THE TECHNOLOGICAL INFRASTRUCTURE OF SCIENCE: COMMENTS ON BAIRD, FITZPATRICK, KROES AND PITT

Michael Seltzer, Michigan State University

The concept of a technological infrastructure permeating and perhaps even determining human activity goes back at least as far as Ellul ("technique" as the ensemble of human means), and perhaps further to Mumford (the "megamachine" controlling societies), or even Marx (the infrastructural mode of production determining superstructural social relationships). The view that the human activity we call science is constrained in important ways by technological infrastructures, appropriately defined, is the unfolding contribution to technology studies of Joseph Pitt (e.g., 1993a, 1993b, 1995). The papers presented here suggest that the technological infrastructure of science is a fertile construct for research in philosophy of technology and related disciplines. In my comments, I (1) introduce the technological infrastructure concept; (2) lay out briefly some of the key problematics associated with technological infrastructure; (3) provide some analysis and extension of Pitt's view of technological infrastructure; and (4) show how the papers presented fit in to my development of the concept of the technological infrastructure of science.

THE TECHNOLOGICAL INFRASTRUCTURE OF SCIENCE

Joseph Pitt (1995) defines technological infrastructures as sets of "mutually supporting artifacts and structures which enable human activity [including scientific activity] and provide the means for its development" (p. 6). With this definition, Pitt means to include more than merely machines, as in the traditional notion of technology. For Pitt, a technological infrastructure of science is "that assembly of different forms of work relations among people which makes the doing of science possible." As he argues, this definition "automatically includes the people, artifacts, institutions and networks which constitute the environment within which work occurs." Clearly, with technological infrastructure, Pitt has in mind a broad and powerful research tool with which he aims to answer important philosophical (and historical) questions regarding the sciences and their changes over time.

Pitt's hybrid construct, a conglomeration of material and social cultures, we can compare to Ellul's ([1954] 1964, 1962) technological determinism and its embodiment in the concept of technique. For Ellul (1962), technique was "the new and specific milieu in which man is required to exist" (p. 10). And, as Merritt Roe Smith (1994) notes, technique "refers to a good deal more than machines. . . . It also encompasses organizational methods, managerial practices, and . . . a manner of thinking that is inherently mechanistic" (p. 30). However, Ellul's construct, insofar as it was "independent of all human intervention" (Ellul, 1962, p. 10), Pitt rejects as "reification." Indeed, Pitt is no technological determinist; for Pitt technology is "humanity at work." So, while Ellul's technique shares and perhaps anticipates some of the characteristics of Pitt's technological infrastructure—it involves material and social constructs—Pitt departs company on the issue of determinism. For Pitt, people create and apply technologies in various ways; he rejects the notion that "technology is threatening our way of life." Whether or not technologies can have agency, however, I leave temporarily unanswered. I return to this question later in my comments.

Another scholar who developed a concept similar to Pitt's technological infrastructure is the economic historian Robert Heilbroner. In his classic 1967 essay, "Do Machines Make History?," Heilbroner ([1967] 1994) noted that it is difficult to determine "the degree to which the technological infrastructure is responsible for some of the sociological features of society" (p. 61). Here we have explicit use of the terminology of technological infrastructure, yet it was Heilbroner's goal, as it was Ellul's, to argue for a form of economic technological determinism. Indeed, Heilbroner believed that as far as a society's process of production was concerned, "We can indeed state that the technology of a society imposes a determinate pattern of social relations on that society" (p. 59). Again, Pitt's technological infrastructure is incompatible with such a notion, even if Heilbroner will permit "the machine [to] reflect, as much as mold, the social relationships of work" (p. 61). Pitt will not allow for the reification of economic forces Heilbroner takes as primary, whether it be modes of production, or his more recent focus on the economic "force field of maximizing" possibilities and their function as a "mediating mechanism by which changes in technology are brought to bear on the organization of the social order" (Heilbroner, 1994, pp. 71-73). Pitt rejects all attempts to maintain a "general rule or universal explanation" of this sort, for change in science and presumably for historical change in general.

