial society is a 'game between persons' (116).

[310] Browning, *Lyotard and the End of Grand Narratives*, 28–31.

[311] Jameson in the foreword to Lyotard, *The Postmodern Condition*, xx. Lyotard's 'Introduction', *ibid*., xxiii–xxv, immediately makes clear that science, and more particularly the 'altered game rules for science', is the principle subject of 'the present study'.

[312] Lyotard, *La condition postmoderne*, 74–7; *The Postmodern Condition*, 45–7.

[313] On Heidegger's influence among French philosophers of Lyotard's generation see note 37, above. Lyotard does not, however, refer directly to Heidegger in *The Postmodern Condition*.

[314] Lyotard, *La condition postmoderne*, 77; Lyotard, *The Postmodern Condition*, 47.

[315] Staudenmaier, 'Recent Trends', 715.

[316] So, for example, Segal, in his introduction to Ezrahi *et al*., *Technology, Pessimism, and Postmodernism* (1995) 2–3, and Marx, 'The Idea of "Technology"', 252–7, his contribution to that volume, were misled by their own antipathy to technological optimism into linking postmodernism with technological pessimism. Segal found on looking around that, in contrast with the situation in 1979, 'technological pessimism has become an integral part of the emerging culture of postmodernism'—where in fact just the opposite is the case. Reprinting 'The Idea of "Technology"' in Smith and Marx, *Does Technology Drive History?*, Marx acknowledged no contradiction to his pessimism thesis but did acknowledge in his brief introduction with M. R. Smith, 'the growing credence given [in 'popular discourse'] to the idea of technological determinism', 'the increasingly strong hold of that claim on the public imagination'. Indeed, Marx and Smith went so far as to allow—and I have not seen the like anywhere else in the writings of historians of technology—that 'if any particular form of human power now has an outstanding claim to that distinction [viz. driving history], it probably is technological power'.

[317] Mehrtens, 'Gilt das Trennungsaxiom? Zum Verhältnis von Wissenschafts- und Technikgeschichte' (1995), 251, observed that 'Das historiographische Trennungsaxiom für Wissenschaft und Technik ist unsinnig. Aber die Trennung ist nicht zu leugnen. Sie hat ihre eigene Geschichte, die noch zu schreiben ist'. I have not that wide knowledge of the writings and

relations of historians of technology as would be required to write that intellectual history of the discipline and its (unilateral) divorce from science. Rather, my intent is to draw attention to an interconnected set of orthodoxies prevailing in the discipline today, and to their consequences for the historiography of technology, but to do so from a 'genetic' perspective, so far as analytic reflection and my limited knowledge of those writings permit. My exposition and argument relate almost solely to the US division of the history of technology discipline. As pointed out by Rürup, 'Historians and Modern Technology' (1974), 170–1, the efflorescence of research and the formation of institutions was occurring from the mid 1950s onward not only in the US, but also in East and West Germany, France and Britain. How far these other national divisions of the history of technology discipline developed the same ideologies and orthodoxies, and in what tempi, I am even less well equipped to address. As indicated in the next note, there has been some animosity between the British and the US divisions. Rürup's paper ('Historians and Modern Technology', 171, 173, 186–7), originally published in German in 1972, gives evidence, and is itself evidence, of an ideological development of the history of technology in West Germany largely parallel to that in the US. In regard to the history of science, however, Mehrtens' observation that 'There are historians of science like historians of technology who say that history of science and history of technology are two essentially different things which one should keep separated' (p. 229), suggests some difference between the two countries: by 1995 it was as hard to find American historians of science who accepted this axiom of separation as it was easy to find historians of technology who affirmed it. (I am indebted to Oskar Blumtritt for drawing my attention to Mehrtens's paper.)

[318] So also have the SHOT-allied European historians of technology. So, e.g. Jamison in *Technology and Culture* (1989), 518, has *Technics and Civilization* as Mumford's 'magnum opus … which, more than any other single book, would serve to define the history of technology as a distinct historical specialty'; and again, in 'American Anxieties', 78, Jamison says that Mumford's work 'gave rise to' history of technology. See, likewise, note 226, above, and 323, below. There has been, however, a number of more discerning European historians of technology for whom 'It is a melancholy thought that far from sinking under ridicule', *Technics and Civilization*, 'this hotch-potch of commonplaces, unchecked assertions ("the mechanical arts advanced as the humane arts weakened and receded") and historical errors has passed for an original work', so that the broad public has acquaintance only 'with a certain kind of history of technology, that which seeks to overrule the facts'. Daumas, 'The History of Technology' (1969), 95. Daumas' essay was translated by A.R. Hall and published in 1976 in the first volume of the British annual, *History of Technology*, as a shot at the SHOTites, who for 20 years had been belittling the 5-volume Oxford *History of Technology*. (Especially provoking, presumably, was Layton's attack on Hall in 1974 referred to in note 352, below.) Hall expressed his indignation 'that the prosecution of the technical history of technology as envisioned by [the great French historian Lucien] Febvre, discussed by M. Daumas and practiced (to the best of their powers) by many writers who have considered themselves historians of technology, should now be largely abandoned in favor of a Mumfordian study of inter-relations between technology and society' (87).

[319] Kranzberg co-opted Mumford to his 'Advisory Committee for Technology and Society', created to create support for the creation of the Society for the History of Technology and *Technology and Culture*. Kranzberg, 'At the Start', 10. Mumford then served on the Executive Council of the Society, and in 1969 Mumford was awarded the Society's highest honor, the 'Leonardo da Vinci Medal', q.v. The seven previous recipients were all distinguished by their original research in the history of technology—all, that is, except Kranzberg himself.

[320] By Burlingame ('The Hardware of Culture', 17) and Allen ('Technology and Social Change', 50) in their texts, and by Multhauf ('The Scientist and the "Improver"', 46) in his notes.

[321] Mumford, 'Tools and the Man'; 'History: Neglected Clue'; 'Authoritarian and Democratic Technics'; 'Man the Finder'; 'Technics and the Nature of Man'. Kranzberg included 'Authoritarian and Democratic Technics' and 'Technics and the Nature of Man' in his

selection of 21 articles from the first 10 years of *Technology and Culture* to form Kranzberg and Davenport, *Technology and Culture*, where he also included two articles each by Lynn White, Jr., and Peter F. Drucker.

[322] Hughes and Hughes, *Lewis Mumford*, vii; Hughes, 'Machines, Megamachines', 107; 'The Seamless Web', 291. See, also, notes 228 and 252, above.

[323] For comparison consider Lynn White, Jr. The third Leonardo da Vinci medalist, White was the scholarly historian of technology of greatest prominence in America in the postwar decades. Advancing sweeping theses on the basis of his own research on medieval technology, White is the only person ever to have served both as president of SHOT and as president of the American Historical Association. In the first 10 years of *Technology and Culture*, 1959–68, there were 23 articles and four book reviews referring to White, and 20 articles and five book reviews referring to Mumford. Thirty years on, during the decade 1989–98, with the number of articles published by *Technology and Culture* remaining roughly constant, but with a great increase in the median number of works referenced in them, and likewise a great increase in the number of book reviews published, only 16 articles and 12 book reviews in *Technology and Culture* referred to White, while 22 articles and 28 book reviews referred to Mumford. The only scholars having today as many or more references as there are to Mumford (8) in the index to the Blackwell *Companion to American Technology*, edited by Pursell, are Thomas Hughes, Leo Marx and Purcell himself. (I have excluded the contributors self-citations.) Going a bit beyond the historians to 'science and technology studies': Jamison, 'The Making of Lewis Mumford's *Technics and Civilization*' (1995), rated the book 'as perhaps the single most valuable work ever written in the field of science and technology studies, … . The book itself created a new field of study: history of technology.' As a measure of the importance Mumford holds for the field of the philosophy of technology, Mitcham, certainly the most broadly learned among those engaged in this field, in his survey *Thinking Through Technology* discussed four writers under the rubric 'humanities philosophy of technology': Mumford, Ortega y Gasset, Heidegger and Ellul, in that order.

[324] Mindell, *Between Human and Machine*, 1.

[325] Williams, 'Lewis Mumford as a Historian of Technology in *Technics and Civilization*' (1990), 63, is quoting Mumford, 'An Appraisal of Lewis Mumford's', 530–1: 'The main value of these early chapters [of *Technics and Civilization*] was a shift in the whole point of view, which make technics an integral part of higher civilization'. Williams's characterization of Mumford's treatment of technology as 'untraditional' in its elevation of technics into the sphere of higher culture is also that of Burlingame in the first article in the first number of *Technology and Culture*: 'Whether or not one agrees with his epochal classification, there can be little question that the book [*Technics and Civilization*] is a pioneer—perhaps the first in completeness of pattern—in breaking the tradition about which Macaulay complained and which had held so firm a grip on historians. To me it not only gave the inspiration that stretched my project into three volumes, but it reformed my entire attitude toward cultural history'. Burlingame, 'The Hardware of Culture', 17; see also p. 15. This elevation of hardware into higher culture was, however, quite the opposite of Mumford's own conception and intention, as he laid out clearly in 1960 in a paper presented in opposition to Burlingame's at SHOT's first meeting in conjunction with the American Historical Association: Burlingame, 'Technology: Neglected Clue'; Mumford, 'History: Neglected Clue'. It bears mentioning that Burlingame, not a trained or professional scholar, but rather a prolific writer of novels, biographies, institutional histories, and latterly history of American technology as history of invention, did not account science any less highly, nor any less fundamental to modern technology, than did Mumford: 'I have', he wrote, 'of course, had to meet the increased difficulties of the technics themselves with an untechnical mind and so have presented the brief descriptions of the inventions as a layman must do in the common terms with which I am familiar. With the additional momentary glimpses I have had of the workings of physical law in the devices of the later inventors, I

am more than ever humble before the great mystery which is called Science'. Burlingame, *Engines of Democracy*, viii.

