

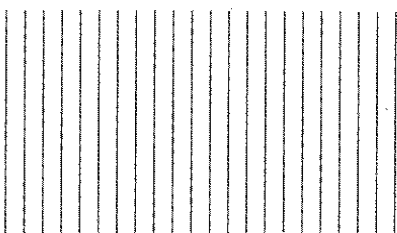
Science
Transformed?

**DEBATING CLAIMS OF
AN EPOCHAL BREAK**



EDITED BY

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Climbing the Hill

Seeing (and Not Seeing) Epochal Breaks from Multiple Vantage Points

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It seems obvious that some relatively recent technologies, such as personal computers and the Internet, have had a pointed effect on the conduct and values of science. So far, so good.

But a vantage point isn't necessarily a stopping point. If we've gone to the trouble of climbing this high, shouldn't we see if it's possible to climb further? The present epochal break is said to be a fracturing, over the past three decades, of the centuries-old partnership of science and Enlightenment. If, however, we examine that partnership not from a vantage point where we can view the past thirty years, but from a vantage point where we can see the past few centuries, what will we glimpse? In the next section I describe some of the historical landscape that a higher vantage point reveals—a landscape littered with epochal breaks, each bearing a family resemblance. The recurrence of such breaks is, of course, known to those who argue that the current one not only exists, but is more far-reaching and important than its predecessors. Nordmann, for instance, argues that the perpetual proclamation of epochal breaks is a defining feature of modernity, so it is no surprise that a higher vantage reveals more of them. He also argues that the desire to keep climbing to the vantage point at which the historical landscape becomes monotonous and the current epochal break appears trivial is itself a product of the current epochal break.

This is also the argument of Paul Forman (2007), another epochal break diagnostician, in his monumental lament for the death of science at the hands of technoscience: For Forman the fact that most historians of technology do not view the current break (if they concede that it exists) as singularly important indicates just how singularly important (and crippling, and pervasive) it is. For Forman, not seeing the enormity (both in the sense of size and monstrosity) of the epochal break amounts to complicity in the overthrow of the Enlightenment. What I want to do in this chapter is to take the opposite tack. I will argue that from some vantage points a *deflationary* view of the present epochal break is empirically coherent and not necessarily equivalent to a *complacent* view.

1

One reason Forman laments (and Nordmann urges caution) is that the current epochal break is allegedly shifting science away from its "commitment] to an unending quest for truth" (see Nordmann's chapter in this edited volume). On this view the pursuit of truth is perhaps not expelled from the new science, but it is now seen as secondary, or as inextricable from the production of socially useful (or profitable) applications. For Forman the era of science was characterized by an understanding that the means justified the ends—that the methods that ensured that science was uninterested in applications were what made the

ALFRED NORDMANN, IN THIS EDITED VOLUME, lays out several sophisticated and plausible arguments for seeing today's science as undergoing an epochal break. Unlike many epochal break believers, Nordmann recognizes the near-impossibility of convincing epochal break skeptics simply by inundating them with facts about *Diamond v. Chakrabarty* or the Bayh-Dole Act. Instead, he wants to show skeptics a path to a "vantage point" from which an epoch-making transition from science to technoscience is visible.

The vantage point from which an epochal break is presently visible is not where I usually locate myself, but it is not a great stretch to climb up there. Nordmann writes that one reason many historians and sociologists of science (such as myself) are epochal break skeptics is that they are stuck down in the valley of microstudies, where the details of science are in continuous flux, but general trends are invisible. Only by climbing up the hill, and viewing those microstudies from a distance, does an epochal break become visible. Taking a broader view of contemporary public discourse about science, it does look obvious that pronouncements of a new way of doing science have become rather commonplace. Some technoscientific activities, such as the patenting of academic research, clearly have become more routine than they were in 1970. And

knowledge it produced worthwhile. The new era, he says, is characterized by an understanding that the ends justify the means—that so long as socially useful technologies result, whatever technoscientists do is acceptable.

