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TOWARD A POST-ACADEMIC SCIENCE POLICY: SCIENTIFIC COMMUNICATION AND THE COLLAPSE OF THE MERTONIAN NORMS

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ABSTRACT

This essay explores the transformation of the textual economy of science from the academic mode that prevailed in the postwar period to an emerging model of post-academic science. A close reading of Robert K. Merton's 1942 essay on the norms of science links the Mertonian norms to the patronage structure of U.S. science during the cold war. Following John Ziman's term *post-academic science* to describe the emerging standard, I trace how the norms of industrial and academic science are being combined into new forms of collaboration and patronage, and how the academic understanding of science fails to describe emerging practices. In my conclusion, I examine some recent controversies in the public understanding of science and explore how a dialectical, post-academic model helps us understand the present crisis.

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A tower of ivory becomes untenable when its walls are under prolonged assault.

— Robert K. Merton, 1942 (1973, p. 268)

After years of battering from without, the walls of the ivory tower are finally crumbling.

— Merle Jacob and Tomas Hellström, 2000 (p. 1)

INTRODUCTION

At the end of the Second World War, a new model for scientific knowledge production emerged along with a new university-based patronage structure for its support and expansion. This model, which separated academic from industrial science and basic research from technological application, helped ensure the dominance of the United States in postwar scientific research and has lasted in university culture even as the grant-centered patronage structure that sustained it declined. Many scholars in science studies and related fields have recognized that the economic and communicative structures of science today are undergoing profound changes.[1] The emerging standard, to which scholars have given such names as "Mode 2 knowledge production" (Gibbons et al., 1994) and "post-academic science" (Ziman, 2000), is radically transforming the textual economy of science. Yet scholars tend to underestimate the effects of this transformation on science itself, and the culturally dominant understanding of scientific communication in the United States remains grounded in a model of the textual economy that developed during the middle of the last century. The economic and social systems supporting this model, though unique to their time, bore important similarities to the patronage systems that had characterized science from the seventeenth century. The sociologist Derek de Solla Price could speak of the continuous growth of science from its birth until the 1980s as "exponential" because "Big Science," the rise of which he was tracing, had not yet experienced the downward pressure of constraint (1986, p. 4). Price foresaw the inevitable need for an eventual slowdown in the growth of science (1986, pp. 28-29) but could not have predicted how the patterns of support would change with the end of the Cold War, the rise of new public-private partnerships, and the information economy. We are only now starting to come to terms with the scope of these changes, and our understanding of their effect on scientific communication is even more rudimentary.

This essay shows how the textual economy of science is transforming, perhaps into something startlingly new, in part because of shifts in the *financial* economy.[2]

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[1] For the present essay, influential works include those by Gibbons et al., Bruno Latour, and John Ziman (Gibbons et al., 1994; Latour, 1993; Ziman, 2000).

[2] By *textual economy* I mean to designate the dynamic whereby individual scientific articles play specific roles within a broader self-organizing, self-evaluating system of texts. The word *economy* is meant to suggest how the academic — in this case, the scientific — text gains value by being circulated and exchanged among other texts. My perspective derives distantly from the kind of investigation initiated by Robert K. Merton, whose work emphasizing the critical importance of recognition — citation — is

Public understanding of science is still dominated by an idealized image of scientific knowledge making that descends from a model introduced by Robert K. Merton over sixty years ago. Therefore, I begin with a brief history of recent scientific knowledge production that examines the convergence between the Mertonian ideal and the postwar patronage structure. Next, following the British physicist John Ziman, I describe the emergence of a post-academic science in the period after the cold war. I conclude by proposing some elements of a policy approach to scientific communication based on a dialectical understanding of post-academic knowledge production.

THE DOMINANCE OF ACADEMIC SCIENCE

The writings of Robert K. Merton (1910–2003) have had a broad and lasting effect on how both historians and policy-makers understand the relation between scientific practice and institutional structures. As a sociologist, Merton tended to start with a few fundamental observations into practice which he then connected strongly to social theory. For example, in examining how scientists were rewarded for their research, he saw that reward came primarily in the form of recognition rather than money, an insight that helps account for the importance scientists place upon citation as a reward system.[3] None of

discussed below. Researchers who continued this aspect of the Mertonian project include Derek de Solla Price (Price, 1986), Diana Crane (Crane, 1972), and many others who formed the discipline of scientometrics. Bruno Latour and others associated with social constructivism continue the study of citation in other directions, focusing on how, for example, specific statements get repeated and modulated in subsequent texts (Latour, 1987).

Social constructivist scholars have been attacked on various grounds, including and perhaps especially for their refusal to separate scientific knowledge from social activity (for background on social construction see Hacking, 1999). In framing the production of scientific knowledge as the outcome of a textual economy, this essay stands within this social constructionist tradition. Barbara Herrnstein Smith has done as much as any contemporary scholar to explore the epistemological problems suggested by the impasse over social studies of science. My term *textual economy* dovetails well with Smith's sense of the constructivist view of knowledge (Smith says "what we call Nature") as a "relatively stable product of the ongoing reciprocal coordinations of our perceptual, conceptual, verbal, manipulative, and other practices, formed and maintained through the very processes of our acting and communicating in the worlds in which we live" (Smith, 1997, p. 130). Smith goes on to stress the key function of "reciprocal coordination" as "not social interaction or discourse alone, and not social interaction or discourse simply *added to* empirical evidence, as the latter is classically understood, but a complex interactive process that simultaneously dynamic, productive, and self-stabilizing" (130). Neither *textual economy* as I use it nor *reciprocal coordination* as described by Smith should be confused with the quantitative concept of equilibrium in economics, though that concept is sometimes employed — as by Ingrao and Israel (Ingrao & Israel, 1990) — in the history of science. Throughout *Belief and Resistance*, where this argument is advanced, Smith connects reciprocal coordination with the autopoietic circularity characteristic of living systems. More recently, Smith has continued this line of analysis both historically (through an examination of the work of the microbiologist Ludwig Fleck) and in contemporary debates (through an engagement with the claims of evolutionary psychology) (Smith, 2005).It would be too much to ask the term *textual economy* to carry this kind of freight, but I hope at least that my affiliation is clear.

[3] This idea of citation as a kind of stand-in for direct economic reward — what is sometimes called the citation credit cycle — is often seen as a feature of academic reward generally, and it is. Historically, however, citation has held particular sway in the sciences. Of course, citation is only recognition's initial and most basic form. More desirable and elusive forms include eponymy (*the Copernican system* or *Darwinian evolution*), Nobel and other prizes and medals, memberships in scientific academies, honorary degrees, endowed positions, academic tenure, and so forth (Merton, 1973, pp. 297-302). But with the

Merton's writings on science has had more influence than "The Normative Structure of Science" (1973, pp. 266–278). This short essay attempts to define the "ethos" of science by reference to four norms or "institutional imperatives," which he calls *universalism*, *communism*, *disinterestedness*, and *organized skepticism* (1973, p. 270). Like social norms generally, the normative structure of science is not explicitly learned, and almost never explicitly taught; rather, these norms are internalized by scientists themselves as part of their scientific training. They constitute the social mores of the scientific culture, and are reinforced by cultural practices and organizational structures. Although Merton's norms are generally well known, it is worth reviewing them individually here.

Universalism means that the claims of science are not constrained by social and national markers. Because there is no such thing as American, French, or German science, the claims of science are not accepted or rejected because of "the personal or social attributes of their protagonist; . . . race, nationality, religion, class, and personal qualities are as such irrelevant" (p. 270). Universalism for Merton does not mean that the claims of science are universally applicable or universally true; his point is that limits on scientific claims are determined by the rules of science rather than by the prejudices of society. The norm of universalism has implications beyond the negotiation of claims: it means, for example, that "careers [must] be open to talents" and that though scientists may be bigots or snobs in daily life, social prejudice must not be allowed to affect the behavior of scientists as scientists (p. 272).

Communism, which Merton sometimes put in quotes, should not be confused with the political system of the same name. The communistic norm refers to the sharing of scientific information among scientists and for the good of the scientific enterprise. In Merton's eloquent phrasing, "Property rights in science are whittled down to a bare minimum by the rationale of the scientific ethic" (p. 272). The products of science are public property, and so the practice of scientists must affirm the public character of knowledge. "Secrecy," Merton wrote, "is the antithesis of this norm" (p. 273); scientists may not hoard the information they develop or the conclusions they draw, but they must freely share their results, methods, and materials.