[326] Williams, 'Classics Revisited', 140. Williams there fashions a claim for Mumford's achievement by following Mumford's program—*Technics and Civilization* having begun as 'Form and Personality'—rather than accepting Mumford's later claims, themselves already much influenced by the acclaim of the historians of technology. Certainly, Williams's 2002 claim corresponds more closely to Mumford's committed subordination of the material to the mental than does her 1990 echo of Mumford's 1959 and 1970 claim of elevation of technics to high culture status. Still, Williams's 2002 claim corresponds poorly to what Mumford in fact argued in *Technics and Civilization*, where, in typical romantic fashion, his concern was not at all to show the determination of technology by personality, but the (deplorable) determination of personality by technology. This Williams in effect says herself (143–4) when she gets down to specifics, observing that Mumford alleged that 'members of certain occupations (miners, monks, soldiers, financiers) were "mechanized" by their tools and other "external" conditions associated with their work'. Similarly, in those few places where Mumford does have personality determining technology, it is the deplorable personality of the engineer producing deplorable results in society.

[327] In 2002, Williams implicitly admited this (139): 'Writing this review has been an effort to recover a usable Mumford'. Williams is here, as she is explicitly in Williams, *Retooling: A Historian*, grappling with the fact of male primacy (in modernity).

[328] Molella, 'Mumford in Historiographical Context' (1990), 41–2; Molella, 'The First Generation: Usher, Mumford, and Giedion' (1989), 91, 99.

[329] See notes 251–7, above. I have in preparation a paper considering more closely the development of Mumford's only briefly positive valuation of technology and of engineers.

[330] Quotations from Mumford, 'History: Neglected Clue to Technological Change' (1961), 232.

[331] As Fores, '*Technik*: or Mumford Reconsidered' (1981), 121–2, observed with uncharacteristic understatement, 'Mumford turns out not to have separated "science" from "technics" as effectively as some of his admirers may believe'.

[332] Molella, 'Mumford in Historiographical Context', 41. In 'The First Generation: Usher…', Molella similarly made no mention of the fact that Usher too presupposed the primacy of science for technological advance throughout history. An entire chapter of Usher's *A History of Mechanical Inventions* (in both the 1929 and 1954 editions) is devoted to 'The Early History of the Pure and Applied Mechanical Sciences'. Although it might seem to us that the interests and methods of the economic historian should have rendered Usher immune to the presupposition of the primacy of science, he, like Mumford and moderns generally, insisted that 'The more important aspects of the history of science must be included in any serious analysis of the history of mechanical invention' (1954 edn: 57).

[333] Although the words 'science' and 'scientific' appear nowhere, the word 'scientists' does appear once (147)—not as an intentional recognition of scientists figuring importantly in *Technics and Civilization*, but as an inadvertent revelation of Williams' acceptance of the conventional view that our contemporary 'technological world' is created by 'scientists and engineers'. In 'Review of Joel Mokyr, *The Gifts of Athena*', Williams opened by acknowledging that 'A primary contribution of this important book is to reframe the problem of the relationship between science and technology', adding that 'This is a well-worn—some would say worn-out—intellectual path, which Mokyr reworks to make it into a more promising route for future inquiry'. That uninformative concession is all the attention that that 'primary contribution of this important book' got in Williams's long review. In *Retooling* (2002), Williams began with a nostalgia trip to the farm on which her engineer grandfather grew up, and went on to represent the culture and identity of the engineer as being disintegrated by the disappearance of the boundary between science and technology. She did not care to see that in their ideology the engineers had erased this boundary more than a century ago, and thus the more recent *de facto* erasure is of far less significance for engineering than it is for science, which, by

contrast, had insisted throughout modernity on the fact, and the essentiality of the fact, of such a boundary.

[334] Williams, 'Classics Revisited', 143–4. Cf. note 239, above. The reference to 'financiers'—so oddly assorting with these lowly occupations—should be understood as Williams's commuted reference to scientists, for in Mumford's account the mechanistic mentalité of financiers and scientists represented essentially the same apprehension of the world. So, in *Technics and Civilization*, 25, where Mumford quoted Veblen for this thesis.

[335] Williams, 'Lewis Mumford', 48. In a milder form one can see the same animus in the writings of Williams's mentor, Hughes. The one point in Hughes and Hughes, 'General Introduction' to their 1990 volume of essays where they show annoyance and impatience with Mumford, finding in him 'an unexpected naïveté', is when noting that Mumford 'had idealized nineteenth-century physicists like Michael Faraday and biologists such as Louis Pasteur' (8). In *American Genesis*, Hughes has almost nothing to say about science—notwithstanding that 1870–1970 is almost precisely the period in which American enthusiasm for invention and technology was accompanied by a nearly universal attribution of primacy to science in relation to technology. On the few pages where science appears (48–52) Hughes dwelt upon such antagonistic and dismissive attitudes toward science as he was able to find among the independent inventors whom he there celebrates. See, likewise, note 360, below. This antagonism toward science was not yet present, however, when Hughes was writing the 'Introduction' to his compilation *Changing Attitudes Toward American Technology* (1975).

[336] Staudenmaier, *Technology's Storytellers*, 1. The book is an extension of a dissertation completed under Hughes in 1979. Regarding inversion in disciplinary creation myths see Forman, 'The Discovery of the Diffraction of X-rays'.

[337] Staudenmaier, *Technology's Storytellers* (1985), 1. Staudenmaier presented the story as fact, but in the form of Kranzberg's recollection, as communicated in a letter to him. When repeating the story in 1990 in his own words Staudenmaier, 'Recent Trends in the History of Technology', 715, implicitly cast the matter in quite a different light by presenting the group calling upon Guerlac as representing an already existing Society for the History of Technology.

[338] Seely, 'SHOT, the History'. Sinclair, 'The Road to Madison', S8–9, showed an indicative indifference to the fact of its being a myth and delight in repeating the alleged grievance: 'in SHOT's mythology, 1957 is a key date precisely because in that year Henry Guerlac categorically rejected the idea that *Isis* publish articles in the history of technology—an action that directly led historians of technology to found a separate organization. *Technology and Culture* editor Bob Post believes that story may be a myth, which is to say, evocative though not entirely true. But, as myths are supposed to do, it worked on us.' Fitzgerald, 'Review', even as she maintained that 'younger scholars are sometimes baffled by this apparent tension' and that 'the disputes of an earlier generation are not their own', thought the myth worth repeating in her review of the two volumes of proceedings from that 1991 conference in Madison, and for something like the same reason: 'some historians of technology claim that it was … 1957 when *Isis* banished from its pages articles on the history of technology, which act did in fact lead to the founding of SHOT. Apocryphal or not, this tale and its retelling signal the continuing tensions between the two fields.'

[339] Layton, in his 'Eloge' for Kranzberg in *Isis*, observed irenicly that 'Perhaps too much emphasis has been placed on the confrontation of Mel and a group of historians of technology with the then-president of the History of Science Society, Henry Guerlac. A separate society concerned with technology had been in the cards for some time. Three years before the formation of SHOT, Mel had helped bring together a critical mass of historians of technology and engineering educators in the American Society for Engineering Education, where Mel was Secretary of the "Humanistic-Social Division" from 1955 until the founding of SHOT in 1958'. Regarding 'careerism' as underlying this institutionalizing, David F. Noble's strong views expressed at 'The Roanoke Conference: Critical Issues in the History of Technology' (1978) are reported by Roland, 'What Hath Kranzberg Wrought?', 698–9. Although Staudenmaier, 'What SHOT

Hath Wrought', 707, thought that 'little can be learned from our formal name except that the history of technology was the group's focus', Kranzberg, 'At the Start', had made clear the intent to recruit constituencies with primarily presentist interests, what is reflected in the omission of history from the title of the society's journal, and is reflected more weakly in the society's name, which is contrived to commit its members only to advocacy 'for' the history of technology without affirming the member's identity as historian of technology.

[340] Seely, 'SHOT, the History of Technology', 772. This was still the dominant approach among European historians of technology as late as the mid 1970s. See note 318, above.

[341] Staudenmaier, *Technology's Storytellers*, 83. Or, as Staudenmaier put it more pointedly in his front matter (p. xxi): 'the science versus technology theme'. Appendix 3, Staudenmaier's classification of the 272 articles in 21 years of *Technology and Culture*, has 109, 40%, dealing in one way or another with that theme. Cf. Staudenmaier, 'What SHOT Hath Wrought', 714.

