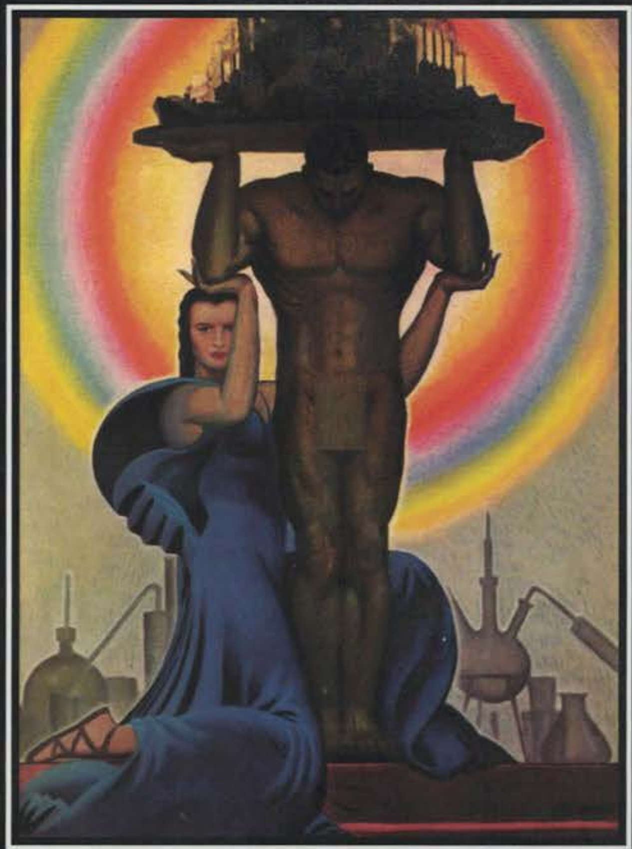
The Science–Industry Nexus

History, Policy, Implications



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Beyond Linear Models

Science, Technology, and Processes of Change

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The "linear model" of science–technology–industry was put forcefully in the motto of the 1933 Chicago World's Fair, "Science Finds—Industry Applies—Man Conforms."[1] In the subsequent two decades, the trend in social sciences toward universal, positivist, and functionalist approaches—manifest in such varied notions as William Ogburn's cultural lag theory, Walt Rostow's stages of growth, Talcott Parson's influential teaching at Harvard, and the modeling efforts of the Department of Defense and National Science Foundation in the United States—entrenched the idea that there was a knowable relationship between science, technology and social, economic and cultural change. Government efforts at managing large-scale bureaucratic agencies charged with research and development, as well as the first generation of technology assessment, grew up deeply influenced by this thinking. Over time the general concept of a linear model became something of an axiom for scientists and science-policy analysts, but it has fallen out of favor with historians of science and technology. The empirical record on balance just does not confirm the model's interrelated elements: (a) the empirical claim that scientific advances are the principal cause of technical change and economic growth; (b) the analytical claim that there is a one-way relationship between science, technology, and industry (and a determinist corollary about technology and social change); and (c) the normative claim that science ought to be relatively free from political meddling and oversight.

Nevertheless, the style of thinking underlying the linear model remains an influential article of faith for many entrepreneurs, technologists, scientists, and journalists. It is a pervasive justification for science policy; and in the United States it serves as a means for sidestepping the more politically contentious concept of "industrial policy." It also remains a commonplace in popular discourse. How can we diagnose this striking disconnect between academic discourse and the concerns and activities of practitioners, policy makers, and citizens? And how might we revision historical studies of science and technology to better engage such issues of policy debate and popular concern?

In examining these questions, this paper makes the following argument. I start by noting the persistence of the "linear model" and discussing mainstream economists' generally inadequate conceptualizations of technical change. This misconception of technical change stems in large measure from the dominant neoclassical framework of mainstream economics (despite the richer conceptualizations of evolutionary and institutional economics). The highly aggregated or "macro-level" view of science and technology held by most mainstream economists and many economic historians has persisted despite the historiographic turn toward specifically theorized "micro-level" studies of science and technology. A legion of such micro-level studies (e.g., most contextualist, constructivist, and actor-network inspired studies, irrespective of whether they are explicitly theorized as such) has convincingly shown the contingent, constructed, and contested character of science and technology, and has accordingly deflated naïve views of scientific rationality and technological determinism. But, with few exceptions, authors of these micro-level studies have shown little interest in or awareness of the results following episodes of controversies and stabilization, including the consequent patterns of social, cultural, and economic change.[2]

Presently, I believe, historians of science and technology need approaches that can link our fine-grained empirical research with broader theoretical frameworks and reflections on practical issues. Among these are the economic, structural and organizational analyses of science, technology and industry; in addition there are also the wider practical fields of public health, the environment, globalization, and modernity. In other words, to gain a voice in these on-going debates concerning the place of science and technology in the modern world, we need methods and approaches that, while maintaining our detailed knowledge about and insights into the construction of science and technology, also comprehend the technological and scientific shaping of society and culture. To do this, I consider an approach to historical processes situated conceptually between the micro and macro levels that centers on longer durations of study, broad questions about historical processes, a comparative or even transnational framework, and the desirability of international and collaborative work. This middle- or "meso-level" approach is, I argue, a powerful way to gain fine-grained insight into long-run processes that result in social, cultural, political, and economic change.

A holistic meso-level history can show that science and technology are at once social and cultural constructions as well as forces in historical processes. In slightly different conceptual language, I would say that we need ways of conceptualizing technology not only as a social product and as a social force but also as a social process. More succinctly still, we need to grapple with both the social shaping of technology as well as the technical shaping of society.[3] As instances of this approach I can point to two book projects—a collaborative volume on *Modernity and Technology* and my *Leonardo to the Internet*—as well as early results from an international collaborative research network on "Tensions of Europe: Technology and the Making of 20th Century Europe." Finally, I suggest that if our aim is fashioning a better, analytically robust conception of science, technology, and industry—or indeed, more broadly, the economy, the environment, globalization and modernity—we will need to create a deep institutional response. Programmatic essays (such as this one) will need to be matched with initiatives in funding, education, career paths, publication outlets, and public outreach and publicity. One thing we have learned about science and technology is that while it sometimes appears that "ideas make history," what really makes history is determined historical actors vigorously promoting attractive ideas.