Disinterestedness, like communism, is subject to confusion. By referring to science as disinterested, Merton does *not* mean that scientists possess no internal motivation. Scientists are surely guided in their work by passions and commitments; however, in submitting their work to peer review and testing by the scientific community, Merton pointed out, scientists subordinate their own interests to the wider protocols of the institution. In scientific communication, the norm of disinterestedness is upheld by such

publication and later digitization of the *Science Citation Index* (*SCI*), citation has come to play a direct role in validating these later forms. For example, citation rates and journal impact factors (the impact factor is a yearly calculation of citation rates for specific journals over the previous two years) are regularly used in making tenure decisions for academic scientists; tenure enables other kinds of advances; and so on. Definitions and defenses of impact factors and citation indexing have been provided by Eugene Garfield, creator of the *SCI* (Garfield, 1994a, 1994b, 1994c, 2004). Derek de Solla Price was the first scholar to examine how the *SCI* mapped the developing front of research (Price, 1965). Following Price's work, later researchers as well as practicing scientists have examined citation indexes and the related concept of the journal impact factor as tools for, and obstacles to, proper recognition (Callaham et al., 2002; Colquhoun, 2003; Lee et al., 2002; Seglen, 1997; Weingart, 2005). For a recent comprehensive study, see Blaise Cronin (Cronin, 2005).

practices as the correction and the retraction. Although sometimes a scientist refuses to accept the judgment of the larger scientific community, the consequences of such refusal are severe. The norm of disinterestedness, Merton writes with what we would now see as misplaced optimism, helps explain "[t]he virtual absence of fraud in the annals of science" (p. 276). The public admires science precisely because of the separation of science from social interest. This admiration is dangerous, however, because it makes the public more likely to be swayed by pseudo-scientists — or what Merton called "new mysticisms" — who use "the borrowed authority of science" to influence political and other authorities (p. 277).

Organized skepticism, according to Merton, is a methodological as well as an institutional norm — think of the routine practices of hypothesis testing and experimental control. But it has broader implications as well. Because the scientist "does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect and that which can be objectively analyzed" (pp. 277–278), science sometimes comes into conflict with sources of religious, economic, or political authority.

This list of norms was not final. In later writings Merton talked about the norms of *originality*, which allows all the norms to function within a reward system that sets great value upon priority of discovery (1973, pp. 297–302), and *humility*, which may be viewed as an outcome of disinterestedness (1973, pp. 303–305). Like all social norms, the norms of science "are expressed in the form of prescriptions, proscriptions, preferences, and permissions" (1973, p. 269). The norms as such are rarely stated directly; rather, scientists learn to behave in certain ways — by sharing data freely, for example, or by accepting the refutation of a cherished idea as part of the process — that help these norms emerge as tacit limits of acceptable behavior within science.

Merton's norms are affirmed by standard elements of communicative practice in science. Notwithstanding the various complaints periodically made about its form, the stereotyped IMRAD (Introduction, Methods, Results, and Discussion) structure of the research article provides a powerful generalized template for the articulation of scientific claims under the Mertonian model. The citation patterns of the Introduction and Discussion affirm the norm of humility while staking a claim for originality as well. The Methods section is surely too much detail for most readers — how many readers want to know that much? —yet its very presence affirms that scientific knowledge is community property. By excluding evaluative comments from the Results section and confining them to the Discussion, the IMRAD form acknowledges the presence of interests and motivations while ensuring that such interests do not determine the shape of the data. In addition to such structural considerations, the publication of an article at the end of the peer review process is a small celebration of organized skepticism, while that same publication marks the beginning of skepticism's next round in the subsequent generation of replications, refutations, and refinements. Or so the authors hope: for their worst fate is to be ignored entirely.

Merton's essay has become one of the classic early texts in the sociology of science. There are ways, of course, that Merton seems to idealize the practice of science, removing not only race, class, and gender from everyday science but also behavioral factors such as ambition and ideological commitment. (One could respond to this critique that Merton never locates the norms of science within individuals but always at the level of communal practice, which should provide a corrective to individual aberrations; but of course, this solution does not address the problem of collective prejudice guiding practice (e.g., Tuskegee), or collective nationalist ambition or ideology.)[4] Yet though Merton's particular form of analysis may seem outdated from one perspective, the norms he named in 1942 have persisted impressively in public understanding. We still tend to assume that science follows the Mertonian framework — or would, if social factors did not keep getting in the way. *Of course* claims should be evaluated on their merit, not on who made them; *of course* scientific knowledge should be open to inspection and evaluation; *of course* personal interests should be subordinated to the scientific enterprise; *of course* the institutions of science should pursue rigorous testing of hypotheses. Such views are hardly controversial; they represent the conventional wisdom about what we think, or what we hope, science to be. (The status of such views as conventional wisdom helps explain the widespread resistance among scientists to strong claims by the sociology of science, which are taken as attacking the realization, if not the ambition, of scientific practice.)

When a paper becomes a classic, however, it tends to lose its original historical context: that is what being a classic means. But this context is worth recovering. Published in 1942 in *The Journal of Legal and Political Sociology*, what we now know as "The Normative Structure of Science" was originally called "Science and Technology in a Democratic Order" and later reprinted as "Science and Democratic Social Structure."[5] Its successive titles remove technology and then democracy, leaving an image of basic science being conducted on its own, disconnected from social order or questions of technological application. These changes push "The Normative Structure of Science" toward the very universalism it embraces. Yet nobody who reads even this later version can miss its specific, highly local context: the shadow of the Second World War haunts the paper at every turn. Even without the word in the title, the structure Merton represents is clearly a democratic one. Moreover, throughout the essay, *democratic* science is distinguished from the *Nazi* science it opposes. "Crisis," Merton writes, "invites self-appraisal" (1973, p. 267), and the war, which the United States had officially entered only the year before, provided the ultimate invitation. The political dimension of Merton's point is unavoidable:

> The Haber process cannot be invalidated by a Nuremberg decree nor can an Anglophobe repeal the law of gravitation. The chauvinist may expunge the names of alien scientists from historical textbooks but their formulations remain indispensable to science and technology. However *echt*-deutsch or hundred-percent American the final increment, some aliens are accessories before the fact of every new scientific advance. The imperative of universalism is rooted deep in the impersonal character of science. (1973, p. 270)

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[4] Blaise Cronin: "Social constructivists may decry the lack of attention to individual practice, materiality, grounded observation, and situated action in sweeping, functionalist interpretations of science, but for some of us Mertonianism continues to offer a suave theoretical framework that melds institutional with individual motivations" (Cronin, 2005, p. 6). The "social constructivist" is a straw opponent in this passage, but the defense of Merton is reasonable.

[5] "Science and Technology in a Democratic Order" was published in the *Journal of Legal and Political Sociology* in 1942; "Science and Democratic Structure" was published in Merton's *Social Theory and Social Structure* (1949). I am using the version in *The Sociology of Science* (1973).

This passage is given in support of the norm of universalism, but the other norms, as well, are described with Nazi science in mind, while also casting a sideways glance at the emerging threat of the Soviet Union.[6] At first glance, Merton's essay simply *defines science* in a way that excludes Nazi science. But its larger purpose, as the original title suggests, is to *define the kinds of society* that will be most supportive of scientific discovery. Such societies admit to self-correction, allow the revision of previously sacrosanct ideals, and subordinate personal interests to common pursuits, all the while allowing individuals the freedom to pursue original work unconstrained by the demands of authority. Not surprisingly, such supportive societies will tend to resemble a kind of idealized Western liberal democracy.[7]

A comprehensive historical interpretation of Merton's norms would take us beyond the scope of this essay. In any event, there is a rich and growing literature on science and its patronage during the Cold War, not only in the United States but also in the Soviet Union and elsewhere.[8] My point here is merely that the norms Merton laid out in 1942 fit so well with actual postwar scientific practice in the United States in part because they dovetail nicely with the social structure of scientific patronage that emerged simultaneously and lasted until recently. This structure began to take shape during the war itself with such government agencies as the Office of Scientific Research and Development (OSRD), which oversaw the Manhattan Project and other wartime research efforts. And while the Manhattan Project's primary research ambitions were realized in the bombs dropped on Hiroshima and Nagasaki, nuclear-related wartime research projects continued after the war: the Office of Naval Research (ONR), which supported basic science research in nuclear physics and related areas, was established in 1945; the Manhattan Project's research work continued to be pursued, after 1947, by the Atomic Energy Commission. In addition, a number of wartime research laboratories with university connections continued to operate, including the Applied Physics Laboratory at Johns Hopkins, the Jet Propulsion Laboratory at Cal Tech, and the Berkeley Radiation

[6] See the note added in the later version which discussed how "By 1948, the political leaders of Soviet Russia strengthened their emphasis on Russian nationalism and began to insist on the 'national' character of science" (Merton, 1973, p. 271 n. 6).