[342] Writing in *Science* in 1962, Kranzberg, 'The Newest History', 466, could say that 'To the degree that technology is concerned only with the making of physical objects, it lies in the realm of the physical sciences'. In 1966/67 Kranzberg, 'The Unity of Science–Technology' was still looking toward a future in which science and technology are fused—technoscience, as it were—but a year later, 'The Disunity of Science–Technology', 32, he was insisting on 'the persistence of the dichotomy between the two'. Hindle, *Technology in Early America* (1966), 4–6, which book represents his reorientation from history of science to history of technology, noted 'tensions surrounding the relationship' but also insisted that the historian of technology 'must certainly be cognizant of the state of science in the period whose technologies he studies'—i.e. evidently the opposite was already being maintained by some historians of technology.

[343] Not in Layton's writings only: Sinclair's early work similarly took science as the higher good guiding technology (and scientists as the more perfect embodiment of it). Thus Sinclair's preface to *Early Research at the Franklin Institute…1830–1837* (1966) opened: 'The Franklin Institute's boiler explosion investigation was clearly the most outstanding scientific accomplishment in the organization's early history. It involved a brilliant group of young scientists.' Sinclair remained of that view in writing his fuller account of the organization's early history in *Philadelphia's Philosopher Mechanics: A History of the Franklin Institute, 1824–1865* (1974). Setting the theme of his exposition by quoting Dupree, 'The History of American Science', 863—'science is a thread woven into the very fabric of American civilization from the beginning'—Sinclair himself added (2) that 'science also implied a technology inspired by scientific principles'. But by the time Sinclair had come to write the preface to that book, he no longer wished to emphasize science. It opens: 'Technology has been a central force in the American experience … closely tied to a democratic ideology … America's technology would prove the virtue of her political system'. In writing *A Centennial History of the American Society of Mechanical Engineers, 1880–1980* (1980), Sinclair knew from the outset that there was no place for science in *his* history of technology, and so it simply does not appear, notwithstanding that it held great importance for the engineers whose history he wrote.

A bit of the same reorientation can be seen between two presentations by Pursell of the matter of mobilization and organization of American engineers in response to the First World War. In '"What the Senate Is …": A National Academy of Engineers' (1986), Pursell was by no means pro-scientist, but the engineers' failures were of their own making. Twenty years on, in 'Engineering Organization and the Scientist in World War I', Pursell is emphatically with the engineers in what he presents as a struggle over the 'commanding position' in contributing to the war effort and hence in industrial research after the war: 'The result was an almost complete victory for the scientists, one that persists to this day' (258). (Evidently antagonism toward science is no help to a historian of technology in recognizing the postmodern reversal of rank and role between science and technology.)

[344] Layton, *The Revolt of the Engineers*, 1

[345] *Ibid.*, 58. Layton's original sociological orientation remains evident there; it is clearer still in the concluding paragraph of his 'Veblen and the Engineers' (1962). Fores recognized Layton's

underlying scientism and assailed it, both in '*Technik*: or Mumford Reconsidered', 123, and in later, less well-considered, papers. McGee, 'Making Up Mind: The Early Sociology of Invention', opens with an indirect but telling critique of Layton's response to Fores, pointing out that Layton's intellectual orientation derived from sociology.

[346] Layton, 'Mirror-Image Twins'. The citation with the award of SHOT's Leonardo da Vinci Medal to Layton in 1990, published in *Technology and Culture* 32 (1991): 579, 581, points to *Revolt of the Engineers* and to 'The other celebrated area of Layton's work … a series of articles on the nature of engineering knowledge and the relationship between science and technology, most famously his 1971 article "Mirror-Image Twins: The Communities of Science and Technology in 19th-Century America." His rejection of the notion that technology was applied science, and assertion of the essentially symmetrical nature of the relationship between science and technology, are now widely accepted. Layton was not alone in this crucial reassessment of previous conventional wisdom, but when written, "Mirror-Image Twins" was heterodox enough almost to have been rejected by *Technology and Culture*'. (Heterodoxy? Recall how greatly the SHOTites pride themselves upon the fact that 'openness remains one of SHOT's most attractive characteristics and greatest strengths … there is no party line to which all must conform', to quote Layton's response as medalist (585). Could perhaps the alleged suggestion of rejection of the paper have simply reflected doubts that the paper was so important as to warrant dual publication—for it was already committed to the proceedings of the conference at which it was presented: Daniels, *Nineteenth Century American Science*.) The intellectual importance that historians of technology ascribe to this essay is disproportionate, and the more so as the essential conceptual element in the 'mirror image' metaphor is nearly always overlooked—as here—so as to effect an ideological elevation of technology both greater and more simple-minded than that which Layton intended. Thus Laudan, 'Natural Alliance' (1995), S19: 'One important step toward a more equitable treatment of the two enterprises'—i.e. technology and science—'came in the early 1970s, with Edwin Layton's proposal of a separate-but-equal, or mirror-image twins, model. According to this, technology was not merely applied science but its mirror image. Separate but equal, like science it had its own institutions, its own values and methods, and its own kind of knowledge.' And in a footnote she added: 'To a quite significant extent, this is still the official posture of historians of science and technology.' Indeed, 'the official posture of historians of technology', but surely not of historians of science, if only because *they* have no 'official posture'. Cf. note 50, above.

[347] Layton emphatically did not intend equivalence with his metaphor of 'mirror-image twins', what only Cowan, 'Technology Is to Science', 580, coming from the history of biology, seems to have recognized. Mirror symmetry implies inverted parity, and therewith the introduction of something left-handed, something sinister. Layton chose this metaphor for that reason: 'In the case of mirror-image twins there is a subtle but irreconcilable difference that is expressed as a change in parity. Between the communities of science and technology there was a switch in values analogous to change in parity' (Layton, 'Mirror-Image Twins', 576). Moreover, Layton had no doubt that the values of science were right-handed, the more desirable values; those of engineering left-handed, less desirable. So, Layton explained, 'The reversal of "parity" between science and technology further reduced the engineers' ability to respond effectively to social problems' (*ibid*., 579–80). See note 366, below. Today, in postmodernity, we are experiencing the inverse: science is exchanging its distinctive values for those Layton regarded as the less desirable values of engineering.

[348] Layton, 'Mirror-Image Twins', 562, 567–8. Layton did not cite Price, 'Is Technology Historically Independent of Science?' (1965), but that essay in *Technology and Culture*, which was then rightly drawing much attention (e.g., Fores, 'Price, Technology, and the Paper Model'), contained the kernel of Layton's mirror image metaphor in a more imaginative and testable form.

[349] Layton, 'Mirror-Image Twins', 562–3.

[350] *Ibid*., p. 563. See text at note 179 for passage in Bush, *Science, The Endless Frontier*.

[351] Layton, 'Technology as Knowledge', 34.

[352] *Ibid*. Ironically, it was historians of *science*, not historians of technology, who, beginning in the late 1950s, took the lead in expressing skepticism regarding the significance of science for technological innovation prior to the 19th century—even prior to the *late* 19th century. For example, Hindle, *Technology in Early America* (1966), 86, cited only historians of science for this new skepticism, about which he remained non-commital. Layton acknowledged, but only implicitly, some pages later, in a footnote (note 29 on p. 39), that this argument stemmed from historians of science: 'The insufficiency of the established model of science–technology relations has been shown by a large number of studies, too many to cite here. Some of the ones that have influenced me are …'. Following which, Layton cited one work each by Robert P. Multhauf, Charles C. Gillispie, Thomas S. Kuhn, Derek J. de Solla Price and 'M. Gibbons and C. Johnson'. (Layton should have included A. R. Hall, who had been in the forefront of the endeavor to decouple technology from science in the early modern period—see Mayer, 'Setting Up a Discipline'—but Layton chose to make Hall the villain of the piece.) None of the scholars listed was a historian of technology, apart from Multhauf, whose affiliation with the history of science was at least as close as with the history of technology. Mollela, although then still a historian of science, was not among those showing the way. On the contrary, as pointed out above, note 69, he and Reingold, 'Theorists' (1973; 1991), 128, 139, dismissed this 'chorus' of voices denying the importance of science for technological advance in earlier centuries as being raised with 'usually no evidence whatsoever'.

[353] Layton, 'American Ideologies', 689, quoting Bush, *Science, The Endless Frontier*, 13–14: 'In the nineteenth century, Yankee mechanical ingenuity building largely upon the basic discoveries of European scientists, could greatly advance the technical arts. Now the situation is different. A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill.'

[354] Layton, 'American Ideologies'. Cf. Mumford's statement, more than 50 years earlier, quoted in note 237: 'The actual world of machinery is at present, it seems fair to say, a parasite upon this body of knowledge', i.e. upon the physical sciences. Nor would it be unfair to say that American engineers themselves represented the relationship in that way, especially in adopting Huxley's thesis re: 'applied science'. (See notes 158–62, above.)

[355] Forman, 'From the Social to the Moral'. Rose, 'The Historiography', 34–5; Hounshell, 'On the Discipline of the History'.

[356] Staudenmaier, *Technology's Storytellers*, xxi, 102, 103.

[357] *Ibid*., 83–5. Much the same is asserted on 89–90, 96–8.