Now, announcements that scientists are no longer interested in knowledge as an end rather than a means are as old as science itself. And such complaints have often been tied to proclamations that science has entered a new epoch or that the generation of new knowledge has become foreshortened by (or embedded in) the application of that knowledge. Take, for instance, the postwar scientific disciplines in the United States, beginning with physics. From 1945 to 1951 the number of physics PhDs being churned out by American universities doubled every 1.7 years (Kaiser 2002). Graduation rates then leveled off for a while, but after *Sputnik* they shot up again, doubling every 6.2 years from 1958 to 1968. Federal funding for basic physics research went up by at least a factor of twenty from 1938 to 1953. Physics experienced this inflation first, but practically all scientific disciplines followed suit. As a result, the leading institutions of scientific research ballooned almost without bound. Stanford University, for instance, administered \$127,599 in government contracts in 1946; a decade later \$4.5 million; a decade after that, \$13.5 million (Leslie 1987).

If anything counts as an epochal break, surely this demographic and fiscal shock should. The spike in money and enrollments changed the methods of American physics by incentivizing use of fast calculation techniques, such as Feynman diagrams, in which students could be quickly trained, rather than discussion of fundamentals requiring prolonged student-teacher interaction. The money bubble also influenced the *content* of American physics. Such fields as solid-state and nuclear physics, in which students could be trained quickly and would have jobs waiting for them, grew rapidly. David Kaiser (2004) has argued that phenomena such as gravitation, which were conceptually thorny and yielded few applications, drew proportionally much less study than before World War II.

Many older physicists complained that the pedagogical style required by the rapid expansion of their discipline produced a new generation who had no individual creativity, who had “skills” but little “wisdom” or physical intuition, who were judged solely for their abilities as “team players” rather than as truth-seekers (Kaiser 2004). They also complained about the students’ values—that they were “clock-punchers” who saw research as a job rather than a calling, who merely wanted a comfortable life rather than the pleasure of seeking truth. Students, meanwhile, identified the same trends but saw them in an entirely different moral light. Many embraced the notion that physics was a track to

middle-class comfort and security. A few senior physicists who oversaw the Cold War’s trademark large research organizations—such as Luis Alvarez or Ernest Lawrence—were likewise happy that the new pedagogical regime could supply them with team players able to submerge their egos and live within a bureaucracy. These researcher-managers expressly saw Cold War security needs as requiring an experimental style (and a new pedagogy) that mixed up ends and means in exactly the way Forman sees as characteristic of *today’s* epochal break.

In particular, they saw nuclear weapons as experiments at least as much as they saw them as bombs. The great fiscal and demographic shock was, after all, predicated on a view that science should (and could) contribute to the nuclear state. Whole disciplines were reorganized to make this possible: geodesists studied the figure of the Earth to make missiles fly straight; seismologists found ways to verify test-ban treaties; biologists examined the effects of radiation; linguists and anthropologists devised ways to tell our descendants to stay away from nuclear waste. Technoscience in the Cold War mode was not limited to one or two precocious individuals, such as Alvarez or Lawrence; it was the framework that redefined practically all science and engineering disciplines in the United States—and in much of the rest of the world.

The paradigmatic Cold War technoscientists were the chemical engineers, metallurgists, physicists, mathematicians, and others who researched new nuclear “devices.” Those devices weren’t just envisioned as the eventual applications of postwar science—they were seen as the draw, and the means, to do science. As John Wheeler said of Princeton University’s physics graduate students in 1951, “It will be hard for them to do better than [to work] on the thermonuclear project for all-around range of ideas” (quoted in Galison and Bernstein 1989, 320). “Many physicists were entranced by the prospect of creating a phenomenon on earth—for the first time—that before then had only existed in the heavens: the chain fusion reaction” (Galison and Bernstein 1989, 276). Norris Bradbury, the first postwar director of Los Alamos, declared in 1945 that testing such a weapon would “provide some intellectual stimulus [and] might even be FUN” (Galison and Bernstein 1989, 277–78). The whole infrastructure of nuclear weapons development was designed to coproduce truth and bombs—from the by-products of plutonium breeder reactors that became an indispensable part of biological and ecological research to the giant nuclear craters in Nevada that provided data for geologists and climatologists.