MEASURES OF INNOVATION

A vivid example of the linear model's current influence can be found in *Technology Review* magazine's annual "Patent Scorecard." Since its relaunch as "MIT's Magazine of Innovation," *Technology Review* has trimmed its coverage of policy issues, technical controversies, and citizens' perspectives on technical decision-making to focus instead on the excitement of technology-driven innovation. On its website, *TR* sharpens its focus even further to "Emerging Technologies and their Impact." A recent special section of the magazine, published in May 2002, reports that despite the dot-com bust and the high-tech slowdown, patent activity is "booming" with year-on-year increases of 20 percent or more in the information and telecommunication sectors. A select "handful of patents [. . .] represent advances that could transform a number of industries—or even create new ones."[4]

Technology Review's "Patent Scorecard" might be dismissed as high-level journalistic hyperbole except for the specific structure and criteria used by *TR* to score and rank-order the world's top 150 innovating firms. Each firm's overall "technological strength" is composed of a multiple-component index, the company's total number of patents multiplied by its "current-impact" index; among the relevant categories that *TR* tabulates is the company's "science linkage." The science linkage is important because scientific papers as well as prior patents are "sometimes" cited as prior art in patent applications. The science linkage, then, is "the average number of science references listed in a company's U.S. patents." According to *TR*, "a high figure [for science linkage] indicates the company is closer to the cutting edge than its competitors."[5] Vannevar Bush, the MIT engineer widely hailed as the architect of postwar U.S. science policy, could hardly have put the point more forcefully.

Patent statistics and science-citation indexes are endlessly fascinating, of course. They both are presumed to be complete, comparative, and cumulative. And to the extent that *Technology Review*'s readership of engineers, corporate executives, patent lawyers, and venture capitalists really believes in an exercise correlating these aggregated measures, and makes investment decisions accordingly, it becomes "true." Still, there are many questions for anyone with a passing acquaintance with how patents are produced. (Different but equally skeptical questions come to mind when looking at the details of scientific authorship.)

Patenting, as you can sometimes see clearly in a company's archives, is an elaborate game played by inventors and patent lawyers, as they seek to construct a patent application that will cover as much intellectual territory as possible and support other patents held by the company, while not infringing patents the company does not possess. Many technology companies have recently adopted formal efforts to manage their "portfolio" of patents.

One's assumptions about the timeless objectivity, reliability, repeatability, and validity of science, invention, or patenting take a terrible beating, however, when you look in the archives and uncover the messy story behind the construction of a patent. Here I revisit the Bethlehem Steel company and follow in brief outline the ten-year saga of the famous Taylor-White patents for high-speed tool steel.[6] These two patents claimed legal protection for two decades of engineering research work by Frederick Taylor, the father of "scientific management," and his associates. Using research methods they deemed to be scientific—an early and paradigmatic instance of the engineering research method known as "parameter variation"[7]—they had developed a particular tool-steel alloy and set of associated heat-treatment processes that appeared to revolutionize machine shop practices and much of heavy industry across the world.

Henri Le Châtelier, the famous French physical chemist, was at first skeptical about the far-reaching claims for high-speed steel, but, as he related, "we had to accept the evidence of our eyes" when the cutting tools went into action. At the 1900 Paris Exposition, he wrote, "We saw enormous chips of steel cut off from a forging by a tool at such high speed that its nose was heated by the friction to a dull red color."[8] The new tools cut steel up to three times faster, and continued cutting even when the great heat of hard and heavy cutting made the tools glow red-hot. The desire of metal-working companies to get in on the action was the platform for Taylor's extensive consulting practice, which began when he helped companies install high-speed tool steels and reorganize their shop management and labor practices to deal with the vastly increased machine output the fast-cutting tools brought about. Taylor's tool steels, then, not only had direct economic benefits to metalworking companies (and unsettling shifts for the craft-oriented tool treaters who were discharged or deskilled with the adoption of his "scientific" heat treatments) but also helped Taylor launch the far-reaching "scientific management" movement. Here, it appears, is a technology that "changed the world."

Yet the closer you look, the murkier the invention of high-speed tool steel becomes. Daniel Nelson first drew attention to the fact that, contrary to Taylor's own boastful accounts, even though some 200 institutions were consulting clients of Taylor and his many disciples, only two factories adopted all aspects of the Taylor system, which included not only high-speed steel but a raft of planning, monitoring, and managing initiatives. Instead, most factories installed only selected elements of Taylor's shop reforms such as high-speed steel or stopwatch time studies. On closer inspection, the patents themselves lose much of their luster as a faithful recording of the technical work. The Bethlehem company's patent lawyers shaped the official account of the invention (choosing one certain day out of a month of promising experiments, October 31, 1898, as embodying the "Eureka" moment deemed necessary to convince the Patent Office) and the specific claims made in the patents. Company executives determined the patents' relations to corporate strategy. Bethlehem itself took, as one company executive phrased it, "the proper steps" to *delay* the patent's being issued (plans for an international tool-steel cartel were at play), and later brought a high-profile patent infringement suit against a domestic rival.

Bethlehem's strategy to monopolize the high-speed steel patents backfired spectacularly, however. The tool-steel cartel was stillborn, and the threatening rival constructed a compelling defense built around testimony gathered in England with the assistance of a "more or less secret tribunal," the Sheffield Steel Makers Ltd. Eventually, in 1909, the court nullified the two Taylor-While patents. Early on, Taylor had sold the U.S. rights to Bethlehem and casually blew off the company's entreaties to testify in support of the (company's) patents during the protracted court proceedings. But don't cry for him: he made a fortune from the half-share he retained on the patents' European rights, which generated a handsome stream of cash until their demise. (Here I should flag that I am employing a classic fine-grained micro-level account to question the timeless objectivity and rationality of the patenting process, a methodological choice I discuss below.)

The messy complexity surrounding an individual patent as well as the substantial uncertainty about what a patent really "is," of course, disappears once you turn to patent statistics and patent tabulations, such as *TR*'s. This aggregate view is helpful and perhaps even necessary if you are seeking broader patterns—and willing to make the assumption that uncertainties about individual data points will balance themselves out in large samples.[9] While there are several intriguing patterns in the *TR* patent tabulations, one that really stands out is that the largest firms dominate the key measure of "technological strength." IBM, with an overall "technological strength" of 6,321, towers over the field. The semiconductor firms Micron Technologies and Advanced Micro Devices, the computer firm NEC (ranked #2 in that sector behind IBM), and Lucent Technologies are the only other firms scoring above 2,000. It appears that competing against IBM and these technology heavyweights in the *TR* patent index requires a company not only to have patentable inventions but also to compete against well-funded legions of researchers and patent lawyers.

The schematized linear model of science–technology–industry is also consistent with the dominant mainstream neoclassical economics. While there are rival models of technology, innovation, and economic growth to be found in institutional, evolutionary and so-called neo-Schumpeterian economics,[10] policy makers (in the United States at least) are most heavily influenced by neoclassical models. Very broadly, these come in two types: those stressing that innovation results from so-called demand-pull factors and those that stress instead science-push factors. With the recent prominence of biotechnology, information technology, and nanotechnology, conceptualizations stressing science or more broadly "knowledge" and "information" are clearly in ascension.[11] Economists such as Edwin

Mansfield reinforce the science-push viewpoint by computing the "social return" from "basic science" in the United States to be an attractively high 28%. Routinely we hear in the United States that, as President George H.W. Bush put it to the American Association for the Advancement of Science in 1991, "Over a third of the economic growth that we've enjoyed since the 1930s, over a third of it, has been the result of new knowledge, including science and technology."[12] (Not to be outdone, Al Gore in the 2000 campaign upped this fully to half.)