Landgon Winner is another scholar who has placed significance in a concept close to Pitt's technological infrastructure. For Winner (1986), a major issue for those studying technologies is "evaluating the material and social infrastructures specific technologies create for our life's activity. We should try to imagine and seek to build technical regimes compatible with freedom, social justice, and other key political ends" (p. 55). Winner's view that technologies are inherently value-laden—that is, political—underscores the social component of technological infrastructures. And this again raises the question of agency. While Winner attributes some degree of agency to technologies in affecting sociocultural change, it is not clear that Pitt will follow Winner on this issue, even though Pitt describes a technological infrastructure (of science) as a "set of working relationships without which [a] specific scientific development could not have happened." Pitt's remark suggests that technological infrastructures have the ability to make an historical difference, but his view of technology as "humanity at work" suggests that nonhuman entities are inert and value-neutral. In my comments that follow, I suggest a possible way to reconcile these views. For now, suffice it to say that Pitt will clearly reject Winner's view of technology on the grounds that it is too deterministic—it grants too much autonomy to technologies.

I could indicate many others who have developed constructs similar to Pitt's technological infrastructure, but important differences will remain that leave Pitt's construct fresh and innovative. Thomas Hughes's (1987) technological system construct, for example, includes "physical artifacts" as well as "organizations, such as manufacturing firms, utility companies, and investment banks, and [it] incorporate[s] components usually labeled scientific, such as books, articles, and university teaching and research programs" (p. 51). In addition, Hughes's (1994) view, according to which mature technological systems gain a degree of "momentum" or autonomy from the surrounding cultural environment, seems to echo Pitt's view of the technological infrastructure of science. For Pitt, "The more sophisticated and mature the science, the more embedded and indebted to its technological infrastructure." Here it seems we have a good match, if we overlook the point that Hughes does not consider scientific change in his work.

Nevertheless, it is not clear that Pitt and Hughes have in mind the same kind of mechanism for embeddedness. That is, what is it that accounts in Pitt's view, for the embeddedness of a science in its technological infrastructure? Similarly, what is it that accounts for the partial autonomy, or momentum, of a technological system from the surrounding culture? Do we invoke power or political mechanisms; economic forces; social forces; or epistemological criteria? Hughes (1994), on the one hand, is somewhat vague on what exactly momentum is and how it works, although he does say its "characteristics" include "acquired skill and knowledge, special-purpose machines and processes, enormous physical structures, and organizational bureaucracy" (p. 108). Pitt, on the other hand, requires that the epistemic context of the change in scientific knowledge be taken into account in evaluating the role of the technological infrastructure, and that the explanation generated for the knowledge change be historically relevant and coherent. It seems both Pitt and Hughes want a variety of types of mechanisms to operate potentially in any specific historical context that is to be evaluated. What we seem to need, however, are further criteria that specify how knowledges, machines, and organizations are to be molded into a story of technical change, and how to apply criteria of epistemological and historical relevance to the telling of an appropriate story of scientific change. In short, we need to know how technological infrastructure and technological system operate as historiographical tools.

KEY PROBLEMATICS

The key problematics I have raised explicitly with regard to Pitt's technological infrastructure concept are determinism and agency. With determinism, we have a number of questions; among them are: Can technologies develop a degree of autonomy, or momentum, from the surrounding culture at large? Can nonhuman entities (that is, technologies in the traditional sense) determine the kinds of sociocultural forces that will operate in a given historical context? Pitt is clear on the point of autonomy; we should reject it. However, on the issue of technologies affecting sociocultural change, Pitt suggests that nonhuman entities are inert, yet he desires the technological infrastructure, which has both material and social components, to indeed make a sociocultural and historical difference. One way to make sense of this paradox is to specify to what extent a technological infrastructure is an historical construct, and to what extent it is an historiographical construct. I return to this issue shortly.