It’s hard for me to see much difference, then, between the nuclear bomb and objects such as the OncoMouse that are continually cited as evidence of a new era of technoscience. What to make, for instance, of Project Plowshare, the

Atomic Energy Commission's attempt to use nuclear explosions to carve out artificial harbors and canals (Kirsch 2005)? Edward Teller and other proponents consistently described Plowshare as an "experiment"—not just in the sense of something untried, but in the sense of a means for generating knowledge in soil science, climatology, geology, and physics. Yet, not least because of secrecy concerns, there was virtually no attempt to generalize that knowledge to anything other than the experimental system (bomb + soil + air) itself.

Similarly, what about the physicists and mathematicians who developed game theory in the 1950s, partly through simulations of political crisis and nuclear warfare? No one at that point (or fortunately since) had ever fought an extended nuclear war, so these physicists and mathematicians were able to assert that their technical expertise trumped the combat experience of military officers (Chamari-Tabrizi 2000). The outcome of that expertise was the political war game—a protracted, multiperson, embodied, sometimes highly emotional method for better understanding group dynamics during crisis. Almost all the participants in these games felt they were learning something about group dynamics; yet there was precious little attempt to generalize that knowledge into a sociological theory of groups, or to divorce that knowledge from the national security infrastructure. Nor did most participants even believe that what they had learned was "true" in any objective sense. Rather, what they learned from the simulation was the excitement and emotion and chaos of crisis, a subjective truth that might or might not have any bearing on behavior in a "real" crisis.

II

So here we have mathematicians and physicists in the 1950s, eagerly developing a new research tool, yet not particularly worried that that tool can't, even in principle, generate objective, enduring truth. I can't see the difference between Cold War tools, such as nuclear earthmoving or political war games, and technological, postepochal break tools, such as the OncoMouse. Nordmann and Forman may mean that there is no difference, except for the gestalt switch of seeing a difference. Nothing can prove a difference between the OncoMouse and the atom bomb, but sitting in 2008 rather than 1958, we have access to a vantage point that lets us announce a difference between them.

But why should the current moment be *the* epochal break, the one that separates modern technoscience from the scientific enterprise that arose in conjunction with modernity and the Enlightenment? Why should this break be any more epochal than the one around 1945—which many people at the time viewed as epoch-making in terms of the conduct, content, values, and point of scientific research? Indeed, many saw the consequences of that epochal break

in terms that sound very similar to the way the current epochal break is often described. Whether they viewed the new epoch as a good thing or not, many Cold War scientists believed their peers were becoming more superficial, more interested in experiments that were simultaneously superficial applications, and less interested in knowledge for knowledge's sake.

Why should even 1945 be *the* epochal break, though? Certainly, something important happened then—something at least as epoch-making as any turn in 1980 or 2000—but at second glance we find all the same tropes floating around in 1910 or 1880. Take the case, for instance, of Irving Langmuir, the Nobel laureate chemist who spent most of his career in General Electric's research labs. When Langmuir arrived at GE in 1909, he was tasked with preventing the company's lightbulbs from blackening too quickly (a common problem at the time). Almost the only restriction on his work was that any patentable discoveries would belong to GE. But Langmuir was free to choose any method of arriving at those discoveries—even if that meant he drifted well away from the immediate, applied problem with lightbulbs.

Before the era of big corporate research, such a problem would have been approached largely through trial and error. Various parameters of lightbulb construction would have been tinkered with until blackening became less of a problem, and empirical observations of which variations worked best would have been folded into paths for further trial and error. Langmuir's insight, based in his training as a physical chemist, was to treat the lightbulb not solely as a product in need of fixing, but as an experimental apparatus capable of generating extreme conditions and thereby revealing new data on the nature of matter. In the end he used GE's lightbulbs to develop a theory of adsorption and desorption of ions that won him the Nobel Prize. But, as George Wise (1983, 19) puts it: "Langmuir's descriptions of these matters were not just outstanding science. They also taught GE how to build more efficient lightbulbs, and even helped put the company into the new business of electronics."