[358] Staudenmaier, 'What SHOT Hath Wrought', 714; Staudenmaier, 'Recent Trends in the History of Technology' (1990), 718. Consistent therewith, Staudenmaier, *ibid*., 717, ignored science in his analysis of 'the processes that produce new technologies'. This step to exclusion of science from the purview of the historian of technology was resisted by Kranzberg, 'Let's Not Get Wrought Up', whose policy was never to exclude. It presented a real difficulty for Layton, whose endeavor was to redefine, not to ignore, the technology–science relation. Nonetheless, in his 1986 SHOT presidential address, Layton paraphrased Staudenmaier's argument as 'Historians of technology had to demolish the myth that technology was no more than applied science in order to establish the legitimacy and autonomy of their own discipline. But once they were successful in this, they found that it was not really the interaction with which they were primarily interested. Rather, the emphasis lay in the nature of technological knowledge'. Layton commented there: 'I agree with Staudenmaier, although I have a few small qualifications'. Layton, 'Through the Looking Glass', 600.

[359] A third course, emphasizing neither autonomy from nor ignoration of science, but seeking liberation from subordination to science through obliteration of the distinction between technology and science—and, more generally, denial of all distinctions, and thus of all hierarchies, among human activities—was that proposed by Hughes, centering at this time on the 'seamless

web' of history–society–reality. Thus where the autonomists sought to achieve the liberation of technology from science's primacy by a parochial program restricting the compass of the history of technology to technology's cognitive essence, Hughes's program, proceeding in the opposite direction, denying the existence of any borders and any hierarchies, meshed easily into the postmodern reversal of primacy occurring among sociologists of scientific knowledge in the mid 1980s for which Latour's thinking was exemplary. Hughes was party to the first major foray by those sociologists into technology, a 1984 workshop and ensuing publication: Bijker *et al.*, *The Social Construction*. That book's radical denial of a distinction in principle between technology and science is to be found only in an unsigned 'Introduction' (11) obviously written by Hughes, where he foisted upon Bijker and Pinch a more radical view than any that they themselves there expressed.

[360] More than 10 years ago Laudan, 'Natural Alliance or Forced Marriage?', S19, had already drawn attention to the absence of science from then recent synthetic works in the history of technology, directing her readers to 'See, e.g., Alan Marcus and Howard Segal, *Technology in America: A Brief History* (New York, 1989); George Basalla, *The Evolution of Technology* (Cambridge, 1988); and Thomas P. Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870–1970* (New York, 1989). In none of these texts does science get more than a passing reference'. Siding as she did with the historians of technology, Laudan excused them with a *tu quoque*, pointing out that 'David Lindberg's recent, and masterful, *Beginnings of Western Science: The European Scientific Tradition in Philosophical, Religious, and Institutional Context* (Chicago, 1992) leaves all technologies, except the medical, to one side'. As though failing to discuss technologies (except the medical) in connection with medieval science were as little justifiable as failing to discuss sciences (any at all) in connection with modern technology.

[361] Claiming that 'Layton again played a pivotal role' through his papers in the 1970s on technology as knowledge, Laudan, 'Natural Alliance', S22–3, judged that 'it is now generally accepted that there is something distinctive about technological knowledge and that it is neither irremediably tacit nor simply applied science'. The lack of any positive content in Laudan's characterization of technological knowledge rather underscores the unpromisingness of that approach. To my knowledge, Vincenti, *What Engineers Know* (1990), is the last serious endeavor along that line. When, in his SHOT presidential address, Roland, 'What Hath Kranzberg Wrought?', 709–12, stressed technology as knowledge, it was no longer in the sense of the Layton–Vincenti quest for a form of knowledge definitional of technology, but in the implicitly postmodern sense that knowledge is a *consequence* of technology, as it is of *every* social–cultural activity..

[362] So, for instance, Pursell, *A Companion to American Technology* (2005), with 21 chapters on various technologies and aspects of technology, has none on technology and science, and no entry in its index for 'science'. Similarly, Misa *et al.*, *Modernity and Technology* (2003), has no entry in its index under 'science', nor does 'science' appear among the 21 separate entries for 'technology'. Nor does Misa, in his introductory chapter, 'The Compelling Tangle of Modernity and Technology', mention science. Similarly, Misa, 'Theories of Technological Change' (1992), omitted entirely the matter of the relation between science and technology. In his most recent publications—'Beyond Linear Models', and 'Inventing Europe'—Misa appears to be abandoning that blinkered approach to modern technology. However, Schatzberg, '*Technik*' (2006), is still advancing in his concluding sentence the divorce of technology from science as motive and goal for his work.

[363] See Laudan's handling of the matter, quoted in note 360, above. Animosity is frankly admitted, nay, flaunted, by Sinclair, 'The Road to Madison' (1995). Notable for its subtext consistently denying and denigrating science is the collection of readings put together by Smith and Clancey, *Major Problems in the History of American Technology* (1998), with its chapters on the telephone as the product of spiritualism, and on radio as a product of hobbyists. The chapter on 'scientific management' serves as a vehicle for putting science (not engineering) in question,

as does its one chapter on science-based technology, devoted to insecticides. Happily science is absent from the chapter on 'the military–industrial–university complex'.

[364] Sinclair, 'The Road to Madison', S3, S10: 'For the sake of our institutional memory, the most important question is: Why did we go to Madison at all?', i.e. agree to a joint meeting with the History of Science Society on critical problems. 'Those of us on the SHOT side … intended to distinguish our sessions from theirs. As I think of it, of course, that *was* our principal objective. The crucial point for us was ownership of the history of technology and that meant defining for ourselves the teaching and research agenda of our field, separate and distinct from the history of science'.

[365] Postmodern is, of course, not necessarily postmodernist, but postmodernist is, at least to that extent, postmodern. I am aware of only one undeniably contributive historian of technology who is avowedly postmodernist: Philip Scranton. It is therefore in Scranton's case uncertain whether his anti-science attitudes stem mainly from his identity as historian of technology or his persuasion of postmodernism. Those attitudes have been clearly expressed in Scranton, 'Theory and Narrative' (1991), 386, 390; 'Determinism and Indeterminacy' (1995), S47. Indicatively, there (S35) where Scranton allowed that his analysis of the notion of technological determinism 'would be rejected by scores of historians in other fields for its insufficient attention to culture, markets, gender, the labor process, or the diversity of individual and institutional appropriations of technology, and/or for its elision of power relations', he did not mention attention to science.

[366] Wise, 'Science and Technology', 229, quoting Price, 'Of Sealing Wax and String' (1984), 49. (The pertinent paragraphs of this essay are included in the more gracefully worded abridgement by Merton in the posthumous collection of Price's essays, *Little Science, Big Science*, 239–40.) Broad, 'Does Genius or Technology Rule Science', writing in 1984 for *The New York Times,* represented Price as being, until his death in 1983, the leader of a 'rebellion', of a 'new school' arguing that new technologies, far from following necessarily from scientific discoveries, are themselves the cause of scientific discoveries. Broad pointed to Layton as his sole example of those who 'have violently opposed Dr Price's relegating the heros of science to the role of mere handmaidens. [Note the role reversal!] "Derek went way overboard," said Edwin T. Layton, president-elect of the Society for the History of Technology.' Given what historians of technology have made out of Layton, his reported resistance is puzzling. It fits, however, with the fact, pointed out above, note 347, and below, note 395, that Layton, early and late, did indeed rank science above technology.

[367] Wise, 'Science and Technology', 229.

[368] *Ibid*., 244, the opening of his 'Conclusion'. (Wise meant, of course, to say that 'Not all knowledge is science'.) Setting up the opposition as between science-policy wonks and historians allows Wise to evade the question when, why, how historians came to free themselves from this misconception; by his account, they never held it. Wise has it held only by 'a small elite of leaders, mainly drawn from academic science departments or deans' offices' (231).

[369] *Ibid*., 229–30. Behind the ellipses is an assertion that, like the assertion quoted immediately above, is formally incompatible with what follows—an incoherence that seems to be the product of Wise's agitation over the injustice done technology by the ascription of primacy to science. This sense of grievance is expressed almost hysterically in Wise's plaint that 'Bush's dream of a National Research Foundation supporting the work of modern-day counterparts of everyone from Einstein to the Wright brothers gave way to a National Science Foundation aimed at supporting Einsteins only' (231).

[370] *Ibid*., 230. How Wise's passion over the wrongness of 'technology-comes-from-basic-research' arose, is entirely unclear to me. I have seen none such in Wise, 'A New Role for Professional Scientists in Industry' (1980), or in Wise, *Willis R. Whitney… and the Origins of U.S. Industrial Research* (1985).

[371] MacKenzie, 'Marx and the Machine', 486–7, reprinted in MacKenzie, *Knowing Machines*—it is the only 'older' paper MacKenzie included there—and reprinted in other collections and

translated into other languages, it was, early on, awarded SHOT's Usher Prize for 1986 for 'best scholarly work published during the preceding three years under the auspices of the Society for History of Technology' (www.historyoftechnology.org/awards/usher.html). Directly engaging the question is Bimber, 'Three Faces'.