[7] At times, the ideal manifestation of the Mertonian norms closely resembles the social democracy of the Roosevelt administration, including its mixed economy. Merton notes that "the ethos of democracy includes universalism as a dominant guiding principle," but also observes that "insofar as laissez-faire democracy permits the accumulation of differential advantages for certain segments of the population, differentials that are not bound up with demonstrated differences in capacity, the democratic process leads to increasing regulation by political authority. . . . The political apparatus may be required to put democratic values into practice and to maintain universalistic standards" (Merton, 1973, p. 273). Later, Merton writes that the norm of communism "is incompatible with the definition of technology as 'private property' in a capitalistic economy." However, though noting that some scientists have responded to this contradiction, "by advocating socialism," Merton does not go that far. Rather, he points out that others have leveraged their intellectual property rights in the service of the scientific ethos, coming "to patent their work to ensure its being made available for public use" (Merton, 1973, p. 275). So if the norm of communism conflicts with intellectual property rights under capitalism, those very rights, for Merton, provide a way of assuring the freedom of scientific inquiry through ensuring free access to patented work.

[8] See for example Geiger (Geiger, 1992) and the articles collected in the 2001 issue of *Social Studies of Science* devoted to "Science in the Cold War" (Cloud, 2001; Gerovitch, 2001; Hounshell, 2001; Solovey, 2001a, 2001b; van Keuren, 2001).

Laboratory and the Los Alamos National Laboratory, both run by the University of California (Geiger, 1992).

The most important postwar development in government science funding involved a significant shift in policy. On July 25, 1945, less than a month before the end of the war, OSRD Director Vannevar Bush submitted his report *Science: The Endless Frontier* to President Truman. (Franklin Roosevelt, who commissioned the report the previous November, had died in April.) Some federal grants for scientific research already existed: in particular, the National Institutes of Health (NIH) had a limited program, and the Department of Agriculture also offered grants for research under its general mandate. Vannevar Bush, however, advocated a greatly expanded federal grants program for basic science research under a single administrative unit — a National Research Foundation — that would provide great leeway to researchers in determining the shape of their research. Bush's proposed foundation rests on five principles: (1) long-range support for research; (2) an administrative agency composed solely of people selected for their "interest" and "capacity"; (3) a structure of grants provided directly to researchers outside the government; (4) "policy, personnel, and the method and scope of the research" left entirely to the grant recipients; and (5) foundation (not grantee) accountability to the President and Congress (Bush, 1945). Bush's report never mentions Merton; but his report, designed to provide scientists with autonomy and freedom of inquiry, supports a conception of basic science that is Mertonian in all of its essentials.

In advancing a lofty vision of scientific progress that was broadly consonant with the Mertonian norms, Bush's proposal was rather bold. For example, Bush's report embraced the communistic ideal by advocating the postwar lifting of security restrictions on wartime knowledge:

> It is my view that most of the remainder of the classified scientific material should be released as soon as there is ground for belief that the enemy will not be able to turn it against us in this war. Most of the information needed by industry and in education can be released without disclosing its embodiments in actual military material and devices. Basically there is no reason to believe that scientists of other countries will not in time rediscover everything we now know which is held in secrecy. A broad dissemination of scientific information upon which further advances can readily be made furnishes a sounder foundation for our national security than a policy of restriction which would impede our own progress although imposed in the hope that possible enemies would not catch up with us. (Bush, 1945)

Recall that in Europe, where the ink was barely dry on the German surrender, the former theatre of war was the site of a scramble among the Allied powers for Axis war secrets and rocket technology. Vannevar Bush's advocacy of "a broad dissemination of scientific information" in such a context, and where new threats were already being perceived, could be seen as fairly visionary. So was his advocacy of a research program that was run by science professionals rather than political appointees, that distributed monies without narrowly focused national-interest ends, and that saw administration of the foundation as a buffer between scientist and elected officials. John Ziman points out that the patronage structure of postwar academic science would create many such buffers: "all patronage, public or private, is channeled through *communal* filters," primarily the filters of peer review (Ziman, 2000, p. 52). Seeing the grant as a political buffer is one way of understanding its difference from the research contract: whereas contract funding requires that the contractor pursue ends specified by the contracting agency, grants are given for goals identified *by the applicant*. Of course, the applicant must appeal to the values and concerns of the contracting agency, and the success of a grant application may depend on factors beyond the applicant's control. Nevertheless, grants allow researchers a striking amount of freedom, and it was this freedom — a necessary condition for pursuit of science according to the Mertonian norms — that Bush was keenly interested in maintaining. It was as though Bush were imagining the ideal structure of government science funding under Mertonian terms, and was challenging the administration to imagine it with him.

Though Bush's plan was not wholly adopted, many scholars acknowledge that the essential outline of his idea for distribution of U.S. federal monies to science has become the standard (e.g., Geiger, 1992; Wilson, 1983; but also Mowery, 1997). The new model took hold quickly. The NIH expanded its grants program starting in 1947, and the National Science Foundation (NSF), the federal agency that came closest to meeting Bush's vision, was created in 1950. Grants for defense-related basic science came from the ONR and other defense-related agencies, and still other agencies supported other areas of basic science research. In addition to individual research grants, the NSF and other federal agencies supported basic scientific research more generally, especially within universities. As John T. Wilson notes, "it was support of basic research, almost exclusively within university and college settings, that established and cemented the [National Science] foundation's relationships with the higher education community" (1983, p. 9). More than simply an institutional relationship, this support mechanism helped define academic science in basic research terms and, insofar as science was identified with basic research, likewise defined science itself under the Mertonian norms.

That the Mertonian norms were idealizations rather than accurate descriptions of practice seems obvious in hindsight, but the postwar period was an idealizing time, and science took its place within the general narrative of triumph. With expanding government support for basic science and a perceived need for continued defense research, the quarter century following the Second World War was a golden age of U.S. scientific expansion. According to one study, annual research and development spending (both public and private) between 1953 and 1970 increased at an average of 6.7 percent, adjusted for inflation (Brown, 1998, p. 13). This same study showed that from 1957 (the year of the Sputnik crisis) through 1971, the number of Ph.D.s in science and engineering showed an average annual growth rate of 9.4 percent (Brown, 1998, p. 16). University science departments grew as well. Not fast enough, however: over the past half century, the period of apprenticeship in science has greatly expanded. Once unusual, postdoctoral fellowships have become the norm in most fields. Scientists-in-training now may go through two or three postdoctoral positions before obtaining a permanent position (if they ever do).

As a result of such differential growth rates, academic laboratories became larger, more hierarchically structured, and more dependent on grants for both research and salary support. Instead of paying a traditional salary, some U.S. research universities came to expect that, after a designated start-up period, science research faculty would begin to support *themselves* through grants. A new nomenclature emerged to describe the labor of funded research: the traditional academic identifiers (professor, postdoctoral fellow, graduate student) were supplemented — and to an extent supplanted — by labels such as Principal Investigator (or PI), Co-PI, Research Associate, and Research Assistant. Knowledge workers in academic science began to be identified by their role in funded research rather than by their positioning in the academic hierarchy. Such identifications allowed labs to be mobile, and occasionally entire labs would move from one university to another. But universities reaped other benefits from such possibly divided loyalties. Government grants for basic science under the NIH or NSF were awarded for a specific research project, but an additional amount was also awarded to the institution as such (the specific overhead rate was determined by negotiation). Like direct awards, these indirect costs were subject to less stringent oversight than with traditional government contracts, though funding scandals have led to restrictions in recent years.