Regarding agency, the questions are related to those of determinism: Who or what has agency? What exactly do we mean by agency? Do technologies affect society, or do people create and implement technologies, which might thereafter have agency? Can there be agency without determinism? These questions immediately raise another problematic, causality. In short, what exactly is causing what? For Pitt, a main question is: How do technologies and technological infrastructures cause changes in the sciences? Pitt is clearly against any naïve view of causality, and is unwilling to specify in advance the factors that are relevant in any historical context. To find these, he argues, we look to the technological infrastructure in which the particular scientific activity is embedded. But is a technological infrastructure an historical or historiographical construct? Is it out there, to be found in the evidence to which we look in order to construct historical narratives? Or is it something we, as historians (and philosophers of history, and of technology) impose on historical data in order to construct adequate historical interpretations? I suggest that it is both, and that there are (at least) two senses of technological infrastructure.

One final problematic involves the subject matter of the sciences: the natural world, or reality. In short, do scientific phenomena, or entities (such as electrons, genes, or black holes) have agency? If a technological infrastructure includes scientific instrumentation, experimental apparatus, and experimental configurations, do we include entities or phenomena as part of the technological infrastructure? In addition, do we ascribe agency to them? If “making an epistemic difference” is important in accounting for changes in science, then presumably such entities have agency in the sense that their behavior in a particular experimental context can cause a researcher to change (perhaps radically) a scientific explanation or theory. Are we then realists, perhaps Pitt's Sicilian Realists?

AN EXTENSION OF THE TECHNOLOGICAL INFRASTRUCTURE CONCEPT

Pitt’s (1993a, 1993b, 1995) concept of the technological infrastructure of science provides a promising theoretical foundation for the analysis of science, technology, and culture. In my work, I extend and refine Pitt’s concept of technological infrastructure in order to create a metahistorical tool that researchers in many fields, including Philosophy of Technology, Science and

Technology Studies (STS), History of Technology, Philosophy of Science, Cultural Studies (of Science and Technology), and History of Science, may utilize in their research. As such, my technological infrastructure construct is a hybrid creation, one that draws on the work of many scholars working in several different fields. In addition, it has theoretical requirements that draw on scholarship from different fields, and the researchers in these fields do not always agree on even the most foundational issues, even if they share professional goals. Hence, I see technological infrastructure to be an alternative to previous attempts at specifying a broad theoretical construct for evaluating science, an alternative that incorporates some of the best features of previous attempts, yet rejects those that are untenable. To this end, I develop technological infrastructure as an incorporation, extension and/or replacement of, for example, Hughes's (1969, 1983, 1987) "technological momentum"; Kuhn's (1970) "disciplinary matrix"; Latour's (1987) "network"; Galison's (1987) "short-, middle-, and long-term constraints"; Hacking's (1992) "coherence of thought, action, materials, marks"; Rheinberger's (1992a, 1992b, 1994) "experimental system"; Pickering's (1995) "mangle of practice"; and Burian's (1996) "interaction of mechanisms, of structures and functions, at a great many levels."

Pitt (1995) defines the technological infrastructure of science as "a set of mutually supporting artifacts and structures which enable human activity [including scientific activity] and provide the means for its development" (p. 6). I fleshed this out by viewing a technological infrastructure as a combination of material and social culture. By social culture, I mean not only social structures, institutional or personal power relations, and interests—but also things such as statistical methods, experimental techniques, and scientific theories. By material culture, I mean not only machines, but also the natural world, and this includes the materials and entities of experiments in science.

(Although I employ the term "technological" in specifying the construct "the technological infrastructure of science," I do not suggest that the traditional notion of "technology," i.e., machines, is what is central to this discussion. Pitt (1995) defines technology as "humanity at work" (p. 5), and I here employ the term technology in similar broad fashion. Hence, this discussion is not primarily about traditional notions of technology, nor should it concentrate on evaluating specific machines, as in the discipline of the History of Technology. It should be about re-conceptualizing our notions of technology, especially as they relate to science. Perhaps, as Marjorie Grene suggested to me at a seminar in 1996, I should use the term 'technique,' for it seems more to capture the notion of humanity at work. However, I prefer 'technology,' because it brings the focus of scientific change on traditional views of technology, and this I believe is a positive step. Rather than broaden "technique" to include the use of machines, I prefer to broaden "technology" to include many kinds of human activities, including statistical methods, policy specifications, experimental techniques, and even narrative strategies.)