[372] MacKenzie, 'Marx and the Machine', 486. Marx's discussion occupies the first pages of *Kapital*, Dreizehntes Kapitel ('Maschinerie und große Industrie'), Abschnitt 1. ('Entwicklung der Maschinerie'), in Marx-Engels, *Werke* (1968), 23: 391–2, available at http://www.mlwerke.de/me/me23/me23_391.htm (accessed 1 December 2005). Marx, *Capital,* 1: 492–5. Marx's insistence there upon a conception of the machine as a mechanized hand tool reflects not his best thinking but rather the romantic view of the machine as mechanizing the worker. (Farther on in *Kapital* Marx showed a better appreciation of the abstract rather than replicative character of the machinery of 'grosse Industrie': see the passage quoted in note 74, above.) It has long been a commonplace that the conception of the machine as a mechanized hand tool, which Marx adopted at the outset of his discussion of mechanization and MacKenzie presented as cogent, was 'the great obstacle that 19th-century inventors had to overcome': Daniels, 'The Big Questions', 20; likewise Mumford, cited in note 239, above.

[373] MacKenzie, 'Marx and the Machine', 486. The passage from *Kapital* which MacKenzie quotes—he seems to know it only in translation (Marx, *Capital,* 1: 492–3); he acknowledges that he has himself italicized '*because the historical element is missing from it*'—is: 'Mathematiker und Mechaniker—und man findet dies hier und da von englischen Ökonomen wiederholt—erklären das Werkzeug für eine einfache Maschine und die Maschine für ein zusammengesetztes Werkzeug. Sie sehn hier keinen wesentlichen Unterschied und nennen sogar die einfachen mechanischen Potenzen, wie Hebel, schiefe Ebne, Schraube, Keil usw., Maschinen. In der Tat besteht jede Maschine aus jenen einfachen Potenzen, wie immer verkleidet und kombiniert. Vom ökonomischen Standpunkt jedoch taugt die Erklärung nichts, denn ihr fehlt das historische Element.' Though Marx, always polemical, is here rejecting the unserviceable definition of the 'Mathematiker und Mechaniker', it is evident that he respects it intellectually.

[374] Far more relevant is that Marx said, but MacKenzie omitted to say, that the same definition, unserviceable for Marx's purposes because ahistorical, occurs 'repeatedly among English economists'.

[375] MacKenzie, 'Marx and the Machine', 488–9. MacKenzie's reference is 'Ure, *The Philosophy of Manufactures* (London, 1835), p. 370, as quoted in Capital 1: 564'. The words are Ure's, not a back-translation of Marx's 'das Kapital, indem es die Wissenschaft in seinen Dienst preßt, stets die rebellische Hand der Arbeit zur Gelehrigkeit zwingt', except that Ure has 'in' not 'into'. The quotation appears at the bottom of p. 368 in *Philosophy of Manufactures* (1835), and on p. 460 in *Kapital,* Band I (4th edn), http://www.mlwerke.de/me/me23/me23_441.htm#Z186 (accessed 5 October 2006).

[376] MacKenzie, 'Marx and the Machine', 496: 'Marx's reliance on sources such as the writings of Ure meant that he had quite plausible evidence for what class-conscious capitalists hoped to achieve from the introduction of the machine. But what they hoped for was not necessarily what happened. Marx quoted Ure's judgment on the self-acting mule: "A creation destined to restore order among the industrious classes." Lazonick's work shows that the mule had no such dramatic effect'. However, Sherwood, 'Engels, Marx, Malthus', 849, found something rather dramatic in Lazonick: 'Far from providing, as Marx believed, the classic example of workers eliminated by the introduction of automatic machinery, the male spinners formed after 1850 "the best organized and the best-financed union in all of Britain".'

[377] Sherwood, 'Engels, Marx, Malthus', 851: 'Engels's assumption'—and Marx's too, for he relied implicitly upon Engels here—'about the consequences of the jenney and mule was typical of analysts of machine productivity at the time. Although their logic was clear, it was also false.'

[378] To be quite complete and accurate, 'science' does appear in one other quotation of Marx given by MacKenzie, 'Marx and the Machine', 488: 'The machine … embodies the power of the capitalist: "The special skill of each individual machine-operator, who has now been

deprived of all significance, vanishes as an infinitesimal quantity in the face of the science, the gigantic natural forces, and the mass of social labour embodied in the system of machinery, which, together with these three forces, constitutes the power of the 'master'."' (MacKenzie is quoting Marx, *Capital*, 1: 549 = Kapital, 1: 446, as made available at http://www.mlwerke.de/me/me23/me23_441.htm#Kap_13_4; accessed 5 December 2006.) This is the only place and manner in which the great importance that Marx attributed to science for the creation of machine technology is permitted to appear in MacKenzie's paper, while MacKenzie himself remains silent about it. Once again, among all the references to science in Marx, MacKenzie has chosen to exhibit one of the very few where it appears as a malevolent force. [If the reader finds difficulty in reducing Marx's rather obscure assertion—that the power of the capitalist is constituted by the system of machinery *together with* the three forces that are embodied in the system of machinery—to MacKenzie's 'The machine … embodies the power of the capitalist', that is in good part the fault of the translator, who, intending to clarify, has confused by interpellating the clause 'together with these three forces'.]

[379] Dupree, 'The History of American Science—A Field Finds Itself' (1966), is in large measure a polemic against John A. Kouwenhoven's pamphlet, *American Studies: Words or Things?* (1963). Skramstad, 'American Things: A Neglected Material Culture', continues the plaint of neglect—in an overview of *American Studies: Topics and Sources* that excludes science entirely—which plaint would become canonical also for the history of technology.

[380] Marx, *The Machine in the Garden*, repeatedly reprinted, most recently in 2000 with a new afterword. The book was an elaboration of Marx's Harvard PhD dissertation, 1950. The citations of Marx by historians of technology are, overwhelmingly, to this book only. Marx has had far more importance for SHOT than SHOT has had for him. Over the years Marx has published only twice in *Technology and Culture*, both publications being only book reviews. The second of those was a review of the Festschrift for Melvin Kranzberg, which Marx used as an occasion to deny the sense and value of a separate discipline of the history of technology—leading Kranzberg to suggest that the author of *The Machine in the Garden* was a snake in the grass: Marx, 'Review of *In Context*', and Kranzberg, 'Comment' (1992); also Roland, 'What Hath Kranzberg Wrought?', 699–700. (Marx had been singled out in 1969 for election to SHOT's elite Advisory Council, along with such superpowers as the chairman of the Committee on Science and Public Policy of the National Academy of Sciences and the director of research at General Electric, but the references to him in *Technology and Culture* became frequent only in the 1980s.)

[381] Marx, *The Machine in the Garden*, 199–203, 399, note 40, puts in evidence only two published statements unequivocally elevating technology and technologists over scientists—and poets. Both are from *Scientific American* in 1850. A third, likewise from *Scientific American* in 1850, Marx admits has 'a defensive tone'. This is so far from persuasive when one recalls that this journal, begun in 1845, initially weekly, carried many hundreds of items annually. The title of the journal itself makes clear that by that date a very different valuation of science relative to technology had already superseded the Jacksonian populist revolt: *Scientific American: The Advocate of Industry and Enterprise, and Journal of Mechanical and Other Improvements*. The identification of technical improvement as derivative from science implied by this title is confirmed by the editor's statements of his intentions with this 'scientific paper' conceived as successor to the *American Mechanic* and three other journals intended for working men. The advertisement at the top of the first page of the first issue, 28 August 1845 (Vol. 1, No. 1), specifies the coverage of the journal as '*New Inventions, Scientific Principles*, and *Curious Works*; and will contain, in addition to the most interesting news of passing events, general notices of the progress of Mechanical and other *Scientific Improvements*', while the editor's letter 'To the American Public' at the top of the second page describes its intended readership as 'the intelligent and liberal workingmen, and those who delight in the development of those beauties of Nature, which consist in the laws of Mechanics, Chemistry, and other branches of Natural Philosophy'. (The text of the advertisement is available at http://en.wikipedia.org/wiki/Scientific_American#History [accessed 6 December 2005].)

[382] For example, Marx, *The Machine in the Garden*, 198: 'In the period between 1830 and 1860 popular discussions of technological progress assume that inventors are uncovering the ultimate structural principles of the universe. In 1850 a writer inspired by a new telescope says: "How wonderful the process by which the human brain, in its casket of bone, can alone establish such remote and transcendental truths."' Kasson, *Civilizing the Machine: Technology and Republican Values in America, 1776–1900* (1977), to whom is due most of the credit for showing that Marx got it wrong with Emerson's and Hawthorne's view of technology, also ignores science after dismissing the matter with the off-hand observation that 'men made little or no distinction in this period between theoretical science and mechanical ingenuity' (8). On the other hand, Hugo A. Meier, although with one foot in the emerging American Studies tradition and the other in what would eventually become an equally anti-scientific history of American technology tradition, was perhaps simply too early to be anti-science. In his doctoral dissertation, 'The Technological Concept in American Social History, 1750–1860' (1950), 150–1, 334–56, Meier took for granted that technology was applied science. Likewise in Meier, 'Technology and Democracy, 1800–1860' (1957), where 'science' appears more than forty times in the text. That orientation and emphasis is maintained in Meier, 'American Technology and the Nineteenth-Century World' (1958). Molella and Reingold, 'Theorists and Ingenious Mechanics' (1973), 140, in their discussion of early 19th century popular attitudes toward science in America, made no reference to Marx, *The Machine in the Garden*, and thus also none to Marx's ignoration of science, but complained, so much the more unfairly, that 'When Hugo Meier writes about the relationship of technology and democracy in *antebellum* America, there is a complete absence of theoretical science'.