Yet when you look carefully at the economic methods used to support these claims, you find that technology is calculated as a poorly theorized *residual*: it is "what is left over" when the other, better calculated contributions to GDP growth are subtracted out. Edward Denison beginning in the 1960s defined technology as a residual category in accounting for total national income growth.[13] The effects of increased labor and capital inputs, economies of scale, improved resource allocation, education, and so on are carefully modeled and quantified; the residual GDP growth is presumed to result from "advances in knowledge, etc." including technological change. Yet surely it's a strange model that when you find greater effects from (say) labor productivity, or any of the other quantified factors, you are forced to find less effect from technology. To stretch the point slightly, it seems the less competent your economists are, the greater your technology-induced growth will be. Similarly, the assumptions made in neoclassical economics about static "equilibrium" conditions and "production functions" also leave much to be desired when examined empirically.[14]

THE DEATH OF DETERMINISM

As historians, we might be tempted to dismiss these conceptual quandaries with the observation that "if only they understood what we know" the world would be a better place. And there have been a number of important studies shedding critical light on the linear science–technology–industry model.[15] But if we look at the dominant historiographic trends in the last decade or so, I believe that we find a compelling circumstance that helps diagnose why it is that academic-history discourses as compared with policy and public discourses are so divergent. We are *part* of the problem. To anticipate the argument of this section: the detailed micro-level methods that have been so powerful in attacking naïve views of scientific rationality and technological determinism have, at the same time, worked to create unhealthy distance between what "we" understand our enterprise to be and what the wider policy communities and intelligent public wishes to know about science and technology. To put the same point in personal terms, I am frequently enough asked by journalists some form of the question "How does technology change the world?" Yet I find that my field—the history of technology—has little to say in direct answer to this well-meaning question. Langdon Winner puts the point more bluntly: "the scholarly community in STS is so inward looking that it seems not to notice the glaring disconnect between its own favored theories and the visions of run-away technology that prevail in society at large."[16]

Detailed, empirical studies of science and technology are nothing new, of course. These stretch back at least to George Sarton's vision to create history of science as a rigorous scholarly enterprise. Historians' traditional concerns with archival virtuosity and subject specificity thus complemented the rise of micro-level *theories* of science and technology—i.e., the social constructivist and actor-network studies rooted in British and European science studies. Historians of science and technology learned a new conceptual vocabulary, while science-studies theorists gained a new audience and a persuasive source of empirical data. One can see this alliance forming through the many polemical pieces and review essays that reached flood tide in the 1980s.[17] While this is not the place for a full analysis of "the rise of constructivism" in science and technology studies, the alliance has had great influence across a wide swath of the academic world. Among the intellectual results has been a rigorous understanding of the contingent and constructed character of science and technology. All models and theories of science and/or technology ought to take these findings into account.[18]

Yet, however valuable they are, these constructivist findings constitute not a full but only a partial and incomplete view of science and technology. Along the way, in our zeal to demonstrate the social construction of science and technology, we as practitioners of micro-level studies have shown too little interest in or awareness of the broader *consequences* and *results* of science and technology. Often we have ended our empirical studies at the temporal moment at which processes of construction or closure have resulted in a "temporarily stabilized" fact or artifact; and we have used this result to argue that, at least in principle, the order and regularity that one can observe in the world and that one might relate to the deep structures of science and technology is, instead, either an evanescent social construction or an on-going achievement of social and cultural processes.[19] (The accompanying commitment by some constructivists not merely to a methodological or epistemological relativism, but rather to a full-blown ontological relativism made clear constructivism's wider "elective affinity" with postmodernism, with its *critique* of the certainty of knowledge.[20]) Where once science and technology were durable, hard, and permanent, the forces that changed society and culture, they are now, at least in the light of constructivism, quasi-stable constructions with no force of their own.

An equally serious shortcoming is that we have too rarely framed studies designed to show the technical or scientific construction of society and culture.[21] At least among historians of technology this has been the result of a pervasive wariness of being branded by our peers as a technological determinist. No greater crime could be imagined. Our studies have taken the stance of debunking or demystifying the longer-term or higher-level patterns of social, cultural, and economic change where technology is implicated. We have dismissed statements such as "the telephone changes the structure of the brain" or information technology is "the biggest technological juggernaut that ever rolled" or "globalization and IT crush time and space" as suspect specimens of linear-minded progress talk or some other anti-intellectual ideology.[22] Our response, so far, has too often been to deploy our skeptical and deconstructive armamentarium. I did just this above with the Taylor-White tool-steel patents. When faced with the question about a higher-level pattern about patent statistics, I showed the contingent and constructed character of a patent itself—suggesting, if not exactly proving, that higher-level questions about patent statistics are empirically ungrounded.

"FINDINGS FOLLOW FRAMINGS"

There are good reasons to be aware that the analytical "level" of our empirical studies strongly conditions the questions we will ask, the research we will do, and the results we will find, including what we conclude about the "nature" of science and technology and the relationship between science, technology and social change. One might say, to poach a line from architect Louis Sullivan, that *findings follow framings*. Time and again, studies conducted at the aggregated "macro" level lead to a certain view on the nature of science, technology, and change; whereas studies conducted at the fine-grained "micro" level tend, just as regularly, to an opposite view.[23] This reflexive perspective clarifies the running debate on technological determinism as well as sheds light on the even more fractious debates on scientific rationality, including the so-called culture wars.

Once we recognize the underlying importance of analytical levels, we can see that we need a way of "moving" between them, keeping in mind a reflective sensibility that recognizes that at each level we can conduct valid and robust research. We can label them, very roughly, as ranging from the most detailed micro level, through an intermediate or meso level, to an aggregated or macro level. Traditionally, we have understood phenomena at the micro level to be smaller or more concrete (e.g., individual historical actors or specific artifacts) whereas the macro level involves larger or more abstract entities (e.g., the state, modernity, the global economy). I have suggested that the poorly theorized "meso level" involves intermediate, mediating, or coordinating institutions or phenomena (e.g., standards-setting or rule-making bodies; consumer organizations, user groups, government advisory committees; agents of technology transfer and finance; various consortia of companies, engineers, and workers; and nearly all so-called networked entities). The so-called micro–macro problem also correlates strongly with the more general structure–agency problem in the social sciences.[24]

Addressing the micro–macro problem was a significant underlying issue for our recent *Modernity and Technology* volume. There, we tried to address the fundamental problem of connecting and relating the macro-level theorizing about modernity that is pervasive in the social theory tradition with the detailed micro-level research into processes of technical and social change prevalent in the technology-studies field. The volume took form with our frustrations (first explored in a Dutch seminar in 1997 and then refined during an international workshop in 1999) that extant theories of "modernity" said nearly nothing sensible about technology, while the active research tradition in technology studies mostly ignored the condition of modernity. We believe that technology and modernity should

both be problematized and their co-constructions jointly studied. We argued in the volume that any reasonable view of "modern technology" needed the insights of both fields (modernity studies and technology studies) and that a major barrier to developing these insights was the analytical gap between them. I believe our volume makes a promising first step toward developing concepts and methods for traversing these various disciplinary, conceptual, and analytical gaps.