Of course, Langmuir's Nobel Prize was awarded not for his improvement of GE's lightbulbs, but for the generalized theory of matter derived from them. Indeed, Langmuir was ambitious to be taken seriously by his academic colleagues for something other than lightbulbs. Yet it is worth pointing out that in his more philosophical moments Langmuir was rather enamored of Percy Bridgman's operationalism—in which, presumably, attempts to take lightbulb knowledge past the lightbulb would be treated with caution. Admittedly, Langmuir rated his journal articles (where he advanced more generalized theories) as more personally satisfying than his patents, but he saw patenting as indispensable. Moreover, both he and GE's lawyers clearly saw journal articles not

simply as contributions to chemists' quest for truth, but as ways to build an intellectual property advantage for the company in disputes over technologies such as the thermionic valve. To return to Forman's formulation, it's not clear that this was in fact a period when the means justified the ends. To all appearances, it was a period when the means and the ends were mutually justifying each other and bootstrapping each other into existence.

Nordmann would respond, I think, that we can always find examples prior to any epochal break of people who exhibit the characteristics of the postbreak era, yet that the nature of the break lies in the *seizing* of a difference in the entire scientific enterprise before and after the break. Maybe so, but in Langmuir's time the scientific enterprise was undergoing wholesale changes that look strikingly similar to descriptions of today's epoch. Langmuir may have been exceptionally brilliant, but he was not an exception in his attitudes toward science "in the context of application." Indeed, as Philip Mirowski and Esther Miriam Sent (2007) have shown, the whole academic infrastructure that underwrites a conception of pure science conducted without view to application was co-constructed at the end of the nineteenth century with the infrastructure of corporate research. The high-tech companies of the day (Siemens, GE, Westinghouse, and so on) needed the academic world to train people like Langmuir who could move beyond trial and error. And to attract those people, companies were perfectly willing to pay for Nobel-winning research that could never be purified into basic and applied portions.

Indeed, in the early part of the twentieth century, scientists working in private, for-profit institutions easily outnumbered those working in universities. Corporate researchers routinely won Nobel Prizes throughout the past century, before and after the putative epochal break. In such disciplines as condensed matter physics, it was very difficult between, say, 1950 and 1990 to find any leading researcher in the United States (and to some extent in Europe and Japan) who had not spent formative years working for a for-profit lab. Corporate research wasn't all *applied* research, but it also wasn't paid for out of an altruistic search for truth. Long before the putative epochal break, the entanglement of corporate and academic research was generating much that looks "technoscientific"—for example, the laser. Epochal break diagnosticians therefore need to more compellingly explain just what distinguishes today's scientists from these forebears.

So should the epochal break be pushed back to 1910? Or can we climb a little higher up the hill to yet another vantage point from which that break looks no more epoch-making than the ones at 1945 and 1980? Well, if we keep climbing, we can certainly find people who had the same "technoscientific" attitude

as Langmuir in 1910 or Edward Teller in 1955 or Craig Venter in 2000. Take, for instance, William Thomson, Lord Kelvin. As Norton Wise (1988, 92) has shown, in Kelvin's dispute with the Maxwellians over electromagnetism, Kelvin's main argument was exactly that Maxwell's ideas were *not technoscientific enough*: "Thomson employed the practical reality of the telegraph as at once a moral and an epistemological weapon against what he regarded as the metaphysical idealism of Maxwellian theory." For Thomson the telegraph mediated "between two sets of beliefs about progress: industrial progress based on control of matter and scientific progress based on explanation of matter." Science might be a quest for truth, but it could only get there via practical technologies.

That is, for Thomson, "applied science" was redundant—the order and discipline needed to make science *useful* was a requirement for making science at all. "Neither theory, nor instruments, nor measurements, nor even standard units of measurement existed prior to the ocean telegraph venture" (Wise 1988, 94). Once the telegraph was in place, though, physical understandings based on experience making the telegraph a working technology would necessarily take priority over armchair theories. Moreover, Thomson believed any good theory could only be derived from embodied familiarity with commonplace, *useful* artifacts. For instance, "since experiment required light to consist of transverse waves, and since the only media known to transmit transverse waves were elastic solids, the ether ought to be regarded as like other everyday solids, Scotch shoemaker's wax, pitch, or calf's foot jelly" (Wise 1998, 97). Maxwell's ether, which behaved like no known substance (and certainly no commercial product), was unimaginable and therefore unintelligible.