[383] Marx, *The Machine in the Garden*, 174ff, 181ff, 191. As Marx explained, Walker's essay was provoked by Carlyle, 'Signs of the Times' (1829), which deprecatory analysis of contemporary life and thought fatefully introduced the concept of mechanization as characteristic of modern civilization. Walker himself was then a recent Harvard graduate, acolyte of historian George Bancroft and jurist Joseph Story, and presumably their mouthpiece there in the *North American Review*, that organ of the northeast cultural elite. Higham, 'Divergent Unities' (1974), notes 34 and 36, credits Marx with making Walker's 'Defense of Mechanical Philosophy' a canonical text in American Studies and the history of American technology, but also with having 'contributed to the blurring of historical perspective'.

[384] Mitcham, *Thinking Through Technology*, 20, has it more nearly right: 'Walker's "Defense of Mechanical Philosophy" makes the characteristic argument that mechanical philosophy is the true means for emancipating the human mind in both thought and practice, and that through its correlate, technology, it makes democratically available … freedom', but 'consequence', not 'correlate', is what Walker had in mind.

[385] Marx, *Machine in the Garden*, 185. It has become typical for historians of technology in discussing their displayed quotations to ignore the references to science in those quotations. For example, Smith, 'Technological Determinism', 19, 27–8.

[386] Marx, *Machine in the Garden*, 209–15; quotation, 214–15.

[387] So, for instance, Marx, 'On Heidegger's Conception', 648; 'Technology: The Emergence', 969, 974, 977–8.

[388] Meikle, 'Classics Revisited', 155.

[389] *Ibid.*, 156.

[390] *Ibid.*, 149–50, 151, 157, 158.

[391] Indeed the erasure of science in Marx's representation of the technological enthusiasm of the ante-bellum era is reproduced in Meikle's statement (*ibid.*, 152) that one of the 'mutually supportive points of a millennial ideology of technology' isolated and schematized by Marx was that 'the intellectual progress of inventors and engineers indicated the highest point of human achievement to date'. Is Meikle himself too much within the turn-the-back-on-science historiography of technology—or in a like tradition in American studies—to be able to see in

Marx's ignoration of science a basis for the appeal of *The Machine in the Garden* to historians of technology, or has Meikle simply taken this bias too as understood by his readers.

[392] The work of Hounshell and that of S. W. Leslie offer genuine exceptions. Likewise, Carlson, *Innovation as a Social Process*, 4, 344–9. Definitely not an exception is König, 'Science-Based Industry or Industry-Based Science?' (1996). König noted (pp. 71–2) with apparent surprise in this paper in *Technology and Culture* that although 'It is commonly agreed that in the late 19th century new industries (chemical, electrical, optical, and refrigeration) developed with closer connections to science and to scientific institutions', and although 'It has become commonplace to call these "science-based industries" … . Staudenmaier (*Technology's Storytellers*) does not use the term, and the contributors to *Technology and Culture* rarely do. *Technology and Culture*'s comprehensive index … does not contain the term'. Nonetheless, König's intent was entirely consistent with that of the Staudenmaier policy of ignoration, for his contention was that at least for 'the electrical industry in Germany prior to World War I, the term appears to get the science–technology relationship wrong by almost precisely 180 degrees. I will argue that in the case of that industry, it would be more apt to refer to "industry-based science."' Of course König's argument could succeed only by a sleight of hand, namely that the 'science' which concerned him is not a science at all, but that 'engineering science' which is electrical engineering. And even then, to argue his thesis that electrical engineering originates in industry, not in the academy, König had to ignore the stated fact (p. 81) that in the crucial founding decade of the 1880s most of those appointed to chairs of electrical engineering were physicists entirely without prior experience in industry.

[393] Kline, 'Construing "Technology" as "Applied Science": Public Rhetoric of Scientists and Engineers in the United States, 1880–1945' (1995). It should be noted that there is no warrant for Kline's characterization of the rhetoric that he displayed and discussed as being specifically 'public', with its implication of some insincerity. Further, the rhetoric of *scientists* is not exhibited in this paper but merely imputed; the widely-employed rhetoric of pure science is labeled by Kline with the name of an organization, the National Research Council (NRC), in order to conjure up an institutional power to which Kline has the engineers bow.

[394] *Ibid*., 221. The quotation is from Kline's 'Conclusion', and what it asserts is there asserted not of 'engineers and industrial researchers' only, but of 'a large number of prominent physicists, chemists, engineers, and industrial researchers'. However, as said above, the views of chemists and physicists figure almost not at all in Kline's evidence, or in his several previous adductions of this status/image/politics explanation of the engineers' statements relating themselves to science: 'As shown by Edwin Layton, presidents of engineering societies typically called engineering an applied science in their annual addresses from 1895 to 1920 in order to maintain its professional status in an era of increasing corporate employment' (203); 'leaders of industrial research at General Electric advocated the NRC's pure-science ideal more strongly than [Bell Labs head] Jewett, in order to improve the lab's public image and to attract and retain first-rate scientists (211; no evidence cited); 'As vice president of the engineering section of the AAAS in 1938, he [William Wickenden] said that science was "immensely enriched" by a union with "technology." But he then bowed to the hierarchy of the association by saying that "technology, however, can scarcely exist without the fertilizing principle of science"' (215; no evidence cited); 'During the congressional debates about the establishment of what became the NSF, engineering educators attempted to instruct Congress on the virtues of "engineering science" in order to gain a place at the federal trough. But they did so in a way that deferred to an NRC ideal of pure science espoused by Compton, Langmuir, and others who testified before them, probably in an effort to present a united front with the scientists' (219; no evidence cited); 'Why did engineers and industrial researchers from the time of Thurston [1880s] to that of Bush construe their field as subordinate to "pure science"? The discourses examined here suggest that a major reason was status' (220–1; conclusion, perhaps implicitly acknowledging absence of evidence).

[395] Kline, 'Construing "Technology"', 203, cites pages 56–8, 66–7, of Layton, *Revolt of the Engineers: Social Responsibility and the American Engineering Profession* (1971), as source for his 'As shown by Edwin Layton' attribution quoted in the previous note. No support is, however, to be found there. Nor should there be. For, as Layton's subtitle makes clear, his argument in this book—notably on the pages lying between those Kline cites, most especially p. 62—was that the potential for improved social status lay not in appropriating the mantle of science but in the engineer's assumption of social responsibility. On the pages Kline cites, Layton was already striving to *de*emphasize the extent to which his engineers regarded themselves as applied scientists, while on the intervening pages Layton emphasized the sincerity of their discourse: 'inaccurate as literal descriptions, the engineers' self-portrayals expressed deeply felt aspirations for freedom and responsibility'. (61) Even five years later, Layton was not thinking along Kline's lines. Thus in 'American Ideologies of Science and Engineering' (1976), 690, Layton declared 'What is surprising, however, is to find American engineers endorsing what appears to be the scientific ideology', and for this he had neither Kline's nor any other explanation to offer. More: the issue of *Technology and Culture* carrying Layton's paper also contains a transcript of the discussion following its oral presentation (701–2). There one finds Hughes pushing Layton toward social status as motive for the late 19th century engineers attaching their practice to science, but Layton was resisting that interpretation—in part, at least, because it disserved his representation of those engineers as constituting an autonomous cognitive community.

[396] Although not ordinarily so cited, Calvert, *The Mechanical Engineer in America, 1830–1910* (1967), does provide some evidence in the form of an argument that the mechanical engineering elite in America abandoned in the late 1880s their opposition to collegiate training of engineers when it became clear that by insisting on a rigorous science-based curriculum those institutions would be contributing to the maintenance of an engineering elite. Better evidence has been brought for a 'social status' explanation of the *Verwissenschaftlichung* of engineering in Germany, where indeed one would expect it to have greater validity. Nevertheless, there too the tendency among historians of technology is simply to assume this to be so, to take it as self-evidently so: Manegold, 'Technology Academicised', 146–51; Osietzki, 'Die Gründungsgeschichte', 50; Dietz *et al.*, 'Der "Kulturwert der Technik"'; Braun, 'Technik as "Kulturhebel"', 39, 43. Gispen, *New Profession, Old Order*, is an extreme example, both in his insistance that academic engineering education provided only 'technologically useless erudition' (66), and in his constant reiteration of the nothing-but-status claim without offering argument or evidence. Such evidence as can be brought for the German case argues for the implausibility of that same interpretation in the American: there are scarcely any American parallels to the agitation and literature generated in Germany at the end of the 19th and beginning of the 20th century.