In the textual economy of postwar science, therefore, the writing and reviewing of grant applications has become a central part of the scientist's intellectual labor. Granting agencies typically require grant applications from prospective recipients and renewing applicants. Review boards are charged with responding to applications, and these have generated their own forms of writing. Academic research scientists are keenly aware of when their funding runs out, along with the next application deadline. Meanwhile, workshops on grant writing have sprung up, and grant-writing consultants have come to offer their precious services. For American research universities, the signature administrative unit of academic science is the Sponsored Projects Office (SPO). SPOs provide grant-writing assistance, locate possible sources of funding, obtain required administrative signatures, coordinate with institutional review boards, monitor the application process, track the disbursement of direct and indirect costs, and ensure timely and regular reporting. Here we see the cruel logic of what Merton called the Matthew Effect: the scientist who has failed in one grant application must seek more, and more urgently, while the well-funded investigator is more likely to be successful in the next round.[9]

Scientific journals also proliferated during the postwar period. Precise estimates are difficult to make owing to issues of definition and scope, but one study limited to the United States counted 2,816 scholarly scientific journals in 1960 and 6,771 such journals in 1995 (Tenopir & King, 2000, par. 7) — an average annual increase of 4.0 percent. Moreover, though the growth in scientific journals was not limited to the United States, and though many other countries began to emulate or modify the American grants model, the steady increase in United States science funding helped secure American dominance of scientific publishing, and of the language of science itself.[10] Scientific journals are still published in many languages, but English has been the *lingua franca* of most scientific communication in the West for several decades.

In a nutshell, then, the textual economy of science in the postwar period developed through a simultaneous expansion in scientific research, government grants,

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[9] The name of the Matthew Effect is taken from Matthew 25:29, which reads "For to all those who have, more will be given, and they will have an abundance; but from those who have nothing, even what they have will be taken away."

[10] One would not want to overstate the effects of science funding as such on U.S. dominance in the postwar period. Unlike Europe, America after the war did not have to struggle with a devastated infrastructure. Moreover, the postwar increase in U.S. education (including science education) was facilitated by the GI bill, which helped dampen the effects of postwar unemployment.

and scientific journals themselves. A strange consequence of this expansion was the establishment of "page charges" (a cost per published page of an article that its authors remit to the journal) as a standard practice in many U.S. science journals. In traditional scientific publication, authors subsidize journals, which pay authors back through the "indirect" benefit of publication itself (Bachrach et al., 1998). Charges range widely according to field and journal, and page charges may have become less common in recent years, but in some instances a primary author may pay several hundred dollars for each article — and hundreds more if color figures are involved. Page charges are made possible by the structure of American science funding, where they are specifically covered in a typical basic science grant budget. In recognition of this, some journals waive page charges for scientists writing from countries where such funding is not available.[11]

Though they may seem a minor concern, page charges are worth considering because they cut to the heart of the textual economy of science in the postwar United States. This one distinctive practice illustrates the entire "peculiar social arrangement" of academic science (Ziman, 2000, p. 50), with money distributed and redistributed throughout the scientific community. Page charges are an understandable outcome of a grant-based patronage structure combined with high-cost publishing for a highly restricted audience. Through page charges and other mechanisms, the state subsidizes the publication of scientific journals. Such subsidies may have helped journals maintain high production costs. Certainly they did not drive costs down. Yet if the cost of scientific publishing can seem astronomical, the actual increase in published work represents a triumph of government funding for science — as long as that funding is sustained. Through page charges, scientific publishing is insulated from economic reality, just as academic science attempts to create a space for the unfettered support of inquiry itself. Around the ivory tower of Mertonian academic science, grant money flows like a moat.

POST-ACADEMIC SCIENCE

The public image of science during the postwar period was that of academic science. By contrast, research performed within industrial settings during this same period was hardly seen as science at all but rather as "technology" or applied science. The images contrast in every particular: the institution of academic science is the university, and the institution of industrial science is the corporate Research and Development (R&D) laboratory; academic science is supported by grants, and industrial science is underwritten by real or anticipated profits; academic science is driven by both curiosity and opportunity, and industrial science is driven by a business agenda; academic science is disseminated widely through peer-reviewed publication, and industrial science

[11] In traditional commercial publishing, of course, authors are *paid* for their work. As Merton notes, scientists are paid in the form of recognition, and so we should not be surprised that academic writers are not paid for their research articles. However, actually reversing the model of commercial publishing is another thing entirely. Moreover, page charges are not a feature of academic writing as such, but only of science: in the humanities, for example, the concept of page charges is unknown. Everywhere except in the limited domain of scientific journals, requiring authors to pay for publication is known as "vanity publishing." In fact, some journals have felt legally compelled to identify articles supported by page charges as advertisements.

is closely held and restricted; academic science (generally) focuses on basic questions, and industrial science (generally) focuses on application.[12]

The reality of science is more complex and multifaceted, of course, but I am talking about the scientific idea — how science is understood within educational and cultural systems. John Ziman takes Merton's four original norms, adds his later norm of *originality*, and spells out the reward system of academic science as "CUDOS" (communist, universal, disinterested, original, skeptical). Industrial science contrasts with academic science, Ziman notes, by being *proprietary* (rather than communal), *local* (rather than universal), *authoritarian* (rather than disinterested), *commissioned* (rather than original), and *expert* (rather than skeptical) (2000, pp. 78-79). "It is no accident," writes Ziman, "that these attributes spell out 'PLACE.' That, rather than 'CUDOS,' is what you get for doing good industrial science" (2000, p. 79). ***Table 1*** lists the academic and industrial norms side by side.

Table 1: Academic vs. Industrial Science

Academic Science	**Industrial Science**
Communalist	**P**roprietary
Universal	**L**ocal
Disinterested	**A**uthoritarian
Original	**C**ommissioned
Skeptical	**E**xpert

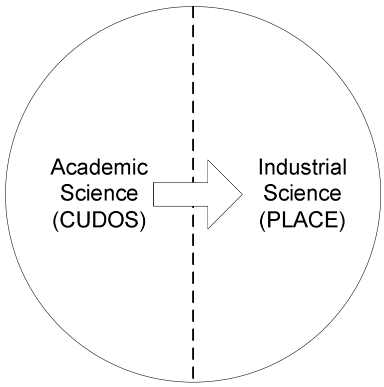
The postwar patronage structure was designed to create a designated space for the pursuit of academic science outside the norms of industrial science. More than outside, however: also prior to, leading toward, and subject to application by, industrial science. Vannevar Bush and others who crafted postwar science policy assumed that scientific knowledge flowed in a single direction — from basic science to technology. For them, knowledge was created in the academic laboratory and then developed industrially. Perhaps their involvement in the Manhattan Project reinforced this picture of knowledge. In any event, for several decades after the war, academic science was assisted by government patronage to operate separately from industrial science. Although the separation of academic and industrial science was never complete, the only acknowledged transition was through technological application (see ***Figure 1***).[13]

[12] This is a simplification. The very existence of Science and Technology Studies (STS) as a field is a recognition of the complex and multifaceted relations among these areas of knowledge-making. Yet the American research university is organized to keep these domains separate. At the undergraduate level, the basic sciences are usually housed in some configuration of the liberal arts college, whereas the applied sciences are contained in schools of engineering and computer science. In graduate and medical schools, divisions between basic and clinical research divisions perpetuate the conceptual divide. Of course, these divisions have practical justifications as well, such as the additional layers of accountability required when conducting research using human subjects. My point here is precisely that these organizational divisions reinforce an intellectual model which is increasingly coming to resemble myth.

[13] Throughout the heyday of American academic science, one heard talk that while the United States excelled in developing knowledge, other countries were much more capable of exploiting such knowledge in technological application. Occasionally this kind of cultural resentment led to discussions about developing a national research strategy in the United States modeled along lines developed elsewhere (e.g., Japan). The very idea of such a strategy was derided by defenders of academic science as an infringement

The last couple of decades have seen significant changes in the textual economy of science. But because these changes are diverse, diffuse, and ongoing, it has been difficult to get a handle on precisely what is happening (Geiger, 2004). Academic science still exists, of course, along with its patronage structures. Some of the postwar trends of have continued: scientific journals keep proliferating, and the dominance of English in scientific writing may even have accelerated in recent years.[14] Moreover, some areas of research (e.g., AIDS, bioterrorism) have received enhanced support in response to specific crises: these local spikes have tended to mask a broader decline in government support since the end of the Cold War. Yet in the view of many observers, we have entered a new era of scientific knowledge making.