Essays by Philip Brey and Paul Edward in the *Modernity and Technology* volume tackle the micro–macro problem head on. Brey's contribution is to specify the terms "micro" and "macro" more carefully, sketching in a more nuanced conceptual map of the different levels and the varied empirical phenomena that they direct our attention to. While some might wish to posit "divides" between the micro, meso, and macro levels, viewing them as conceptually distinct, Brey offers a way of conceptualizing phenomena and processes that link them. In his more empirical chapter, Edwards conducts an historical analysis of large-scale, pervasive society-constituting technical systems—or "infrastructures"—at multiple analytical levels, and shows a reflexive awareness of how one's results and findings are strongly shaped by the analytical level one is examining. That is, in considering such infrastructures as electricity, water, or information networks, Edwards demonstrates that they literally appear quite different depending on what level (micro, meso, or macro) that one is conducting the analysis.

Gaining analytical insight into the micro-, meso-, and macro-levels of analysis as well as devising narrative fluency in moving between them is, quite obviously, a task that we have only begun. Yet, for the reasons I have suggested above, it is a pressing task for our field to consider. At the very least, a multi-level method is a powerful way of relating historians' detailed micro-level research and findings to the larger questions of historical processes, structural changes, and the post-hoc modeling that is a pervasive feature of policy advice. Absent a way to move among these levels, it is difficult to conceive of our devising a satisfactory alternative to the "linear model," let alone to have our conceptualizations brought into policy-relevant discourses.

My thinking about broader patterns in historical change was initially framed by a concern with technological determinism and my own enthusiasm for constructivist ideas. In various ways, from David Noble, Roe Smith, Tom Hughes, and Wiebe Bijker, I learned how to demolish arguments positing technological determinism. I found, moreover, that there were regular patterns in how different subfields of history approached the question of whether (and how much) technology had agency in making history. Those historians who adopted macro-level methods were the ones to affirm some version of technological determinism, while historians adopting micro-level methods denied determinism and embraced complexity. (Philosophers of technology with a preference for high-level abstraction have been, until very recently, the most bold and unbounded technological determinists.) Yet work with several theoretically minded projects kept me exposed to colleagues committed to the importance of studying aggregated macro-level processes. And, of course, among historians there were the writings of the incomparable William McNeill on behalf of what he termed macro-history. "The central notion for all varieties of macro-history," he wrote, "is that of a social process (or processes) acting largely in independence of human awareness and so, by definition, not to be found recorded and awaiting discovery in some primary archive."[25]

Inspired by McNeill's insistence that historians could write intelligently about larger-scale processes and patterns, I began writing my own large-scale work, *Leonardo to the Internet*. This is a survey of technology and cultural change from the Renaissance to the present. The book, while grounded in a series of extended case studies—of individual figures, movements, companies, and agencies as well as their interactions with specific technologies—also deals with larger-scale processes and patterns, without (I hope) lapsing into a meta-narrative where individual actors fulfill a historical destiny by devising rationalistic solutions to complex problems.[26] For each of the book's eight core chapters, I adopt a long-term unit of analysis, and then show how that era's distinct cultural preferences, social and institutional arrangements, and economic structures shaped the development of specific technologies. In turn, I try to show how these technologies variously reinforced, interacted with, and at times undermined these preferences, arrangements, and structures. I take the main challenge for such a volume to be in devising an intelligent set of "eras," and then presenting a reasonable selection of the vast empirical material available to justify their coherence.[27] There are many risks in such an effort, of course; I hope that my readers will find fresh insights into technology from the book's long duration, broad questions about historical processes, and comparative framework. For the remainder of this paper, I would like to consider another longer duration and multidimensional effort, the Tensions of Europe network. Specifically, I would like to examine the *historical processes* that form the conceptual core of this project.

"Tensions of Europe" is an international network consisting of approximately 150 European and American scholars.[28] We aim to develop detailed expertise—across many countries and from varied research perspectives—to address a "big" question about the roles of technology in the making of 20th-century Europe. Rather than review the project's ten working groups, let me provide my personal view as a participant in the project's "cities" working group and as an American historian joining a European history project. (The project's website gives an overview, but I fear without quite "capturing" the project's wider historiographic aspirations.[29]) I will conclude, then, with some speculations about the research findings that such a project might generate to shed light on the "linear model" of science–technology–industry.

The project appealed to me first and foremost since its scope and ambitions—examining the whole of 20th-century Europe—seemed an excellent test bed for working out ideas and methods for large-scale history. Dealing with the "whole" of Europe, of course, entails numerous vexing problems with languages, research methods, sources, and the like. To deal with this set of problems, working groups organized around a research interest (e.g., colonialism, consumption, mobility, etc.) ideally involving historians from all the main regions of Europe. In the cities group, for instance, we have jointly compiled a 750-item bibliography of works in urban-technological history in the principal European languages. Work by our colleagues revealed to me the riches of Scandinavian and Spanish urban history, fields that were simply not on my intellectual map beforehand. Our group also hopes to write a popular, publicly accessible history of 20th-century European cities.

Across the wider project, we have the ambition of initiating historical research that goes beyond national comparisons and adopts a "transnational" approach. So, instead of looking at a one-to-one comparison of (say) German and French electrical systems or telecommunications, we spotlight the various mechanisms and processes—technological, institutional, political, cultural—that span the European countries (and indeed link Europe to its colonies and former colonies and to North America). These involve such varied phenomena as large border-spanning technological systems, international engineering conferences, standards setting processes, political initiatives such as the Marshall Plan, and the bevy of explicit Europe-wide institutions that have sprouted up since the 1950s.[30] Sometimes these technology-laden phenomena have worked to unify countries, or regions straddling two or more countries; other times, most obviously during the Cold War decades, they have worked to divide regions or countries. A simple example involves recalling how the railroad system shaped the internal structure and external frontiers of Europe—where Spain and the Soviet Union defined themselves "out" of Europe through their adopting non-standard railroad gauges for economic and military reasons, respectively.[31]

In the cities theme, we are to a certain extent interested in the manifest similarities (and differences) that one can find in the physical layouts of European and North American cities. We are even more interested in the *processes*, especially the flows of people and ideas that have conditioned these similarities and differences. Thus, we are interested in the series of international engineering and urban planning conferences in the early 20th century that helped define and disseminate varied ideas about what 20th-century cities were to be like; the determined campaign by architects and visionaries of the so-called International Style to create, develop, and impose a certain technological framing on "modern" architecture in Europe, the United States, the Soviet Union, and around the world; and the varied efforts by city engineers and planners to create the "car friendly city" after the Second World War and then in more recent years to promote car-free walking zones in city centers. City planning, transport, and the environment come together in the work of Nil Disco, who is examining the Rhine River as an object that spans borders and links cities. Ideally, we'd also have a parallel investigation of the Danube system and the links and non-links between East and West.