For Pitt (1995), a rather strong thesis arises out of analyses of scientific and technological change, the technological infrastructure thesis: "The development of new information in a mature science is, by and large, a function of its technological infrastructure" (p. 2).. Pitt argues that "scientific discovery today almost always completely depends on the technological context without which modern science would be impossible" (pp. 2-3). Pitt believes that the technological infrastructure construct can provide us with an alternative to scientific realism on the one hand, and the extremes of social constructivism, on the other hand (pp. 3-5). And further, it can bring technology into discussions of scientific change in a way that has been neglected. However, a crucial issue for this discussion is what is normally termed "reality."

A current issue in analyses of science and technology is what to make of the natural world—reality (see Grene, 1985; Latour, 1993). Since Hempel ([1945] 1965, [1950-1951] 1965) and Kuhn (1962), many researchers have rejected positivism as an adequate account of scientific change, and the positivists were not realists. Post-Kuhnian scientific realism has fared no better; one sees few attempts to rehabilitate the realist arguments of the 1970s and 1980s (see Fine, 1984). And now many even in the sociology-dominated Science and Technology Studies community, long permeated by the paradigm of the Sociology of Scientific Knowledge (SSK), are declaring the death of social constructivism. What these positions have in common is the lack of a coherent position on the natural world and how it should be incorporated into analyses of science and technology. And when considering scientific experimentation, distinguishing what is "real" signal (or entity) from artifactual "noise" or impurity is a practice among scientists that must be incorporated into accounts of science and technology. I suggest that the technological infrastructure of science construct must allow researchers to do this. As Pitt (1995) put it, "In this age of increasingly theoretical science, the technology behind the science may be our only contact with reality, and even so it is at best a tenuous one" (p. 3).

(In a recent editorial in *Technoscience* [1996a] Steve Fuller attempts to argue that social constructivism remains the dominant position among the members of the Society for Social Studies of Science (4S), which considers itself to be the primary organization for the field of Science and Technology Studies, even though, at a recent meeting in Bielefeld, Germany, many were proclaiming the "intellectual bankruptcy of 'social constructivism.'" Fuller contends that he "could not help but notice that one of 4S's less admirable tendencies has returned through the backdoor. Many of the same people [at the Bielefeld meeting] were to be found speaking at the most prominent panels . . ., often saying the same sort of thing they usually say. It is not surprising, then, that over the past few weeks, several people who were present at the meeting (NOT Gross and Levitt!) have proclaimed the intellectual bankruptcy of 'social constructivism.' While I think this is an unfair characterization of STS generally, if one only attended the bigger sessions at the Bielefeld meetings, one could easily get that impression. Much of the truly innovative work was tucked away in the smaller sessions that often contained no more than a few postgrads and recent PhDs." Fuller seems to suggest that where STS work goes beyond social constructivism, it is innovative. I claim that there is currently a turf battle in 4S over basic methodology, and social constructivism is losing. Furthermore, it is prominent social constructivists' attempts to justify their positions to scientists and the general public that has, in part, motivated scientists such as Gross and Levitt (1994) to publish their ill-informed attacks against those who study how science works.)

In addition to incorporating into accounts of science and technology a coherent notion of the natural world, the technological infrastructure construct must also presuppose that historiographical concerns be taken into account when considering scientific and technological change, in general, and experimental developments, in particular. That is, researchers must grapple with the view that not only is research in the study of science and technology a fundamentally retrospective activity, but also all human activity is fundamentally retrospective, and it is subject to a variety of epistemological problems (see Rouse, 1990, 1996; Seltzer 1995). As Pitt (1995) argues:

> If we want an explanation for the development of science, we

> need to offer more than a recitation of the sequence of ideas produced by scientists. We need an account of how those ideas were developed and why they were abandoned and/or refined. We are thus dealing with an issue in historiography. An explanation of scientific progress and discovery requires appeal to some mechanism. . . . The mechanism which makes the discoveries of science possible and scientific change mandatory is the technological infrastructure within which science operates. In short you can no longer do philosophy of science, history of science or even sociology of science without the philosophy and history of technology (p. 10).