As Norton Wise (1998, 97) put it: "The analogy thus carried a criterion of commercial success into electromagnetism as a criterion of a valid theory." Note that the criterion was not that an electromagnetic theory be applicable in a successful technology. Rather, a robust electromagnetic theory had to be rooted in the infrastructure and operation of a preexisting commercially successful technology. Kelvin was perhaps an even more hard-core technoscientist than today's academic entrepreneurs. Twenty-first-century biotechnologists only imply that their theories are right because they might someday lead to a profit. Kelvin believed that you had to have a profit already in hand before you had the right to espouse a scientific theory.

So, is it that our current epochal break merely takes us back to 1884, or was Thomson just 120 years ahead of his time? Certainly, he was more commercial than many of his colleagues. Yet Thomson was also the most influential member of the British scientific establishment of his day, a man who exerted enormous power in directing resources and creating institutions. His vision

of conducting research in large, skill-differentiated, multiproject academic lab groups created the template for British (and, more generally, Anglophone) universities right up to the present.

From these higher vantage points, then, we can see how common those moments are when historical actors saw things changing rapidly to accommodate something very much like “technoscience.” We can also see just how many continuities there are across those moments. There might be breaks, but they don’t seem very epochal, unless an epoch only lasts a generation or so. That, however, doesn’t seem to be the meaning of “epoch” that most believers in the epochal break use. Rather, they see the epoch that is putatively ending as having begun with the Scientific Revolution, and as having reached its mature form in the Enlightenment.

Yet even if we look back at the founding of the scientific enterprise, we see many of the same features that supposedly define the new epoch coming into being today. Take, for instance, Galileo’s workshop—surely a central site in the building of the scientific enterprise. Once again, we find the same technoscientific attitude to the means of generating knowledge as we have in other periods. As Mario Biagioli (1993) tells it, the reason Galileo’s discoveries could be taken as objectively true was *not* because they could be purified from the patronage that made them possible or from the applications both Galileo and his patrons expected from them. Rather, Galileo’s discoveries could be taken as objectively true precisely because he successfully obtained patronage from powerful people (the Medicis) who appreciated the political and technological applications of the ideas that he offered them.

Ironically, Galileo desperately needed Medici patronage just so that he could remove the appearance that his discoveries depended on patronage. Well, no contradiction with the epochal break picture of the scientific enterprise there—Galileo at least wanted to appear to be creating durable, objective knowledge. Except we have to ask—why was Medici patronage necessary to connect experimental activities (e.g., looking at the heavens through a telescope) to the quest for truth? Biagioli explains that Galileo’s training and disciplinary affiliations precluded his work from being seen as objective before Medici patronage. As a mathematician and astronomer, Galileo was seen by academic natural philosophers as thoroughly embroiled in the workaday world of surveying, navigation, and astrology.

All those were *commercial* activities, ways of making a living by selling expertise to a customer. Because it is difficult to maintain a lasting relationship with a customer if you don’t tell them what they want to hear, the academic natural philosophers believed that no mere mathematician could be trusted—the

applied nature of their work prevented it from being objectively true. Even in principle, Galileo’s description of the universe couldn’t be anything more than a heuristic in solving practical problems for pay. Thus Galileo convinced the Medicis to appoint him as the court *philosopher*, not mathematician—thereby leveraging patronage to lift his discoveries out of the commercial mud and into the realm of objectivity.

Of course, academic natural philosophers opposed that transformation. So, right at the beginning of the scientific enterprise, we see the same rhetoric that characterizes today’s talk (from both supporters and critics) about an epochal break in science. On the one hand, the academic natural philosophers (much like Forman) worried that commercially minded expertise was unjustifiably being raised to the status of objective knowledge. And on the other hand, scientific revolutionaries such as Galileo celebrated (much like Mike Roco or Craig Venter today) their breaking down of barriers raised by the traditional disciplines and their forging of a new philosophy that would work hand-in-hand with “industry” and the state. Galileo, after all, ran a workshop that manufactured telescopes, military compasses, and other instruments for the Venetian (later Medici) military and diplomatic corps.