Figure 1: The Separation of Academic and Industrial Science



What should we call this era? One international group of researchers has argued that we are entering a "Mode 2" form of knowledge production, where "Mode 1" refers to what we have been calling academic science (Gibbons et al., 1994). John Ziman, whose distinction between academic and industrial science was discussed above, gives the emerging system the name *post-academic science*, which is the term I will use here (Ziman, 2000). What is changing is our definition of science itself: the new regime shows that the idealized picture of academic science no longer holds (if it ever did). The term *post-academic science* suggests that science now fits neither the academic nor the industrial model. For example, the academic science model of knowledge production assumed that knowledge began in the laboratory or other academic setting and moved out in the direction of application or technology. By contrast, post-academic science acknowledges that technological changes may drive basic research: knowledge moves in

on the academic scientist's (Mertonian) freedom of inquiry. But such freedom was highly exaggerated; in fact, funding priorities always highly constrain research efforts and effectively set the boundaries of a national science policy in a thousand small ways. On the other hand, talk about creating such a national research strategy has returned with the new instability that developed after 2001.

[14] Recently the international publisher Elsevier conducted a forum on "the increase in papers submitted from non-native English speaking sources." Journal editors reviewed options for rectifying the "all-too common predicament" of receiving manuscripts in poor English. None of the participants, however, embraced the option that authors "seek publication solely in their native languages," because for better or worse, "English is currently the language of science, technology, and medicine the world over" ("Language Polishing Issues and Options", 2005). Elsevier is located in Amsterdam.

both directions and may be created at the point of application (Gibbons et al., 1994). Post-academic science is as frequently driven by technology itself, and the accumulation and analysis of massive amounts of data, as it is by the testing of hypotheses.

The Mertonian division of academic from industrial science may be a myth, like the idea of a single, universally applicable scientific method (Bauer, 1992). But if so, then it is a myth reproduced by educational structures, extended into the world by numerous daily practices, and supported by an elaborate network of political and economic relationships. None of the features of post-academic science is absolutely new; its roots go back all the way to the early days of academic science itself.[15] The military-industrial complex continually blurred the lines between science and technology, between research and industry, and between academia, industry, and government (Geiger, 1992). For example, the Advanced Projects Research Agency (later the Defense Advanced Project Research Agency or DARPA) was created at the high point of academic science but connected industry, government, and academia form its inception; later, it provided key support for the early networking systems that later became the Internet (Leiner et al., 2003). What is different about the contemporary moment is that various trends of networking and interconnection, some of them with a long ancestry, have converged and have extended into virtually all areas of scientific inquiry, suggesting that traditional understandings of the contract between science and society are fundamentally shifting (Slaughter & Rhoades, 2005).

As an emerging social practice and textual economy, post-academic science eludes precise definition. But we can identify several important trends.

(I) Post-Academic Science Multiplies the Sites of Knowledge Production

The science of the single lab is slowly being replaced by networks of scientific actors collaborating among multiple sites, even internationally, while retaining their own host operations. Of course, scientific collaboration is hardly a new phenomenon. But academic science tended to forge its collaborations out of long-term relationships (such as mentor and protégé) or as a result of interactions among scientists at meetings. In post-academic science, collaborations among scientists are increasingly possible where the scientists have never met in person. These collaborations — we might call them virtual labs — may last only as long as the experiment, after which each person or group will go its way. Moreover, in post-academic science different *kinds* of institutions are prone to collaboration. A post-academic project may begin in a university but branch out to include consultants, technicians, and researchers from industry and government. Alternatively, a research project may begin in a corporate setting but enlist the aid and expertise of academic researchers. On the other side of the virtual lab is the virtual research corporation, an organization with few permanent employees funded by government research contracts. The virtual research corporation subcontracts specific

[15] Critiques of the academic science structure also go back to its beginnings. As early as 1945, scientists involved in the Manhattan Project formed the Federation of American Scientists (FAS) in opposition to nuclear weapons proliferation; the FAS now has a broad mission "to provide the public, media and policymakers with high-quality information to better inform debates and decisions on science-related issues" (Federation of American Scientists, 2005).

tasks and parts of tasks to other corporations and works with academic scientists as partners or consultants to develop publications and maintain credibility.

(II) Post-Academic Science Makes Scientific Knowledge more Open to Public Scrutiny

The same technologies that make virtual labs and corporations possible also make scientific information more widely distributed and disseminated. Take the scientific journal article itself, which in academic science publishing was confined to the boundaries of the IMRAD format. In recent years, scientific journals have started to publish complete data sets accompanying print publication on the web; thus, print articles grow smaller even as the amount of associated available information becomes (as a practical matter) unlimited. Some long-running print journals which have established an internet presence are making such "supplemental materials" a requirement of publication, and journals based entirely on the web practice such openness as a matter of course.[16] At least in theory, this means that both the public and fellow scientists are able to examine claims made in published papers more closely.

If the page charge system of traditional print journals is emblematic of patronage under the academic model, the growth of "open access" scientific journals represents a real threat to that model. Open access was defined by the Budapest Open Access Initiative as follows:

> By "open" access to [scientific and scholarly] literature, we mean its free availability on the public internet, permitting any users to read, download, copy, distribute, print, search, or link to the full texts of these articles, crawl them for indexing, pass them as data to software, or use them for any other lawful purpose, without financial, legal, or technical barriers other than those inseparable from gaining access to the internet itself. The only constraint on reproduction and distribution, and the only role for copyright in this domain, should be to give authors control over the integrity of their work and the right to be properly acknowledged and cited. (Chan et al., 2002)

The connections to the Mertonian ideal of communism are clear here, but this time realized materially in a model of publication and a revision of copyright. Some open access publications mimic the form of traditional print journals, including editorial boards, peer review practices, volume and issue numbers, and so forth. Others are doing away with some or all of these traditions. Generally speaking, developments in open access seek to maintain the quality control associated with traditional scientific publication while lowering publication costs and guaranteeing free use of scientific information. Open access journals are always electronic, though they may have a print component as well: in any case, open access publication seems likely to lower the cost of publishing science significantly. By maintaining a modified form of page charges along with grant and institutional support, open access journals allow libraries and individuals to use them without cost.

[16] Following the work of Kling and colleagues, Blaise Cronin distinguishes among "four kinds of electronic journals: pure e-journals (distributed only in electronic form); E-p-journals (primarily distributed in electronic form but with limited distribution in paper form); P-e-journals (primarily distributed in paper form); and P+e journals (initiated with parallel paper and electronic editions)" (Cronin, 2005, p. 16).

The larger publishers of traditional scientific journals have viewed the rise of open access publication variously. Some have embraced elements of the new practice. Others have allowed their journals to go electronic, but only in a form that exactly replicates the format and structure of the print journal. Some have published editorials and studies attacking open access on both economic and scientific grounds. And they have reason to be afraid: if anyone can read scientific literature in open access journals, yet it costs thousands of dollars a year to subscribe to a print journal bundled with other print journals by a major journal publisher, why continue to subscribe to the restricted publication? But open access is not the only threat to traditional publication; in fact, the distribution systems of the Internet allow individual researchers to pose challenges as well. For example, the web sites of working researchers may contain the complete texts of recent publications in traditional science journals, works in progress, abstracts of seminar presentations, links to related papers, and other information. These practices vary widely by discipline, with physicists and mathematicians, for example, being far more likely to house work in progress on the web than chemists (Cronin, 2005, p. 18). Post-publication, electronic versions of print articles may be floating on the web even if the "published" article remains behind the firewall of a proprietary database.