We also set the construction of "Europe" into the context of world history. This means taking seriously the many exchanges with the European colonies, on the one hand, and with the United States, on the other. Again, ideally, we might also have full-scale inquiries into Europe's relations with Asia, Australia, and

South America, whether or not these included formal colonies. (In the cities group we have a specialist on southeast Asian cities that is a step in this direction.) The colonies were not merely a source of raw materials and captive markets. The colonies also served as laboratories for large-scale experiments in urban planning, public health, and labor relations—which in turn shaped parallel developments "back home" in Europe. Through industrial and technological developments, French, Dutch, and British colonists developed a sense of identity as *European*, by which they meant white, civilized, and superior. Even after decolonization, regions in Africa and Asia continued to provide Europe with raw materials for industrial processes and places to experiment with extraction and agricultural techniques. Some of the most high-profile technological products of late-20th-century Europe—nuclear power plants and space rockets—depended on former colonies to provide uranium and launching pads.[32]

The continual juxtaposition of American and European visions, models, artifacts, systems, and experiments leads naturally enough to an enquiry into how America as well as the idea of "America" has influenced European developments as well as Europeans' sense of identities. From the 1920s, Europeans vigorously debated their cultural identity in relation to such classic American technologies and techniques as skyscrapers, automobiles, and scientific management. Many different—and changing—concepts of "Europe" emerged from these interactions. Ideas about what Europe was and should be were grounded in foundations as diverse as resistance to American industrial imperialism (which, for example, pervaded institutions such as Euratom and the European Space Agency, two of the earlier and more successful Europe-wide bodies) to optimism about the cultural exchange made possible by television (manifested in popular Europe-wide televised competitions such as the "Eurovision" song contest or "Games without Frontiers"). Today, popular brands and contested symbols circulate on both sides of the Atlantic—McDonald's, Disney, Benetton, and Ikea. Then, there are the transatlantic hybrids that have unsettled *American* identities, including Daimler-Chrysler, BP–Amoco, Bertelsmann–Random House, and others. Europeans' varied responses to the technological dimensions of "Americanism" helped define a "European" identity distinct from the United States and yet intimately related to the United States.[33]

This project and these considerations have altered my on-going research project on skyscrapers. Initially, I thought about American skyscrapers during the period from the 1880s to the 1930s as a neatly bounded topic that could be approached solely from an American angle; this made archival research, I thought, more manageable. My touchstone was one prominent builder's proclamation that "the skyscraper is the most distinctively American thing in the world." But while participating in these "transnational" discussions, I began to think of American skyscrapers through European eyes. There are a wealth of commentaries on skyscrapers—from several countries—that shed light on what Europeans found both fascinating and repelling about this characteristic American creation.[34] Published work in this area, mostly by architectural historians who have been more con-

cerned with matters of external "style" than the internal functions of tall office buildings, let alone the sort of broad comparative research outlined here, makes an essential foundation for my inquiry but hardly exhausts the topic.[35]

Then, in something of a shock to the commonplace knowledge that Europeans did not build skyscrapers until the 1950s, I've begun to "find" pre-World War II skyscrapers in Europe. Since these buildings did not fit into the established canons of modernist architecture, they are not well known in the United States even though they are near and dear to their local inhabitants, newspaper editors, and librarians (and surprisingly well documented in local libraries or archives). Some of these, such as Antwerp's Boerentoren (1929–32), would certainly qualify in height and construction style as authentic American skyscrapers. Indeed, this striking 25-story building was constructed with the classic "skeleton" construction—with interlocking steel columns and girders, load-bearing frame, and curtain walls—that might have come straight from Chicago. Another less lofty building, the handsome 12-story tower that anchors the Berlage Plan in Amsterdam, is revered locally as the city's first "wolkenkrabber." The 17-story administrative headquarters of Tomás Bata's enterprises (Zlín, Czechoslovakia, 1937–38), has also been described as a modernist, American-style building.

Europeans, then, were fully capable of building tall office buildings. Why relatively few European skyscrapers were built until the 1950s, especially given European cities' high land values, commercial densities, and available elevator industry—all classic "explanations" for U.S. skyscraper building—remains a research question. Differing building codes is the stock answer, but I hope to address the underlying question of *why* American and European building codes differed. One hypothesis is that the association of skyscrapers with "America" made them unacceptable, that is, they did not fit into the technocultural narrative of what constitutes a typical European city. I am presently conducting research in Europe with complementary material in the United States on the actual use and maintenance of American skyscrapers to help clarify European responses to these buildings as they were experienced by users (as contrasted with European responses to the highly publicized plans of architects).

MODES OF HISTORIANS' KNOWLEDGE

I hesitate to make an argument that research into the technical and cultural constructions of an "American" skyscraper in Antwerp will dispel the many uncertainties surrounding the "linear model," but I would like to conclude this paper by considering the *modes of knowledge* that a long-term and wide-ranging project such as Tensions of Europe might offer on this problem. First and foremost, the transnational scope of our research at least accords with what we know about science and technology, namely, that they do not readily respect national boundaries. The notion that nation-states are the most meaningful unit about which to collect statistics was probably only defensible so long as economic actors were largely bounded by the nation-state (perhaps from the late 1920s to the 1960s). But this focus on national units of analysis is highly problematic for historical studies either before the economic nationalism of the 1920s or since the 1970s as the global economy has gathered force.

To a significant extent, Philips, Ericsson, IBM, Monsanto or any multinational economic actor can quite legally move profits or losses as well as operations and staff between different countries depending on the company's strategic goals; and these involve transnational perspectives on taxes, employment, subsidies, markets, and technology. The "project of Europe," itself backed strongly by these transnational economic actors, has brought about a measure of harmonization of regulations as well as a single currency and centrally collected transnational statistics.[36] The influence of the U.S., with its stronger tradition of shareholder activism as well as corporate transparency, has also shaped the institutional evolution of multinational economic actors. Studying multinational actors—scientists, engineers, planners, bankers, workers, and consumer groups, in addition to corporations—should give a more realistic portrait of how science and technology relate to social, cultural and economic processes.[37]

Second, if my intuition is correct that longer spans of time are needed to comprehend the results and consequences of scientific and technological changes, we will need different ways of framing, organizing, and conducting our historical studies. To put down the obvious point, it is easiest for a graduate student working alone to master a specific well-bounded body of archival materials—and to adopt micro-level methods and approaches in analyzing these materials. It is much more difficult to coordinate traditional historical research with that of a graduate-school mentor (and in the United States there is a distinct bias against students doing so), let alone to collaborate with a group of far-flung scholars in different universities and/or even different countries. In the United States, we have little experience of large groups of historian-scholars collaborating together outside of editorial projects like the Einstein or Edison papers.