Hence, we must develop the concept of the technological infrastructure of science as primarily an historiographical tool with which we can locate particular technological infrastructures that have operated in the history of science.

BAIRD, FITZPATRICK, AND KROES

In "Encapsulating Knowledge: The Direct Reading Spectrometer," Davis Baird develops the notion of instrument epistemology, according to which "scientific instruments are themselves expressions of knowledge." He tells the story of how researchers at Dow Chemical in the 1940s needed a quick and accurate method for determining the calcium content of magnesium alloy. Traditional spectrochemical methods took too long, so one researcher tried using photo-multiplier tubes instead of photographic film, and after overcoming several problems, by late 1944 the Dow Chemical Direct Reader was in operation, analyzing 4,000 samples of magnesium per month. Baird's goal in telling this story is to argue that instruments, as well as theories, "express knowledge of the universe."

In the case of the Direct Reader, the instrument "expresses a knowledge of spectrochemical analysis that is an amalgam of theoretical understanding and skills in working materials—know-how." And, using Popperian evolutionary epistemology as his model, Baird offers a model for instrument development in which the "resulting product"—the instrument—"exhibits knowledge of its particular niche." By niche, Baird does not mean "truth," as in Popperian error-elimination. In the case of instruments, the "regulative ideal" of function serves in place of truth. Instruments have functions; they are "adaptations to their world" in that they permit the "substitution of one phenomenon for another radically different in nature, but which can fulfill the same function." In this sense, instruments "express" knowledge, and "instrumenticians" are "function-smiths"; they proceed by "developing, replacing, expanding and connecting new functionalities from given functionalities."

Baird's instrument epistemology suggests fruitful ways for developing the concept of technological infrastructure. Instruments, clearly, are key components of technological infrastructures, whether infrastructures of science, on which Pitt concentrates, or for largely engineering purposes, on which Baird here focuses. In any event, what we are after is an account of change in knowledge, and viewing instrument-changes as knowledge-changes (changes in material knowledge) seems instructive, for it helps make sense of material reality. Instruments, as Baird argues, are expressions of ideas (propositional knowledge) and, more importantly, material realities. And if, as Baird argues in a longer version of his paper, the main problems involved in the development of new instruments are epistemological problems involving material constraints, then Baird's instrument epistemology provides a way for accounting for changes in knowledge that invokes a coherent notion of reality without degenerating into positivism or radical social constructivism. An interesting task for the future would be to see how instrument epistemology could be applied to the context of the scientific experiment.

In "Teller's Technical Nemeses: The American Hydrogen Bomb and Its Development within a Technological Infrastructure," Anne Fitzpatrick tells the story of how computing technologies were essential to the pace of the development of the H-bomb in the United States in the 1940s and early 1950s. In order to calculate whether the early model of the H-bomb would ignite and then self-propagate in a fusion reaction, scientists such as Edward Teller realized that large computers would be necessary, but such machines were still under construction. Even when the early H-bomb calculations were run on ENIAC, the first large electronic computer, the basic questions of the feasibility of the weapon were not answered. According to Fitzpartick, this situation, coupled with mounting political pressures, prompted the H-bomb scientists to develop a new configuration for the weapon, one that was easier to calculate than the old one, and which was eventually successful. "Lack of adequate computing" is the main factor that Fitzpatrick invokes to explain why the United States did not test an H-bomb before November of 1952.

For Fitzpatrick, the lack of an adequate technological infrastructure—in this case, large electronic computers—helps to explain why developments in the science and engineering of H-bomb weapons development did not proceed at the rate the scientists involved wished. Clearly, we have here a fruitful example of how to deploy Pitt's technological infrastructure construct in order to answer a straightforward historical question. Moreover, Fitzpatrick succeeds in showing how this can be done without advocating determinism. Computing technologies did not determine in any singular sense the design of the weapon. But "computing was the bottleneck, in historical perspective, that draws attention to the technological infrastructure within which the atomic laboratory had to operate." Computing was a material constraint, to echo Baird, in the sense that the nuclear scientists had to wait for the machines to be built in order to construct their weapon in a timely fashion. On this view, the instrument in question is the H-bomb weapon (whose function is to kill and destroy), and the material constraint is lack of computing prowess. Indeed, as Fitzpatrick shows, the scientists adapted their instrument design to fit the niche provided by the material constraint on computing power. Again, the technoscientific development required an adequate technological infrastructure in order to be successful.