[397] Hughes, in 'Machines, Megamachines, and Systems' (1989), a very open statement of his fundamentally ideological commitments in writing history of technology, associated himself with Perry Miller's 'marvellous image of nineteenth-century Americans', namely that in flinging themselves '"into the technological torrent … they shouted with glee … as they went headlong down the chute"'. Hughes included the essay containing this image, Miller, 'The Responsibility of Mind in a Civilization of Machines' (1962), in his collection: *Changing Attitudes Toward American Technology* (1975). Multhauf, in charge of the history of technology and science at the Smithsonian and deeply immersed in the elucidation of early chemical technologies, was a member, along with Mumford and Hughes, of Kranzberg's 'Advisory Committee for Technology and Society' to create SHOT, and a contributor to the opening issue of *Technology and Culture*. Like Kranzberg and Hughes, Multhauf was an admirer of Mumford, and notwithstanding that he was greatly put off by Mumford's *Pentagon of Power*, in reviewing it in *Technology and Culture* Multhauf could not say that Mumford was wrong: 'Lewis Mumford remains one of the most knowledgeable authorities on the impact of technology on society. He is probably correct in describing the danger it poses', for which danger

Multhauf quoted Mumford's declaration: 'I have been driven, by the wholesale miscarriages of megatechnics, to deal with the collective obsessions and compulsions that have misdirected our energies, and undermined our capacity to live full and spiritually satisfying lives'. The entire spectrum of ambivalent attitudes coexisted in Kranzberg, who was always too busy playing upon one or another to notice the contradictions among them.

[398] Misa, 'Beyond Linear Models' (2004), 263, ridiculing the abjuration of every form of technological determinism. In this regard, Misa has steadily stood outside the SHOTist orthodoxy. Schatzberg remains wholly within it: 'Like the definition of technology as applied science, no prominent historian of technology today would admit to belief in technological determinism': Schatzberg, 'Undermining Common Sense' and '*Technik* Comes to America'. It is true that *Technology and Culture* published recently Ceruzzi's 'Moore's Law'. Ceruzzi argues there that 'Moore's law'—the empirical fact of a fixed exponential rate of increase, over the past forty years, of data storage capacity, per unit area of semiconductor 'chips', in digital computers—shows that 'raw technological determinism is at work', and thus is evidence of 'the reality of technological determinism' (590, 593). But, though that is his thesis, Ceruzzi opens his essay by reminding his readers of the faith of their fathers—'Mel Kranzberg and his colleagues organized the Society for the History of Technology in part to foster a view of technology running counter to the notion that technology is an impersonal force with its own internal logic and a trajectory that human beings must follow'—and avowing his own adherence to that faith: 'I agree with and support this approach to the history of technology'.

[399] Staudenmaier, 'Rationality versus Contingency', 269. Re 'official posture', see Laudan as quoted in note 346, above.

[400] In the mid and late 1980s, Hughes associated himself closely with the social construction of technology (SCOT) program—see note 359, above, and note 408, below—specifically as directed against all and any technological determinism: Hughes, *American Genesis*, 5, 469–70. Elsewhere at the same time, 'Machines, Megamachines', 116–17, Hughes was pointing to Ellul, as well as to Mumford, as one of his 'mentors'—Ellul, against whom Winner's *Autonomous Technology* (1977) was so largely directed, and Mumford, whose thesis in *The Myth of the Machine* was no less deterministic than Ellul's—and insisting, as did they, that the determinative forces at 'the depths of the technological society' are *not* 'politics and economics' but 'machines, megamachines, and systems'. Thus disasters such as Chernobyl are *not* the result of 'societal values'; rather, Hughes insisted, those values themselves 'are integral parts of these technological systems'. In the mid 1990s Hughes publicly rejected social construction and returned to the demi-determinism of his earliest work in the history of technology (Hughes, 'Technological Momentum').

[401] In his concluding remarks at the 1978 'Roanoke Conference—Critical Problems in the History of Technology', Sivin, 'The Roanoke Conference II', 629, was pleased, but surprised, that 'no one took issue with Otto Mayr's denial that technology was autonomous', and Sivin urged upon historians of technology the 'useful public service' of debunking the influential myth of autonomous technology. (Winner's *Autonomous Technology* had appeared the previous year; he and Sivin were colleagues at MIT.) There were some objections to this ideology as ideology in the 1980s: Rae, 'What Did We Expect', responding as one of the founders of SHOT to Staudenmaier, expressed concern that Staudenmaier's anti-progress 'theory of history' was interfering with evaluating work on the basis of scholarly merit. Hindle, 'Historians of Technology', 236–8, adduced MacKenzie, 'Marx and the Machine', and Winner, *Autonomous Technology,* as such ideological history, but Hindle refused to see the ambivalence toward technology among the founders of the discipline and put all the onus for 'pessimistic', 'dark-side history' upon the younger generation formed in the rebellions of the 1960s.

[402] Wise, 'Science and Technology', 244. Similarly, on p. 230, after listing the metaphors proposed to describe the relation between science and technology, and concluding that 'The key idea behind all the metaphors is autonomy', Wise continued: 'Science and technology are viewed as autonomous with regard to one another, though far from autonomous with regard

to economics, politics, and ideologies. But no new model for the way these two autonomous enterprises act on each other has yet emerged'. (Of course there is a distinction to be made between 'autonomous technology' and 'technological determinism'. As no historian of technology that I have read has sought to make that distinction, I will not either.)

[403] Responding to Daniels, 'The Big Questions in the History of American Technology' (1970), Layton, 'Comment: The Interaction of Technology and Society', took issue with Daniels's extreme anti-technological-determinism stance, recognizing that it was at variance with his autonomy of technology program.

[404] Daniels, 'The Big Questions', 2. (For Daniels this was not so much ideology as a methodological postulate.) Nye, *Technology Matters*, teaches that technological development is not autonomous, that social, economic, or political developments are not determined by technology, *and* that no technological innovation is socially determined. Also Nye, 'Shaping'. That is, an unavowedly postmodern*ist* anti-determinism. Scranton, although avowedly postmodern*ist*, is too seriously engaged with the nuts and bolts of technological change to adopt so wholly ideological an indeterminism. He has admitted 'local determination': 'whereas technical advances and the quest for innovation and efficiency are not universally regnant, there may well be sites, sectors, and periods in which a technology-oriented logic governs'. However, he drew the line at allowing 'any notion that shifts in technology govern the restructuring of social formations (families, schools, firms, governments) or of cultural practices'. Scranton, 'Determinism and Indeterminacy', S33, S42.

[405] Staudenmaier, 'Rationality versus Contingency', 263. Staudenmaier has been the longest, most consistent, and most explicit in this position, what has been with him an essentially anti-modern position (see 'What SHOT Hath Wrought', 716; 'Disciplined Imagination', xi). Without Staudenmaier's anti-modern baggage, Schatzberg, '*Technik*', 488, 512, treating the history of the meanings of the word technology as 'the struggle between deterministic and nondeterministic interpretations of *technology*', proceeds from the same premise as Staudenmaier, viz. only by wholly disconnecting technology from science is it possible to throttle the 'mystifying, deterministic discourse that portrays technological change as the inevitable fruit of scientific discovery'. See also Molella on Mumford at note 332, above. Scranton's ignoration of science, to which I drew attention in note 365, above, is pertinent in this connection as well. As Heilbroner, 'Technological Determinism', 78, aptly remarked in this connection, 'disapproval is one thing; disavowal another'.

[406] http://www.historyoftechnology.org/pubs/booklet.html (accessed 27 July 2006). The statement appears in the 'Series Introduction', signed by Pamela O. Long and Robert C. Post as editors, 'Historical Perspectives on Technology, Society, and Culture. A booklet series produced by the Society for the History of Technology in cooperation with the American Historical Association'.

[407] That, and almost all the rest of the ideological tenets explored here, are provided as essential information for the general reader in Dunlavy's article 'Technology' for *The Oxford Companion to United States History* (2001): 'As engineers strove to enhance their status, meanwhile, they embraced the term ["technology"] but defined it as "applied science," closely allied with "pure" or "basic" science. … Historians of technology, organized professionally in the 1950s, disputed the "applied science" definition … . Also rejecting technological determinism and autonomy, historians explored the role of social choice and human agency in technological change.'

[408] On average, at least one genuflexion to Bijker *et al.*, *The Social Construction* was to be seen in every issue of *Technology and Culture* in the 1990s. In this connection Hounshell, 'Hughesian History' (1995), 211–13, reports some revealing information which Bijker provided in April 1990 about the origins and motivations for that program 'which', as Hounshell there observed, 'has for the last seven to eight years become the leading fashion in the field': Bijker 'and many of his colleagues in Europe had become alarmed by the growing apathy and sense of helplessness among the public' because it 'had come to view technology as autonomous

and deterministic'. 'Bijker and his colleagues determined to counter such resignation. In social construction they found their method.' They 'literally taught social construction of technology in the streets'. 'Only later did Bijker develop his approach more formally and take it into the academic markets of the world.' Hounshell (209–15) has an extended discussion of how and why the issue of technological determinism figured so prominently in the then recent history of the history of technology discipline, in which he gave Alfred Chandler's work some importance: 'The problem of technological determinism drove much of the scholarship in the history of technology in the years immediately after *The Visible Hand* appeared' in 1977. Hounshell did not, apart from what is implicit in his account of the origins of SCOT, find as problematic, or calling for explanation, the fact that the discipline lined up so solidly *against* technological determinism. Nor did Laudan, 'Natural Alliance', S26–7, in explaining that, though possessing only weak evidence, 'many historians of technology have embraced social constructivism because it gives them a way of combating technological determinism—the idea that social change is driven by technological change, that there is a technological imperative'. Pinch, 'The Social Construction', 20, while insisting that 'the radical version of social constructivism … is opposed to any conception of technological determinism', conceded, in effect, that very few (if any) of the historical case studies allegedly confirming social construction of technology met the requirements of the radical version.