Nevertheless, if it is the case that traditional, individually conducted research methods with their focus on archival virtuosity and subject specificity strongly bias our field toward micro-level analysis (with its corresponding insights and blindnesses), I'd like to think hard about different, broader research methods and about ways of overcoming the institutional and financial barriers for conducting these new collaborative forms of research. Addressing these concerns seems necessary if we wish to connect our work as historians of science and technology to the broader debates about policy and practical concerns. Consider just one obvious topic understudied by historians, the role of science and technology in the global economy; obviously, since both the promoters of and protesters against globalization are themselves organized globally, the phenomenon needs to be studied at a transnational level. At the very least, our field needs methods—robust, repeatable, and teachable—for generating knowledge at "broader" levels and across "larger" units of analysis. I believe it would be a healthy development for historians of science and technology to more vigorously engage the on-going debates about science and technology and more actively relate our work to public and policy concerns. "We cannot afford," as historian William McNeill put the problem, "to make the world in which our fellow citizens live historically unintelligible."[38]

ENDNOTES

1. Cheryl R. Ganz, "Science Advancing Mankind," *Technology and Culture* 41 (2000): 783–787.
2. There have been efforts to build bridges between economic, organizational, and sociological analysis of technology: see Stephen R. Barley, "The Alignment of Technology and Structure through Roles and Networks," *Administrative Science Quarterly*, 35 (1990): 61–103; Wanda J. Orlikowski, "The Duality of Technology: Rethinking the Concept of Technology in Organizations," *Organization Science*, 3 (1992): 398–427; Rod Coombs, Paolo Saviotti, and Vivien Walsh, eds., *Technological Change and Company Strategy: Economic and Sociological Perspectives* (London: Academic Press, 1992); Robert J. Thomas, *What Machines Can't Do: Politics and Technology in the Industrial Enterprise* (Berkeley: University of California Press, 1994); David A. Hounshell, "Hughesian History of Technology and Chandlerian Business History: Parallels, Departures, and Critics," *History and Technology*, 12 (1995): 205–224; Thomas J. Misa, "Toward an Historical Sociology of Business Culture," *Business and Economic History*, 25 no. 1 (1996): 55–64; Johan Schot, "The Usefulness of Evolutionary Models for Explaining Innovation: The Case of the Netherlands in the Nineteenth Century," *History and Technology*, 14 (1998): 173–200; and Edward Constant, "Recursive Practice and the Evolution of Technological Knowledge," in *Technological Innovation as an Evolutionary Process*, ed. John Ziman (Cambridge: Cambridge University Press, 2000), pp. 219–233. A growing literature in business history addresses "organizational capabilities."

   Still, most constructivist studies of technology simply "end" the analysis with the "construction" of an artifact—showing that it embodies varied social, cultural and/or economic aspects. For a rare attempt to deal in a single framework with both construction and consequences, see Wiebe Bijker and Karin Bijsterfeld, "Walking through Plans: Technology, Democracy and Gender Identity," *Technology and Culture*, 41 (2000): 485–515.
3. For technology as social force, product, and process, see Merritt Roe Smith, "Introduction," in *Military Enterprise and Technological Change*, ed. Merritt Roe Smith (Cambridge: MIT Press, 1985), pp. 1–37. For the social shaping of technology, see Donald MacKenzie and Judy Wajcman, eds., *The Social Shaping of Technology*, Second Edition (Buckingham and Philadelphia: Open University Press, 1999).
4. Erika Jonietz, "Economic Bust, Patent Boom," *Technology Review* (May 2002), quotes p. 71.
5. "The TR Patent Scorecard," *Technology Review* (May 2002), quote p. 77. Keith Pavitt pointed out to me the lineage between *TR*'s "Patent Scorecard" and a famous effort to document that technology was dependent on science, the NSF-funded TRACES study of the late 1960s: Francis Narin, the founder of CHI Research, which provides the *TR* data base, was also the principal investigator for the TRACES project.
6. I summarize the Taylor-White patent case (1899–1909) in Thomas J. Misa, *A Nation of Steel* (Baltimore: Johns Hopkins University Press, 1995), pp. 195–205. The docu-

ments I consulted from the Hagley Museum, the Stevens Institute, and the Sheffield City Library contain a great deal more information on the patent suit. On the "invention" of high-speed tool steel, see the hedge by Daniel Nelson, *Frederick W. Taylor and the Rise of Scientific Management* (Madison: University of Wisconsin Press, 1980), p. 86: "The effects of these treatments, apparent in tests between October 31 and November 4, were extraordinary. [. . .] Taylor and White thus 'invented' high speed tool steel." Contrast Taylor's emphasis on the gradual evolution of the techniques to harden certain steels; Taylor (in "Art of Cutting Metals," quote 258) wrote: "During the original development [sic] of modern high speed tools," he and Maunsel White "accurately heated chromium-tungsten tools of standard shapes, one set after another, to different temperatures, and then determined their standard cutting speeds. [. . .] This accurate practical heating and running of the tools (without any theory on our part as to the chemical or molecular causes which produced the extraordinary phenomena) led to the discovery that marked improvements in the cutting speed [. . .] were obtained by heating the tools up close to the melting point."

7. Walter Vincenti, *What Engineers Know and How They Know It* (Baltimore: Johns Hopkins University Press, 1990), pp. 137–169.
8. Discussion of Fred W. Taylor, "On the Art of Cutting Metals," *ASME Transactions*, 28 (1907): 31–350, (pp. 295, 301–303).
9. On patent statistics see Naomi R. Lamoreaux and Kenneth L. Sokoloff, "Inventors, Firms, and the Market for Technology in the Late Nineteenth and Early Twentieth Centuries," in *Learning by Doing in Markets, Firms and Countries*, eds. Naomi Lamoreaux et al. (National Bureau of Economic Research/University of Chicago Press, 1999), pp. 19–57. For a complementary fine-grained account of the U.S. patent system, see Steven W. Usselman, *Regulating Railroad Innovation: Business, Technology, and Politics in America, 1840–1920* (Cambridge: Cambridge University Press, 2002), pp. 97–176.
10. See Giovanni Dosi, Christopher Freeman, Richard Nelson, Gerald Silverberg, and Luc Soete, eds., *Technical Change and Economic Theory* (London: Pinter, 1988); Bela Gold et al., *Technical Progress and Industrial Leadership: The Growth of the U.S. Steel Industry, 1900–1970* (Lexington: Lexington Books, 1984); Norman Clark and C. Juma, *Long-Run Economics: An Evolutionary Approach to Economic Growth* (London: Pinter, 1987); Rod Coombs, P. Saviotti, and V. Walsh, *Economics and Technological Change* (London: Macmillan, 1987); Martin Fransman, *Technology and Economic Development* (Boulder: Westview, 1986); Jorge Niosi, ed., *Technology and National Competitiveness* (Montreal: McGill–Queen's University Press, 1991).
11. For recent treatments, see John Ziman, ed., *Technological Innovation as an Evolutionary Process* (Cambridge: Cambridge University Press, 2000); Daniel Headrick, *When Information Came of Age* (New York: Oxford University Press, 2000); Alfred D. Chandler and James Cortada, eds., *A Nation Transformed by Information* (New York: Oxford University Press, 2000); and Joel Mokyr, *The Gifts of Athena: Historical Origins of the Knowledge Economy* (Princeton: Princeton University Press, 2002).
12. Edwin Mansfield, "Academic Research and Industrial Innovation," *Research Policy* 20 (1991): 1–12; as cited in Lewis M. Branscomb, *Empowering Technology: Implementing a U.S. Strategy* (Cambridge: MIT Press, 1993), p. 15; George Bush address to AAAS reprinted in *Science and Technology Policy Yearbook* (Washington: AAAS, 1991), p. 64.
13. Linda Cohen and Roger Noll, *The Technology Pork Barrel* (Brookings, 1991), p. 9.