In "Technological Explanations: The Relation between Structure and Function of Technological Objects," Peter Kroes delves into the relationship between the structure and function of technological objects, or instruments created by humans. All technological instruments are at the same time physical objects and objects that have functions. Engineers routinely design instruments to perform particular functions. Such designs normally include a physical description of the instrument plus a technological explanation of the instrument. For Kroes, a technological explanation is "an explanation of the function of a technological object [instrument] in terms of the physical structure of that object." So, it seems that engineers are able to "bridge the gap between a structural and functional description of a technological object"; they are able to reduce, apparently, the function of an instrument to its physical description. This is a problem for Kroes, for if this reduction is deductive, then we are explaining the human activity of instrument use in terms of the physical makeup of the object. This is undesirable, both logically and historically, for any given instrument would have only one possible function, and history (and perhaps common sense) shows us that this relationship is false. Deducing the structural makeup of an instrument from its proposed function runs into similar problems. This would mean that given a function to be performed by an instrument, only one possible structure would be possible. Again, logic and history do not cooperate. So, Kroes asks, How do the engineers do it? That is, What is the relationship between the structure and function of a technological instrument?

Kroes's answer is that the relationship is clearly not deductive. Using the example of the development of the Newcomen steam engine, Kroes succeeds in showing that in any attempt to explain the function of an instrument in terms of its structure, the explanans will contain functional concepts. And if we try to explain structure in terms of function, we run into similar dead ends. The upshot of his analysis is that the relationship between structure and function is never deductive, and that engineers bridge the gap between structure and function in technological designs by transforming "causal relations into pragmatic maxims," where these maxims are "based on the causal relations." That is, given a causal relationship (running the steam engine will cause the up and down motion of the pump rods), one can "derive" (but not deductively) a "pragmatic rule of action" (the engine will pump water); yet the engine will pump only because of its physical structure, since the pragmatic rule of action is based on that causal relationship, which is in turn derived on the basis of the engine's design.

Clearly, Kroes has solved neatly the logical paradoxes involved in the relationship between the design and purpose of an instrument. Using basic logic, he shows why naïve views of causality and determinism will not work in technology studies. Indeed, we need a notion of technological infrastructure that will eschew such deterministic explanations, and will look instead to the complex relationships that operate among the constructs we call science, technology, society, and culture.

REFERENCES

Bijker, Wiebe E.; Hughes, Thomas P.; and Pinch, Trevor, eds. 1987. *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*.