[409] Sinclair, 'The Road to Madison', S10. History and literature offer no parallel: Novick, *That Noble Dream*; Graff, *Professing Literature*.

[410] Sinclair, 'An Agenda for SHOT', 598, and again, Sinclair, 'The Road to Madison', S11: 'the friendly, open, and democratic nature of our society'. Likewise Layton ('Leonardo da Vinci Medal'), quoted in note 346, above.

[411] Staudenmaier, 'Recent Trends', 715; Staudenmaier, 'Disciplined Imagination' , x; Hounshell, 'On the Discipline', 855, 863; Sinclair, 'An Agenda for SHOT', 598. The SHOT presidential addresses by Hughes, 'Convergent Themes' (1981), Cowan, 'Technology Is to Science' (1996) and Roland, 'What Hath Kranzberg Wrought?' (1997), are indirect arguments against so narrow a conception and exclusive a preoccupation. Yet the very fact that it seemed necessary to make that point—and not politic to make it explicitly—is itself evidence of the ideological character of the body being addressed.

[412] What Scranton, 'Determinism and Indeterminacy', S35–6, pointed out forcefully, seemingly to no effect.

[413] Kline, 'Construing "Technology"', 194, quoting Mayr, 'The Science–Technology Relationship', 671. What Mayr saw as the most fundamental aspect of this question, namely the relative *rank* of technology and science 'on our scale of values', and how that has changed over time, was not brought forward by Kline.

[414] As 'notable exceptions' to this generalization that 'little attention has been paid to the history of this view and why it (and similar beliefs) has been so pervasive in American culture', Kline, 'Construing "Technology"', 195, note 5, cited Molella and Reingold, 'Theorists and Ingenious Mechanics', and Layton, 'American Ideologies'. As we have seen, neither is notably so, and neither even addresses the matter of pervasiveness.

[415] That would remain true even if Kline had in fact provided the explanations he claimed to (196): 'Explanations for why historical actors took specific rhetorical positions will draw on changing practices in science and technology and the complex process of creating social boundaries around academic science, industrial research, and engineering.' For what such explanations take as a given—the importance of the social boundaries that are to be maintained—itself requires explanation by reference to cultural values.

[416] Googling 'The cultural turn' on 9 August 2006 turns up 77,000 web pages. For indications, quantitative and nonquantitative, of the decline of 'the social' see Forman, 'From the Social to the Moral'.

[417] Subordination of the social to the cultural is the finding, though not the historiographic presupposition, of Forman, 'Weimar Culture'. The invocation there of social pressure to

explain ideological reorientation was, and I think remains, required by the great size and suddenness of the change in the values articulated by physicists and mathematicians—in some cases by one and the same physicist or mathematician over the course of but a few months. More generally, where change in cultural values is what requires to be explained, the historian must keep an open mind, but the ultimate explanans need not necessarily be 'the social'; it can very well be the socio-economic consequences of 'the cultural' reacting back upon 'the cultural'. Such, I take it, is Bell's argument in *The Cultural Contradictions*, and of Christopher Lasch in his several books on the same subject.

[418] As J. G. A. Pocock wrote in the early 1960s, in the floodtide of 'the social', 'The slogan that ideas ought to be studied in their social and political context is, it seems to me, in danger of becoming a shibboleth; too many of those who pronounce it assume, often unconsciously, that they already know what the relations between ideas and social reality are'. Pocock, *Politics, Language, and Time*, 105. (The essay there republished was originally published in 1965. My attention was directed to Pocock's ageless admonition by Martin Collins.)

[419] So, for example, Godin, 'Measuring Science', 79, poses the question how the so-very-1950s concept of basic (= fundamental) research (= science) 'centered on the [non-utilitarian] motivations of the researchers and the non-application of the research results', adopted early on in the OECD Frascatti manual for 'measuring' research funding, could have continued to the present day notwithstanding the numerous well-founded criticisms of it over the past three decades. Godin's plausible explanation is the universities' political interest in maintaining the concept of fundamental research, and the bureaucrats interest in maintaining a consistent basis for statistical time series. Likewise, Harwood, *Technology's Dilemma* and 'On the Genesis of Technoscience', uses institutional factors to account for departures from culturally sanctioned science-technology relations.

[420] Of course, the inverse does not hold: adhering to the conception of coherent historical eras does not of itself imply a disciplinary orientation, as is well exemplified by such eminent periodizers as Spengler, Mumford and Foucault. That Foucault's aim and method were essentially Spengler's and Mumford's is perfectly clear from the description he gave of it in his 'Foreword to the English Edition' of Foucault, *The Order of Things*, ix–xiii. How much and how highly Foucault had Spengler in mind is indicated by his bracketing Spengler with Hegel and Marx. *Ibid*., 334. While I believe that no one convinced of the fact of postmodernity can hold any but the slimmest hope for the survival of disciplinarity, the alternative to pursuing scholarship 'as if' we remain fully committed to disciplinary objectives and constraints is a chaos of purposes and practices in which only unlovely characters can thrive. If postmodernity, such as it is, continues its advance—and I can see nothing short of a catastrophic alteration of the life conditions on this planet as capable of altering the ever wider spread and deeper seating of this radically self-regarding individualism—then the consequent transformations of personality, culture and society will render the constructive endeavors of the past three centuries increasingly irrelevant and unintelligible. Among those endeavors, science is especially vulnerable. For if science is not regarded as separate and distinguishable from technology in some culturally highly valued ways, and if the fact of scientific laws is not regarded as a greater miracle than the fact that the machine works, then it is 'curtains' for the scientific enterprise. To be sure, the foregoing analysis does not warrant so categorical a conclusion, but only that there will be in the future far fewer sciences than we knew in modernity, viz. only such as, by their objects and procedures, cannot possibly be conflated with technology. The stronger conclusion follows, however, from other features of postmodernity, features cooperating to destroy all possibility of disciplinarity. Something of the anti-disciplinary effects of postmodern values is indicated in my brief papers, Forman, 'From the Social to the Moral', 'In the Era of the Earmark' and 'What the Past Tells Us'.

[421] Edgerton, 'British Scientific Intellectuals', 2.

[422] More recently, Edgerton, 'The Linear Model', in support of his denial that any thoughtful persons in those post-Second World War decades, other than self-justifying scientists,

believed in the linear model, cited Forman, 'Into Quantum Electronics' (1996) and, more especially, Forman, 'Behind Quantum Electronics' (1987), which papers explored how the greatly expanded governmental support of basic physical research in academic settings after the Second World War was conditioned upon and integrated with a far vaster program of achieving national security through superiority in military technology. The fact that only a small percentage (5%) of the funds expended by the military for research and development were set aside for basic research, and that small percentage was neither well rationalized nor used solely for researches properly regarded as basic, was adduced by Edgerton as evidence that the military agencies, their commanding officers and their civilian program officers, had no such faith in basic research as the source of new military technologies. Here, however, it is necessary to distinguish again, as I have repeatedly through this paper, between the ideological primacy of science, of which the linear model was an expression, and the actual practice of those convinced of that ideology. It was not at all the aim of my papers cited by Edgerton to argue against the ideological primacy of science, but rather to expose the contradictory reality that that ideology obscured from our view. At that level, the level of actual practice of technological innovation and of the application of available resources to further their real interests, Edgerton is of course more right than wrong in insisting that 'The Linear Model Did Not Exist', but at the level of belief, he is far more wrong than right. The fate of basic research in industry over the past two or three decades demonstrates that it is that large margin of difference at the level of belief that makes all the difference at the level of practice. In view of that recent history, it seems beyond doubt that what is important about the small fraction of total 'innovation' funds applied to basic research is not its smallness but its existence: absent that credence in the linear model, and, more generally, in the value of flying the banner of pure science, the level of support for basic research by profit-making organizations falls to zero.

[423] See Mirowski, as referenced in note 60, and Mirowski and Sent, *Science Bought and Sold*, for the coincidence in date with a broad shift in conceptions of the relative 'goodness' of the public and the private. Collins, 'One World', for a manifestation of it in global communications. For earlier efforts to define and date the modern-postmodern transition, see Harvey, *The Condition of Postmodernity*; Rose, *The Post-Modern*.

[424] To this circumstance Hollinger, 'The Defense of Democracy', drew our attention in the early 1980s. Yet, to my knowledge, attention has not been drawn to the particular unanimity of science and modernity in the primacy both assert of means over ends. A very clear statement of this axiom common to modern science and the modern polity is given by Jacob Bronowski, *Science and Human Values*, 71: 'the end for which we [scientists] work exists and is judged only by the means which we use to reach it. This is the human sum of the values of science. It is the basis of a society which scrupulously seeks knowledge to match and govern its power.' My attention was drawn to Bronowski and to this quotation through the closing paragraph of Pyenson, 'What Is the Good'.

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