14. For a critique of the assumptions of neo-classical economics, using data from the steel industry, see Misa, *A Nation of Steel*, pp. 262–266.
15. Otto Mayr, "The Science–Technology Relationship as a Historiographic Problem," *Technology and Culture* 17 (1976): 663–673; Ronald Kline, "Construing 'Technology' as 'Applied Science': Public Rhetoric of Scientists and Engineers in the United States, 1880–1945," *Isis* 86 (1995): 194–221; Michael Aaron Dennis, "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science," *Social Studies of Science* 17 (1987): 479–518; Amy Sue Bix, *Inventing Ourselves Out of Jobs? America's Debate over Technological Unemployment, 1929–1981* (Baltimore: Johns Hopkins University Press, 2000).
16. Langdon Winner, "Where Technological Determinism Went," in *Visions of STS: Counterpoints in Science, Technology, and Society Studies*, eds. Stephen H. Cutcliffe and Carl Mitcham (Albany: State University of New York Press, 2001), pp. 11–17 (p. 15).
17. For reviews of constructivism and science studies, see Arnold Thackray, "Science: Has Its Present Past a Future?" *Minnesota Studies in the Philosophy of Science*, 5 (1970): 112–133; Arnold Thackray, "History of Science in the 1980s," *Journal of Interdisciplinary History*, 12 (1981): 299–314; Steven Shapin, "History of Science and Its Sociological Reconstructions," *History of Science*, 20 (1982): 157–211; Henrika Kuklick, "The Sociology of Knowledge: Retrospect and Prospect," *Annual Review of Sociology*, 9 (1983): 287–310; Loet Leydesdorff, "The Knowledge Content of Science and the Sociology of Scientific Knowledge," *Journal for General Philosophy of Science*, 23 (1992): 241–263; and Michael Guggenheim and Helga Nowotny, "Joy in Repetition Makes the Future Disappear," in *Social Studies of Science and Technology: Looking Back, Ahead*, eds. Bernward Joerges and Helga Nowotny (Dordrecht: Kluwer, 2003).

    For reviews of constructivism and technology studies, see John M. Staudenmaier, "Recent Trends in the History of Technology," *American Historical Review*, 95 (June 1990): 715–725; Steve Woolgar, "The Turn to Technology in Social Studies of Science," *Science, Technology and Human Values*, 16 (1991): 20–50; Wiebe Bijker, "Sociohistorical Technology Studies," in *Handbook of Science and Technology Studies*, eds. Sheila Jasanoff, Gerald Markle, James Petersen, and Trevor Pinch (London: Sage, 1995), pp. 229–256; Trevor Pinch, "The Social Construction of Technology: A Review," in *Technological Change: Methods and Themes in the History of Technology*, ed. Robert Fox (Amsterdam: Harwood, 1999), pp. 17–35.
18. Philosophy of technology is one allied discipline where the influence of constructivist studies of technology is becoming apparent. See, for instance, Hans Achterhuis, ed., *American Philosophy of Technology: The Empirical Turn* (Bloomington/Indianapolis: Indiana University Press, 2001); the chapters by Andrew Feenberg and Douglas Kellner in Eric Higgs, Andrew Light, and David Strong, eds., *Technology and the Good Life?* (Chicago: University of Chicago Press, 2000); and the chapters by Feenberg, Junichi Murata, and Philip Brey in Misa et al., eds., *Modernity and Technology* (Cambridge: MIT Press, 2003).
19. I have done this myself: "Controversy and Closure in Technological Change: Constructing 'Steel'," in *Shaping Technology/Building Society: Studies in Sociotechnical Change*, eds. Wiebe E. Bijker and John Law (Cambridge: MIT Press, 1992), pp. 109–139.
20. Andrew Feenberg writes "to the extent that much technology studies reflects Kuhn's methodological innovations, it too bears a certain elective affinity for postmodernism,

or at least for a 'non-modern' critique of Marx's [modernist] heritage." See Feenberg, "Modernity Theory and Technology Studies: Reflections on Bridging the Gap," in Misa et al., *Modernity and Technology*, pp. 73–104.

21. Researchers in the large technical system (LTS) network have often mentioned the way technologies can become "deep structures" but have devoted much more attention to the construction processes (individual and institutional system builders) behind the emergence of LTS. I am indebted to the discussion in Erik van der Vleuten, "Historiographical Perspectives on Network Technologies and Societies: The Research Field of Large Technical Systems" (Tensions of Europe paper, October 2002) <E.B.A.v.d.Vleuten@tm.tue.nl>. Efforts at cultural history of technology are another promising method for dealing with these issues; see e.g., Michael W. Brooks, *Subway City: Riding the Trains, Reading New York* (New Brunswick: Rutgers University Press, 1997); Gabrielle Hecht, *The Radiance of France: Nuclear Power and National Identity after World War II* (Cambridge: MIT Press, 1998); Lisa Gitelman, *Scripts, Grooves, and Writing Machines: Representing Technology in the Edison Era* (Stanford: Stanford University Press, 1999); Ken Alder, *The Measure of All Things* (New York: Free Press, 2002).
22. Stephen Kern, *The Culture of Time and Space, 1880–1918* (Cambridge: Harvard University Press, 1983), p. 215; Pam Woodall, "The World Economy," *The Economist* (28 September 1996): 3–4.
23. See my essays "How Machines Make History, and How Historians (and Others) Help Them to Do So," *Science, Technology & Human Values*, 13 (1988): 308–331; "Retrieving Sociotechnical Change from Technological Determinism," in *Does Technology Drive History?* eds. Merritt Roe Smith and Leo Marx (Cambridge: MIT Press, 1994), pp. 115–141, as well as Philip Brey, "Theorizing Modernity and Technology," in *Modernity and Technology*, eds. Misa et al. (Cambridge: MIT Press, 2003), pp. 33–71.
24. One exemplar for dealing with agency and structure is Eiko Ikegami, *The Taming of the Samurai: Honorific Individualism and the Making of Modern Japan* (Cambridge: Harvard University Press, 1995).
25. William H. McNeill, *Mythistory and Other Essays* (Chicago: University of Chicago Press, 1986), pp. 50–51, 128.
26. Critics of technological determinism have located such meta-narratives in the works of such distinguished figures as Alfred Chandler and James Beniger. By comparison, in his multi-level framework, Paul Edwards accepts the shortcomings of Beniger at the micro level, but praises his account at the macro level.
27. The eras are: courts (1450–1600), commerce (1588–1740), industry (1740–1851), empire (1840–1914), science and systems (1870–1930), modernism (1900–1950), war (1936–1990), and global culture (1970–2001). A concluding chapter deals with the perennial "question of technology."
28. For an overview of the project, see <www.histech.nl/tensions/>. This section draws on thinking and writing that Gabrielle Hecht and I did jointly in the context of program development and fund raising.
29. See: <www.histech.nl/tensions/>.
30. Relevant topics for study include single-issue organizations such as the European Coal and Steel Community (ECSC), European Atomic Energy Community (EURATOM), European Space Research Organisation (ESRO), and European Launcher Development Organisation (ELDO), as well as international organizations such as the International Telecommunications Union, Economic Commission for Europe, Organization for Economic Cooperation and Development (OECD), and North Atlantic Treaty Organization (NATO).