The relationship of open access to print publications was not always antagonistic. When Paul Ginsparg founded the ArXiv preprint server for physics and mathematics, he envisioned an electronic version of the kinds of exchange that had always gone on in those fields, where researchers exchanged to-be-published articles ("preprints") as a means of accelerating communication (see Judson, 2004, pp. 329-338). More recently, however, he has suggested that such servers herald the death of the print journal and even of traditional peer review. On the other hand, he notes,

> Ultimately, issues regarding the correct configuration of electronic research infrastructure will be decided experimentally, and it will be edifying to watch the evolving roles of the current participants. Some remain very attached to the status quo, as evidenced by responses to successive forms of the PubMedCentral proposal from professional societies and other agencies, ostensibly acting on behalf of researchers but sometimes disappointingly unable to recognize or consider potential benefits to them. . . . It is also useful to bear in mind that much of the entrenched current methodology is largely a post World War II construct, including both the largescale entry of commercial publishers and the widespread use of peer review for mass production quality control (neither necessary to, nor a guarantee of, good science). Ironically, the new technology may allow the traditional players from a century ago, namely the professional societies and institutional libraries, to return to their dominant role in support of the research enterprise. (Ginsbarg, 2001)

The major journals such as *Science* and *Nature* are not threatened — yet. But the speed with which some journals (such as the *British Medical Journal*) have adapted to open-access practices and protocols suggests that the dominant model of scientific publication will not dominate forever. Open access publications are also having impact in the literal sense: what is called the "impact factor" of the journals associated with the Public Library of Science has risen rapidly and beyond anyone's expectations.[17] This

[17] The impact factor (IF) is a calculation of the relative citation rates to a particular journal. The first IF of *PLoS Biology*, announced in the summer of 2005, was 13.9, which is very high even in the biomedical

distribution of information may eventually have a beneficial effect on scientific accountability; at least one scholar has drawn explicit connections between the continued freedom of scientific inquiry and the open source software movement (Kelty, 2005). On the other hand, opening data to public scrutiny also means opening it to question by groups with specific interests. Parties seeking to manipulate controversial research or to challenge established science for their own ends may find it easier to sway media organizations or legislative bodies by sowing doubt where little exists or irresponsibly buttressing dubious claims with what Charles Darwin would have called "some parade of mathematics" (Darwin, 1998, p. 264).

(III) Post-Academic Science Privatizes Academic Knowledge

A paradox of knowledge creation in post-academic science is that privatization seems inseparable from increased distribution and dissemination. Knowledge in post-academic science tends to cluster at specific sites, and these sites may be controlled by private interests. Internet databases or databanks provide repositories of information on a wide variety of scientific subjects, including chemical structure, protein mechanisms, physical constants, genetic interactions, and material properties. Some online databases also have an analog in the physical world: biological resource centers, for example, provide quality-controlled access to materials as well as information (see Stern, 2004). The privatization of scientific knowledge in post-academic science means that some data, data analysis tools, and materials may be restricted; moreover, the data, tools, and materials that are available will be subject to development by private interests.

If the signature office of traditional academic science was the Sponsored Projects Office, the signature office of post-academic science is the Office of Technology Transfer (OTT). OTTs at American universities are devoted to leveraging the value of technologies developed in the academy. OTTs apply for patents on behalf of universities, arrange licensing agreements, and craft novel partnerships with corporations. In the past few years, OTTs have sprung up even in universities not known for their research efforts. Partly due to the expansion of patent law in the last couple of decades, and partly due to the interests of their new corporate partners, universities have become more likely to view the products of basic research as subject to patent protection or licensing agreement. By patenting the results of research, universities acquire a research-based income stream that does not depend on the next grant.

(IV) Post-Academic Science Facilitates Interdisciplinary Inquiry

Disciplinary research is conducted within academic departments and separated into distinct compartments; it is, in a word, *departmentalized*. But post-academic science creates networks not only between academic departments but also among departments, technologies, corporations, and people. The multiplication of knowledge sites, the emergence of new relationships, the increased availability of technology, and the

sciences. A press release announcing the news noted that such an IF "is an outstanding statistic for a journal less than two years old, from a new publisher promoting a new business model that supports open access to the scientific and medical literature" (Public Library of Science).

increased visibility of data all point toward new, hybrid forms of interdisciplinary inquiry. Sometimes an interdisciplinary effort gains enough adherents, and enough of a unique perspective, to become a department of its own (examples include cognitive science and bioinformatics). But these new disciplines are simply names we give to the success stories of interdisciplinary work.

(V) Post-Academic Science Increases Specialization

Another paradox of knowledge creation in post-academic science is that specialization increases along with interdisciplinarity. In traditional academic science, even the most junior lab tech can, if he or she desires, get a sense of how the larger project is being conducted. Yet the very collaborative relationships that are one hallmark of post-academic science allow for an increased division of labor among the collaborators, and this division of labor, along with cost issues, may result in some work being conducted entirely by narrowly defined specialists who never have contact with the larger project. Imagine a study in which blood samples are collected from patients at multiple hospitals internationally; gene sequences are generated from each sample by locally contracted researchers; each sequence is uploaded onto a single computer server in the United States, with appropriate demographic information; the statistical analysis corporation which manages the server analyzes these data as directed by the study's Principal Investigator, who is a tenured faculty member at a university in another city; the PI works with a pharmaceutical company to compare these data with what is previously known or thought about the genetic markers for a specific disease; the company uses these data to help determine the kinds of human subjects they are hoping to enroll in a clinical trial. This hypothetical study description has not yet mentioned the other people involved, who include lawyers to manage international contracts and subcontracts; Human Subjects Research Boards at both the university and the partnering corporation; nurses at the local sites who get patients to sign Informed Consent forms and explain the study to participants; corporate scientists who have already developed a drug which they are hoping to test soon; and so forth. Each of the players in this drama conducts a specific task. Each worker has a specific and local knowledge of the study, its purposes, and its methods: while a few have a truly interdisciplinary frame of inquiry, most of those working on the study perform the same repetitive tasks without ever coming into contact with a person at another stage of the project.

(VI) Post-Academic Science Strengthens the Bond Between Science and Social Need

Not even the most pure believer in academic science would discount social need entirely. Researchers in academic science always referred to the social good when necessary. Rare was the NIH grant application that did not invoke some possible drug, cure, therapy, or clinical application as the culmination of experiments in an animal model. Moreover, the direction of research has always been subject to fads, and these fads have been associated with perceptions of urgency. However, the last few years have seen the profile of social need rise in prominence. With diseases such as HIV/AIDS and breast cancer, we saw for the first time that those affected by the disease could, through lobbying and protest, gain a recognized voice in how research was conducted. Such voices have multiplied recently with prominent celebrity cases of Parkinson's disease, Alzheimer's disease, and spinal cord injury, and with foundations advocating specific research agendas (such as embryonic stem cell research). Internationally, the intellectual property claims of pharmaceutical companies have been challenged especially with regard to the distribution of antiretroviral drugs in sub-Saharan Africa. The movement for access to medicines is of a piece with other components of the new textual economy because the very internationalization, multiplication, and distribution of knowledge that created vast new drug markets also created possibilities for resistance to being treated solely in market terms. In any event, such trends seem part of the landscape now. Perhaps addressing social need is simply more necessary than before; perhaps, on the other hand, post-academic science makes it more possible than ever to imagine the goals of research addressing social need effectively.

In post-academic science, both the meaning and the significance of projects and results must be explained to diverse groups representing a variety of stakeholders in the scientific process. As its name suggests, academic peer review typically addresses fellow scientists — peers — rather than corporate stockholders, public interest groups, politicians, taxpayers as such, and media organizations. Yet in post-academic science, these and other people and groups may have the power to increase or decrease funding for a project: they are typically interested in such issues as application, technological development, and licensing. The privatization of research in post-academic science means both that projects are imagined early in terms of their application and that projects without obvious application come under increased scrutiny. The investigator in a post-academic context is never far from having to justify a scientific project's worth and value to people who are not themselves scientists.

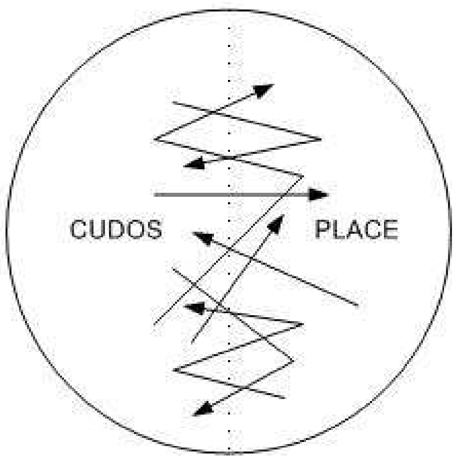
(VII) Post-Academic Science Weakens the Bond Between Science and Curiosity

One of the major justifications of Vannevar Bush's proposal was the need for scientists to conduct research for its own sake, without the constant pressure to create "useful" results. In fact, a guiding principle behind the NSF was that it was important to fund curiosity-driven research that did not necessarily have any practical end either in defense or in public health. (In this respect, NSF research sponsorship differed significantly from the NIH.) While the NSF still sponsors pure research, the space for such research is coming to seem more crowded out by research with practical applications.