Cambridge: MIT Press.
Burian, Richard M. 1996. "Underappreciated Pathways toward Molecular Genetics as Illustrated by Jean Brachet's Cytochemical Embryology." In S. Sarkar, ed., The Philosophy and History of Molecular Biology: New Perspectives. Dordrecht: Kluwer. Pp. 67-85.
Ellul, Jacques. [1954] 1964. *The Technological Society*. New York: Vintage.
_______. 1962. "The Technological Order." *Technology and Culture* 3: 394.
Fine, Arthur. 1984. "The Natural Ontological Attitude." In Jarrett Leplin, ed., *Scientific Realism*. Berkeley: University of California Press. Pp. 83-107.
Fuller, Steve. 1996a. "Editorial." *Technoscience: Newsletter of the Society for Social Studies of Science*, 9:3, 1.
Galison, Peter. 1987. *How Experiments End*. Chicago: University of Chicago Press.
Grene, Marjorie. 1985. "Perception, Interpretation, and the Sciences: Toward a New Philosophy of Science." In David J. Depew and Bruce H. Weber, eds., *Evolution at a Crossroads: The New Biology and the New Philosophy of Science*. Cambridge: MIT Press. Pp. 1-20.
Gross, Paul R. and Levitt, Norman. 1994. *Higher Superstition: The Academic Left and its Quarrels With Science*. Baltimore: Johns Hopkins University Press.
Hacking, Ian. 1992. "The Self-Vindication of the Laboratory Sciences." In Pickering (1992), pp. 29-64.
Heilbroner, Robert. [1967] 1994. "Do Machines Make History?" *Technology and Culture* 8: 333-345. Reprinted in Smith and Marx, pp. 54-65.
_______. 1994. "Technological Determinism Revisited." In Smith and Marx, pp. 67-78.
Hempel, Carl G. [1942] 1965. "The Function of General Laws in History." In Hempel (1965), chapter 9. Reprinted, with changes, from *The Journal of Philosophy* 39: 35-48.
_______. [1945] 1965. "Studies in the Logic of Confirmation." In Hempel (1965), chapter 1. Reprinted, with changes, from *Mind*, 54: 1-26, 97-121.
_______. [1950-1951] 1965. "Empiricist Criteria of Cognitive Significance: Problems and Changes." in Hempel (1965), chapter 4.
_______. 1965. *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: Free Press.
Hughes, Thomas Parke. 1969. "Technological Momentum in History: Hydrogenation in Germany 1898-1933." *Past and Present*, 44: 106-132.
_______. 1983. *Networks of Power: Electrification in Western Society, 1880-1930*. Baltimore: Johns Hopkins University Press.
_______. 1987. "The Evolution of Large Technological Systems." In Bijker, Hughes, and Pinch, pp. 51-82.
_______. 1994. "Technological Momentum." In Smith and Marx,), pp. 102-113.
Kuhn, Thomas S. [1962] 1970. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
Latour, Bruno. 1987. *Science in Action: How to Follow Scientists and Engineers Through Society*. Cambridge: Harvard University Press.
_______. [1991] 1993. *We Have Never Been Modern*. Cambridge: Harvard University Press.
Pickering, Andrew, ed. 1992. *Science as Practice and Culture*. Chicago: University of Chicago Press.
Pickering, Andrew. 1995. *The Mangle of Practice: Time, Agency, and Science*. Chicago:

University of Chicago Press.

Pitt, Joseph C. 1993a. "Philosophical Methodology, Technologies, and the Transformation of Knowledge." In Larry A. Hickman and Elizabeth F. Porter, eds., *Technology and Ecology: Proceedings of the Seventh International Conference of the Society for Philosophy and Technology*. Carbondale, IL: Society for Philosophy and Technology.

_______. 1993b. "The Technological Infrastructure of Science and Change in Knowledge." Unpublished paper presented at Oxford University, September 1993.

_______. 1995. "Discovery, Telescopes, and Progress." In Joseph C. Pitt, ed., *New Directions in the Philosophy of Technology*. Dordrecht: Kluwer. Pp. 1-16.

Rheinberger, Hans-Jörg. 1992a. "Experiment, Difference, and Writing: I. Tracing Protein Synthesis." *Studies in History and Philosophy of Science,* 23: 305-331.

_______. 1992b. "Experiment, Difference, and Writing: II. The Laboratory Production of Transfer RNA." *Studies in History and Philosophy of Science,* 23: 389-422.

_______. 1994. "Experimental Systems: Historiality, Narration, and Deconstruction." *Science in Context,* 7: 65-81.

Rouse, Joseph. 1990. "The Narrative Reconstruction of Science." *Inquiry,* 33: 179-196.

_______. 1996. *Engaging Science: How to Understand its Practices Philosophically*. Ithaca: Cornell University Press.

Seltzer, Michael W. 1995. "Controversies over Evolution and Creationism: Towards a Postmodern Historiography of Science." *Proceedings of the Tenth Technological Literacy Conference of the National Association for Science, Technology, and Society*. ERIC ED 388558.

Smith, Merritt Roe. 1994. “Technological Determinism in American Culture.” In Smith and Marx, pp. 2-35.

Smith, Merritt Roe, and Marx, Leo, eds. 1994. *Does Technology Drive History? The Dilemma of Technological Determinism*. Cambridge: MIT Press.

Winner, Langdon. 1986. *The Whale and the Reactor: A Search for Limits in an Age of High Technology*. Chicago: University of Chicago Press.