III

Nordmann closes his chapter by saying that “the very diagnosis of *this* epochal break is therefore a reaction against it. . . . [and also] against those historians and philosophers of science who have unwittingly embraced a technoscientific attitude by seeing nothing unusual.” Forman comes to much the same conclusion—that ideology has blinded historians of technology to the primacy of science over technology before 1980. In Forman’s eyes, historians of technology have been so busy trying to perceive the primacy of technology over science in the pre-1980 period that they were unable to notice, much less be shocked by, the actual reversal of primacy after 1980. If you can’t see the epochal break, you can’t mourn it.

The problem with this view is that there are plenty of people who are able to diagnose the epochal break, but celebrate, rather than mourn, it. When, for instance, the founders of the U.S. National Nanotechnology Initiative labeled nanotechnology “the next Industrial Revolution,” they clearly had some kind of epochal break in mind, but only in the most positive sense. And when *The Economist* (“Innovation’s Golden Goose” 2002) praised the Bayh-Dole Act as the “most inspired piece of legislation to be enacted in America” in the second half of the twentieth century (neglecting, as Steve Shapin [2003] has pointed out, the Civil Rights Act of 1964 and the Voting Rights Act of 1965, among

others), they clearly meant it had opened a new epoch. After all, Bayh-Dole supposedly ushered in "a flowering of innovation unlike anything seen before." As David Edgerton (2007, ix) puts it in his critique of this way of talking: "We are told that change is taking place at an ever-accelerating pace, and that the new is increasingly powerful. The world, the gurus insist, is entering a new historical epoch as a result of technology. In the new economy, in new times, in our post-industrial and post-modern condition, knowledge of the present and past is supposedly ever less relevant."

The gurus' gushing doesn't indicate to me that diagnosis of the epochal break is sufficient to oppose it. In fact, it looks a lot like diagnosis is merely necessary to tout it. *Announcements* of epochal breaks have real consequences—they channel funding, they persuade people to join collaborations, they give bureaucracies a (wanted or unwanted) stimulus to reorganize. Such announcements puff up economic bubbles—remember the "new economy" of the 1990s, or Thomas Friedman's ubiquitously cited announcement that *The World Is (now, all of a sudden, epoch-makingly) Flat?* Climbing the hill to the point where we can view the consequences of announcing an epochal break is important. But continuing to climb to the point where the epochs look rather flat and continuous doesn't preclude skepticism about whatever epoch we are moving into. We need look no further than arch-modernist Bertolt Brecht to see that (as quoted by Edgerton 2007, viii):

I stood on a hill and I saw the Old approaching, but it came as the New.
It hobbled up on new crutches which no one had ever seen before and stank
of new smells of decay which no one had ever smelt before.

I'm willing to admire the view from the epochal break vantage point. But I've not been convinced to linger there. Indeed, once I've reached the point that I can see an epochal break, I'm all the more eager to keep moving. One option is to head back down the mountain, to the kinds of microstudies that historians and sociologists of science thrive on. If the epochal break has any features worth studying, they should be visible, in some way, down at the microlevel of practice. The other option is to keep heading up the mountain—to view the current epoch in the context of its peers and thereby to historicize, and perhaps deflate, some of its ambitions and justifications.

The epochal break has some analytical value, in that historical actors often speak of their own eras as utterly different from all others. We should take those actors seriously—they have knowledge that we can never fully gain. Yet their claims of epochal specialness are interested claims—they are made for a

purpose, to gather resources or convert foes into friends. As analysts, our interests differ from those of historical actors, as does the knowledge space that we have access to. We blind ourselves if we accept the claims for an epochal break without seeing the interests to which those claims are connected. We lame ourselves if we treat the putative present epochal break as so special or disruptive that we cannot learn from its resemblance to all of the other epoch-making turns, and all of the technoscience, that has come before.

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