Some researchers have argued that these trends, made possible by the success of the research university, have undermined its future. That is, they argue that because universities, especially graduate schools in the United States, have created many more Ph.D.'s than they can possibly hope to employ, they have populated their competitors in industry (Gibbons et al., 1994, pp. 70-89). As I have already mentioned, these trends have resulted in the extension of the period of scientific apprenticeship. Yet to the degree that these researchers argue that the university system has sown the seeds of its own collapse, their claims seem overstated. The American university system is both large and flexible, and it has adapted to the new situation with speed and efficiency; university systems in different countries have responded differently, but higher education remains important to research efforts throughout the world. True, the *role* of the university in research seems to be changing radically, but the university system is too powerful to be supplanted easily; its *status* in the new regime seems assured (Geiger, 2004).

That is one reason for preferring the term *post-academic science* over *Mode 2 knowledge production*. We are not seeing the collapse of academic science in the face of a reinvigorated industry; rather, we are seeing the norms of academic science (CUDOS) transformed by the norms of industrial science (PLACE). And vice versa (Varma, 2000). The shift to post-academic science — or the acknowledgement of the hybridization that has always taken place — brings to mind the collapse of faith in the modern as examined by Bruno Latour (Latour, 1993). Is post-academic science part of this larger transformation? They certainly occurred simultaneously. "We are all called into question," writes Latour, "by the double debacle of the miraculous year 1989" (1993, p. 10). Latour names 1989 "the year of miracles" because it marks both the fall of socialism (signified by the collapse of the Berlin Wall) and the rise of global ecological thinking and thus "the end of limitless Nature" (1993, p. 9). For Latour, these shifts signal the beginning of awareness that "the modern," which sought to separate nature from culture, also — and by the same process — created a series of hybrids or networks that linked nature and culture through and through. The relations of academic and industrial science are similarly networked in post-academic science (***Figure 2***), and their hybridization likewise makes possible the pure products of each location. In Latour's broad map of recent history, the connection between these two processes, which Latour calls "purification" and "translation," can only be seen clearly after the illusion of the modern has been abandoned. Does this mean, to adopt Latour's language, that "we have never been academic"? Perhaps. Those who have worried about the demise of traditional academic research might take comfort in the transformation of traditional industrial research that goes along with it.

Figure 2: Post-Academic Science



CONCLUSION: TOWARD POST-ACADEMIC SCIENCE POLICY

Creating frameworks for post-academic science policy is difficult generally, and no more so than when approaching scientific communication. But without an appropriate theoretical framework for understanding the current textual economy, the work of actual policy-making and regulation will tend either to replicate the problems of earlier periods or apply old solutions to new challenges. Some writers, happy enough to see Vannevar Bush's patronage model called into question, have considered how best to embrace the privatization of knowledge and the new scientific entrepreneurialism (Barfield, 1997; Brown, 1998). Others, who welcome changes in communication but refuse to accept the consequences of related and substantial changes in the shape of scientific knowledge production itself, are hoping the information revolution will revitalize the Mertonian ideals. Both views accept the divide between academic and industrial science that was the sustaining fiction of academic science. This acceptance is both practically disabling and intellectually problematic. Science students trained in an idealized version of academic science will find it increasingly difficult to navigate the post-academic career, though individual scientists may come to terms with these changes in a variety of ways (Cohen et al., 2001). Graduate students who travel back and forth between academia and industry, for example, may find themselves becoming the currency of the new post-academic economy (Slaughter et al., 2002). Such new models of exploitation are enabled by the idealized view of academic science that sends people to graduate school in the first place. Indeed, the practical challenges of thinking through post-academic science may be especially difficult for the very people — academics — who are otherwise best positioned to face the theoretical challenges post-academic science presents; for they may have a stake in the perpetuation of academic science models in particular as well as in the academic values the Mertonian ideals represent in general (e.g., academic freedom, intellectual autonomy, sharing of information, etc.).

Intellectually, the persistence of the Mertonian model for the textual economy — and by this I mean not simply the specific norms Merton named in 1942 and thereafter but the academic/industrial divide they sustain — makes it difficult to imagine a present or future practice of scientific communication that enables informational freedom without simultaneously empowering the forces of industrial and government control. All the pertinent issues are affected: access to knowledge, public scientific literacy, the power of pseudo- and anti-science movements, the practical education of scientists, intellectual property rights and responsibilities, international standards for knowledge dissemination, the philosophical and economic rationales for open-access scientific journals. Progressives are in danger of mistaking nostalgia for description, while those with vested interests in advancing industrial science may turn the Mertonian norms against science itself.

A couple of examples should drive home the severity of this misunderstanding. Consider the following paragraph from a recent (2004) book:

> Scientific knowledge often moves from a spring of open discourse into a stream of adoption and exploitation and from the public arena to the private sector. Complex protocols guide this process, each step embodying different values and ideologies. The rules and terms of discussion begin with consensus-seeking processes within scientific communities. They then consider the demands of market forces to create and enforce

> scarcity and state demands for security. Different ideologies, habits, and rules govern the "upstream" source of knowledge and the "downstream" deployment of it. But the first step, the action in the lab and the library, depends on academics' devotion to radical democracy and openness. The essential question in this matrix of rules and norms is, At [*sic*] what point in the knowledge stream should we install controls and restrict access — to encourage new technologies and to protect people from bad actors who would exploit dangerous knowledge? (Vaidhyanathan, 2004, p. 132)

This passage is from Siva Vaidhyanathan's *The Anarchist in the Library*. Vaidhyanathan stands with Lawrence Lessig (2001, 2005) as one of the most comprehensive popular thinkers on the issue of access to knowledge and the rise of digital culture. In most of *The Anarchist in the Library*, Vaidhyanathan maintains simultaneous attention both to the democratizing possibilities of new technologies (which he historicizes in terms of the anarchist tradition) and to the increasing regulation and control that those same technologies might allow. Yet in the passage above, as throughout his discussions of science, we find Vaidhyanathan embracing a stable, and wholly Mertonian, view of scientific knowledge production. Knowledge in this model moves from an "upstream" location of basic research as "open discourse" to a "downstream" practice of technological "adoption and exploitation"; from "scientific communities" to "market forces"; from freedom to control; from the "radical democracy" represented by the academy to the "bad actors who would exploit dangerous knowledge" — and who, one supposes, are not themselves academics. Vaidhyanathan's passage is haunted by a post-academic bidirectionality, since the "downstream" movement of knowledge is simultaneously an enlargement (moving out from the academy into the world) and a constriction (squeezing the "open discourse" of the university through the "adoption and exploitation" of technology). But this cannot be acknowledged, lest the picture suggest that we lack the "uncorrupted scientific communication" which Vaidhyanathan believes "a necessary (albeit insufficient) condition for improving the human experience" (2004, p. 132).

Vaidhyanathan's conflation of academic science with science in general is a typical kind of public misrecognition about science that depends on a prior distinction between basic science and science applied (Ziman, 2000). This misrecognition becomes nostalgic when it attends to changing *communicative* conditions but refuses to acknowledge changing definitions of *science*. Mistaking academic science for science as such, in other words, does not merely take the part for the whole; it effectively freezes the image of science at a particular historical moment (even if that moment was fictive in certain crucial respects) and reifies a local practice (even if that practice was globalized by the extension of a U.S. model worldwide). But the communicative dimension and the knowledge-making dimension are not only inter-related but mutually generative. Let me be clear here: I do not mean to suggest that any critical perspective is untenable, that we have no choice but to embrace the new entrepreneurial realities, or that we should passively accept whatever forms of knowledge privatization happen to come our way. I am saying that our understanding of knowledge and communication, including scientific communication, needs to be dialectical through and through; reifying scientific knowledge production is no way to develop a critical understanding of contemporary scientific communication. Critical perspectives such as Vaidhyanathan's misrecognize what those new realities are because they fail to acknowledge the actual changes in knowledge production that attend the acknowledged (and well-described) changes in communicative practice.