31. Anthony Heywood, *Modernising Lenin's Russia: Economic Reconstruction, Foreign Trade, and the Railways, 1917–1924* (New York: Cambridge University Press, 1999).
32. On colonial practices, see Daniel R. Headrick, *The Tentacles of Progress: Technology Transfer in the Age of Imperialism, 1850–1940* (Oxford University Press, 1988); Gwendolyn Wright, *The Politics of Design in French Colonial Urbanism* (Chicago: University of Chicago Press, 1991); Nasir Tyabji, *Colonialism, Chemical Technology, and Industry in Southern India, 1880–1937* (Delhi: Oxford University Press, 1995); William K. Storey, *Science and Power in Colonial Mauritius* (Rochester: University of Rochester Press, 1998); Suzanne M. Moon, "Takeoff or Self-Sufficiency? Ideologies of Development in Indonesia, 1957–1961," *Technology and Culture*, 39 (1998): 187–212; and Tirthankar Roy, *Traditional Industry in the Economy of Colonial India* (Cambridge: Cambridge University Press, 1999). On uranium mining and European colonialism, see Gabrielle Hecht and Paul N. Edwards, *Technology in the Cold War*, American Historical Association's series in Global and Comparative History, forthcoming; on the space program, see Peter Redfield, *Space in the Tropics: From Convicts to Rockets in French Guiana* (Berkeley: University of California Press, 2000).
33. Kjetil Jakobsen, Ketil G. Andersen, Tor Halvorsen, and Sissel Myklebust, "Engineering Cultures: European Appropriations of Americanism," in *The Intellectual Appropriation of Technology: Discourses on Modernity, 1900–1939*, eds. Mikael Hård and Andrew Jamison (Cambridge: MIT Press, 1998), pp. 101–127.
34. See e.g. Dietrich Neumann, *Die Wolkenkratzer kommen: Deutsche Hochhauser der zwanziger Jahre: Debatten, Projekte, Bauten* (Braunschweig: Vieweg, 1995); R. Bergh and J. Rutten, eds., *De Fascinatie van hoogbouw* (Rotterdam: Uitgeverij 010, 1985). I am also examining less formal works such as poems, short stories, films, and novels, including (e.g.) Samuel Spewack, *Skyscraper Murder* (New York, 1928), as well as additional materials from France, England, and Italy.
35. On the European–American interactions concerning tall office buildings, see Reyner Banham, *A Concrete Atlantis: U.S. Industrial Building and European Modern Architecture, 1900–1925* (Cambridge, MA: MIT Press, 1986); essays by Schlereth, Harrington, Wilson, and Bruegmann in John Zukowsky, ed., *Chicago Architecture, 1872–1922: Birth of a Metropolis* (Munchen: Prestel-Verlag; Chicago: Art Institute of Chicago, 1987); Hans Ibelings, *Americanism: Nederlandse Architectuur en het Transatlantische Voorbeeld = Dutch Architecture and the Transatlantic Model* (Rotterdam: NAI Uitgevers/Publishers, 1997); Lewis Arnold, *An Early Encounter with Tomorrow: Europeans, Chicago's Loop and the World's Columbian Exposition* (Urbana: University of Illinois Press, 1997).
36. Cris Shore, *Building Europe: The Cultural Politics of European Integration* (London and New York: Routledge, 2000).
37. For varied approaches to transnational phenomenon, see Reyner Banham, *A Concrete Atlantis: U.S. Industrial Building and European Modern Architecture, 1900–1925* (Cambridge: MIT Press, 1986); Arne Kaijser and Marika Hedin, eds., *Nordic Energy Systems* (Canton: Science History Publications, 1995); Matthias Kipping and Ove Bjarnar, eds., *The Americanisation of European Business: The Marshall Plan and the Transfer of US Management Models* (London/New York: Routledge, 1998); Herbert Gottweis, *Governing Molecules: The Discursive Politics of Genetic Engineering in*

Europe and the United States (Cambridge: MIT Press, 1998); Mikael Hård and Andrew Jamison, eds., *The Intellectual Appropriation of Technology: Discourses on Modernity, 1900–1939* (Cambridge: MIT Press, 1998); Ellen Furlough and Carl Strikwerda, eds., *Consumers Against Capitalism? Consumer Cooperation in Europe, North America, and Japan 1840–1990* (Lanham: Rowman and Littlefield, 1999); Jonathan Zeitlin and Gary Herrigel, eds., *Americanization and Its Limits: Reworking American Technology and Management in Post-War Europe and Japan* (Oxford: Oxford University Press, 2000).

38. William H. McNeill, *Mythistory and Other Essays* (Chicago: University of Chicago Press, 1986), p. 95.

The New Production of Reductionism in Models Relating to Research Policy

AANT ELZINGA

IN THE AFTERMATH OF the Second World War, the growth of science became a recognized policy objective. The *Frascati Manual* that was developed by OECD ministers of science and higher education in order to keep tabs on and compare funding flows to science in different countries recognized three categories for accounting: basic research, applied research and product development (R&D).[1] Encoded in the first science policy doctrine in the early 1960s, the definitions of these different types of activity gelled a mind-set, norms and criteria. Basic research was regarded as purely curiosity-oriented and free from attempts to steer it, while applied research and technological development were necessarily subject to external determination, market demands or social policy objectives, later denoted as "sectorial," e.g., defense, energy supplies, housing programs, health care, and so on.

Simplifying greatly, one can say the first OECD science policy doctrine is characterized by science-push GNP growth. This was followed by a second doctrine in the 1970s, distinguished by a belief in market or societal pull and sectorial steering (with a lot of "science for policy" but not so much "policy for science"); the third OECD doctrine, associated with the 1980s was an orchestration policy with a partial focus on basic research to stimulate new and emerging technologies; and in the 1990s, under the impact of macro-economic globalization as well as calls to sustainable development, a popular phrase became, "towards a new social contract for science."[2]

From the outset the definitions were normative, and so were the statistical householding procedures. The very definition of "innovation" is therefore contextually contingent, changing over time; in each period specific social epistemologies and historical background conditions influence the emergence and workings of different modes of boundary maintenance between science and politics.

By the late 1980s, and especially with the end of the Cold War and the collapse of the former Soviet Union, the boundaries and distinctions as originally