Consider how Vaidhyanathan deals with one of the most widely discussed challenges to Mertonian norms in recent years: the simultaneous publication of a partial map of the human genome in the weekly journals *Nature* and *Science*. As is well known, *Nature*, the British journal, published the map created by the National Human Genome Research Institute (a publicly funded program) in the traditional way, with public access to all the data; while *Science*, the American journal, published a genome map created by the private biotech company Celera Genomics but made the data generally available only in a restricted form and the unrestricted data available only to people who had passed through a selective gatekeeping process. The publication of this preliminary map of the human genome was hailed as a major scientific advance, but critics concerned with privacy and privatization viewed the means of publication — especially the restricted release of the Celera data — as an alarming development. Such concerns are not overblown, and the human genome project in general may be said to represent a key post-academic science project. Like other critics, Vaidhyanathan is scandalized by the Celera paper: *Science*, he writes, "decided to bow to the demands of" Celera, while *Nature* "did what scientific journals are supposed to do." (2004, p. 138). While Vaidhyanathan admits that the "dependable expectation of return" is crucial to innovation, and to that extent supports the idea of intellectual property protection as an incentive for development, he finds the drive to patent genetic sequences to be a dangerous development that is of a piece with the departure from traditional publication practices represented by the Celera paper (2004, p. 139).

Vaidhyanathan's position here is appealing in part because the Mertonian ideals of (for example) communism with respect to sharing of scientific data are, as explained above, so deeply ingrained in contemporary understandings of scientific communication. Yet the Celera case itself is a more complicated story than a simple abandonment of dearly held principles. *Nature* magazine, the academic science hero in Vaidhyanathan's narrative, published its genome map simultaneously with the *Science* paper by agreement. Moreover, virtually everyone involved with the project agrees that the mapping itself was vastly accelerated by Celera's contribution. And as a *Science* editorial pointed out, "Two sequences are better than one" (Jasny & Kennedy, 2001). We do not have to accept all the editorial's celebratory language in order to agree that "each project contributed to the other" and that the drive to map the human genome "has become, in the end, not a contest but a marriage (perhaps encouraged by shotgun) between public funding and private entrepreneurship" (Jasny & Kennedy, 2001). Moreover, in 2004 Celera agreed to deposit its genome sequence in GenBank, a publicly accessible gene database (Winstead, 2004). But neither is this a story of the triumph of industrial science. As it turned out, selling informational pieces of the genome did not prove nearly as profitable as anticipated; partly in the wake of this disappointing outcome Celera's charismatic founder Craig Venter left the company in 2002. Celera's decision to release the data publicly was probably market-driven in that Celera after Venter's departure was moving from gene mapping to biomarker-based drug discovery; depositing the genome information in a public database reinforced the new image of the company's purpose while not depriving it of a significant source of ongoing revenue.[18]

To describe quickly the interaction of industrial/academic principles the genome map represents, we might say that the academic community's interest in the **universal** knowledge of the genome led Celera to leverage its **expertise**; that Celera's **proprietary** interests accelerated the publication of **original** results, which led to at least some early sharing of vast information in a **communalist** fashion; that Celera's (and Venter's) **authoritarian** control over genetic knowledge encouraged others to support more **disinterested** research (and spurred James Watson into resigning from the National Center for Human Genome Research); but also that **local** differences between the two maps led to some **skeptical** testing of the data and also has led, in the longer term, to current questions regarding the use of genetic data in economic decision-making (as in health care and insurance issues), and so forth. What Vaidhyanathan calls "the scientific ethical canon" is always formulated, in his work, in Mertonian terms (2004, p. 139). Post-academic science, however, draws *both* from new technologies facilitating openness *and* from new collaborations with private entities. On the other hand, Vaidhyanathan may have a point when he represents the Celera case as the first along a slippery slope. As Yurij Castelfranchi (2004) has explained, what was first represented as a one-time departure from accepted practices of disclosure was repeated with the rice genome less than a year later. Both the human and rice genome projects involved two projects carried out by different entities; in each case, one entity allowed for full access to data while the other restricted data in various ways. Scientists being trained to enter the post-academic science community must understand the sensitive questions of ownership. As Castelfranchi writes, "Research may have been kept a secret or restricted (which delayed its popularisation) in the past, but there was an unbreakable law that science data would only be published and recognised once all restrictions had been removed (eg. after patenting a new invention). Perhaps this is no longer the case" (2004, p. 6).

We have come to view open access to scientific information as a universal good; yet the Celera case demonstrates that the relation between *access* to knowledge and its *generation* is complex and multifaceted. Moreover, it is also possible that the Mertonian norms, including the norm of communism, can be used to intimidate. Consider a set of letters sent in 2005 by Congressman Joe Barton (R-TX), the Chair of the Committee on Energy and Commerce in the U.S. House of Representatives, to several climate change scientists.[19] Among the recipients was Dr. Michael Mann of the University of Virginia, whose studies of global temperature change have been highly influential (the so-called "hockey stick" figure asserting a sharp rise in global temperature over the last century has become one of the iconic images in the field). As Barton's letter confirms, Mann's work "has become a prominent feature of the public debate surrounding climate change policy" (2005). Relying on a February 2005 article in the *Wall Street Journal*, Barton's letter demands a number of items from Mann:

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[18] An additional dimension to the public release of the Celera data is that most if not all of the mapped DNA was from Celera founder Venter (Young, 2002).

[19] The complete set of letters to Dr. Mann and others is available at the web site of the Committee on Energy and Commerce (http://energycommerce.house.gov/108/Letters/06232005_1570.htm).

> Provide the location of all data archives relating to each published study for which you were an author or co-author and indicate: (a) whether this information contains all the specific data you used and calculations your performed, including such supporting documentation as computer source code, validation information, and other ancillary information, necessary for full evaluation and application of the data, particularly for another party to replicate your research results; (b) when this information was available to researchers; (c) where and when you first identified the location of this information; (d) what modifications, if any, you have made to this information since publication of the respective study; and (e) if necessary information is not fully available, provide a detailed narrative description of the steps somebody must take to acquire the necessary information to replicate your study results or assess the quality of the proxy data you used. (Barton, 2005)

In demanding this information, Barton adopts a Mertonian stance when he writes that "sharing data and research results is a basic tenet of open scientific inquiry, providing a means to judge the reliability of scientific claims." Yet repeatedly in this letter, he refers to the federally funded status of Mann's work and the authority of the committee over such funding. Invoking the "quality and transparency of federally funded research" (Barton, 2005) is a post-academic tactic because it both supports the Mertonian norms by referring to "transparency" and undermines the Mann's autonomy by implicitly threatening his federal funding. (Recall that the fifth of Bush's principles for academic science patronage views the grant foundation as a buffer between elected officials and grant recipients.) Now, we would not be wrong to place Barton's letters within a recent tradition of right-wing attacks on the credibility of science (Mooney, 2005). But invoking scientific autonomy in response is problematic because it keeps the respondent within the very Mertonian framework that Barton has already employed. And besides, why keep the data private? The double bind of Dr. Mann's position may be partly addressed by accepting the post-academic role. Response strategies might include invoking PLACE norms of expertise and local knowledge in terms of understanding and evaluating data, demanding symmetrical transparency that revealed the extent of Rep. Barton's ties to the petroleum industry, [20] as well as those of Mann's scientific critics, effectively connecting the rise in global temperature to economic and social costs, and many others. My point is that it is naïve to expect a reaffirmation of the Mertonian norms alone or the academic-industrial divide, especially since some of the features of post-academic science (such as the connection between scientific knowledge and social need) have been crucial for sustaining a progressive scientific approach to the environmental, social, and political impacts of globalization. Crafting realistic post-academic communications policy will have to take account of these realities — their promise as well as their danger — in order to be effective.

As an educator and not a lawyer or policy-maker, I am glad to leave the particulars of such policy to those better placed to design them. I would like to conclude, however, by suggesting that an effective approach toward post-academic science — and a real transformation of what it means to have access to knowledge — needs to transform our understanding of scientific education by including "science studies" within it.

[20] According to the Center for Responsive Politics, Barton was the second-highest recipient of support from the oil and gas industries among House members in the 2004 election cycle, and as of February 15, 2006 he is also second among House members in the 2006 cycle (http://www.opensecrets.org).

Typically, of course, terms such as "public understanding of science" and "scientific literacy" refer to the kinds of knowledge possessed by scientists but not by the general public. In the dominant model of a liberal arts higher education, the person in possession of scientific literacy knows various things about science (e.g., the theory of evolution) and/or holds various cherished ideas about how science is done (e.g., "the scientific method"). But in the history I have sketched here, *scientists* are dispossessed, in their education, of a key component of scientific literacy — an understanding of the social and patronage structures that make the practice of science possible and that shape the making of scientific knowledge itself. A responsible approach to scientific literacy, broadly conceived, must recognize that any scientific education worthy of the name should convey an understanding of the social networks of science, including the rich history of its communication protocols. To twist a phrase, science studies is too important to be left to the humanists.

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