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Source: *Social Research*, Vol. 73, No. 4, Politics and Science: An Historical View (WINTER 2006), pp. 1253-1272
Published by: The New School
Stable URL: http://www.jstor.org/stable/40971882
Accessed: 31/07/2014 13:04

M. Norton Wise

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POLITICIZATION

THE CURRENT POLITICIZATION OF SCIENCE—BY WHICH I MEAN THE attempt politically to control the content of knowledge and not just the direction of research—is arguably unprecedented in history, aside from a few famous and anomalous examples like the Galileo and Lysenko affairs.[1] Although complaints have been developing for years, the first major public protest against the abuse of science by the current administration in the United States was the statement published by the Union of Concerned Scientists in February 2004, "Scientific Integrity in Policymaking: An Investigation into the Bush Administration's Misuse of Science." It charged the administration with "a well-established pattern of suppression and distortion of scientific findings by high-ranking Bush administration political appointees across numerous federal agencies" and with "a wide-ranging effort to manipulate the government's scientific advisory system to prevent the appearance of advice that might run counter to the administration's political agenda."[2] Its signatories grew to include thousands of scientists and many former government officials, with 48 Nobel laureates, 62 National Medal of Science recipients, and 135 members of the National Academy of Sciences (Mooney, 2005: 225). This consensus is as unprecedented as the scope of the abuses it protests.

Chris Mooney, in *The Republican War on Science*, provides a well documented account of the origins of the recent political manipulation of science, which goes back to the 1970s but flourished after the Republican takeover of Congress in 1994 known as the "Gingrich Revolution," with its assault on federal regulation. The signal event was the dismantling of the Office of Technology Assessment, which had functioned for 24 years as Congress's source of independent advice on issues of science and technology. The tactics of the Gingrich assault were borrowed from the long experience of the tobacco industry in defusing claims of the harmful effects of smoking—simply to fund their own research, which ended up casting doubt on the certainty of the claims even when it did not contradict them. The effectiveness of the technique is shown by a 1998 study in the *Journal of the American Medical Association* (*JAMA*) on review articles of research done on secondhand smoke, which showed that a "not harmful" conclusion was 88.4 times higher if authors had industry affiliation (Mooney, 2005: 10).

Increasingly important in the new version of this alternative research technique have been think tanks with sponsorship from industries seeking to block regulation: the American Enterprise Institute, Heritage Foundation, Pacific Legal Foundation, George C. Marshall Institute, Annapolis Center for Science-Based Policy, and others. But perhaps most intriguing in Mooney's analysis is his account of the adoption by opponents of regulation of a systematic rhetorical strategy. Research results that opposed or minimized the need for regulation, typically industrially funded, would be labeled "sound science" while pro-regulation research, typically carried on at universities and government laboratories, would be labeled "junk science" in the interest of manufacturing scientific doubt (Mooney, 2005: 65-76). The genius in this move is that the term "sound science" has been picked up in reporting by the mainstream media, often without recognizing its loaded meaning or that it is inscribed in such conservative organizations as the Advancement of Sound Science Coalition. A countermove on the part of the Union of Concerned Scientists to recapture rhetorical control has apparently not been so effective. Its Sound Science Initiative is an "email-based vehicle for scientists to respond to and influence fast-breaking media and policy developments on environmental issues."[3]

Examples of the overt attempts to control the content of science during the current administration could be taken from virtually any area of political significance: global warming, endangered species, ozone depletion, chemical pollution, or oil drilling, without even entering the fraught areas of abortion, stem cells, evolution, brain death, or the morning after pill. One example may serve to indicate how far this movement has progressed. In March 2006 the *Los Angeles Times* published a penetrating account by Ralph Vartabedian of the controversy over the solvent TCE (trichloroethylene) under the headline "How Environmentalists Lost the Battle over TCE." After a four-year study, the Environmental Protection Agency (EPA) concluded that TCE was 40 times more likely to cause cancer than previously thought and issued a preliminary report in 2001 aiming to begin setting rigorous new standards to limit exposure. Although now virtually eliminated, TCE had formerly been widely used at military installations throughout the country for degreasing metal parts and then dumped into pits where it entered the groundwater. It is reportedly the most widespread water contaminant in the nation, involving 1,400 Department of Defense sites, with 67 EPA Superfund sites in California alone. Huge plumes spread for miles, sometimes under heavily populated areas, and are identified with elevated risks for cancer and birth defects (Vartabedian, 2006a; 2006b).

What makes this story particularly interesting is the sharp upturn in politicization that it represents. It involves the Defense Department, which has traditionally been a rather apolitical organization with respect to domestic issues and, according to the lead author of the 2001 EPA report, had done everything possible to ensure environmental safety. This time, however, faced with monumental costs, the Defense Department joined with the Energy Department and NASA (both of which also have contaminated sites) to launch a full-blown attack on the EPA's science, apparently to at least delay any further remediation. Not surprisingly, they had mobilized the rhetoric of "sound science" to minimize the risks of TCE while accusing the EPA of a left-leaning bias and "junk science." They had the "sound science" backing of a toxicologist from the organization representing TCE manufacturers, the Halogenated Solvents Industry Alliance, who said that "If TCE is a human carcinogen, it isn't much of one." And they were also able to rely on the Bush-appointed research director at EPA, Paul Gilman, who said that "Inside the Beltway, it is an accepted fact that the science of EPA is not good," and that an entire consulting industry has sprung up in Washington to attack the EPA and sow seeds of doubt about its capabilities. Against a background like this, the Defense Department, NASA, and the Energy Department appealed their case directly to the White House, where it was taken up by a working group made up largely of officials from their own agencies, who had originally been assembled in 2002 to combat the EPA's assessment of another pollutant. Ultimately they referred the dispute to the National Academy of Sciences for more study (Vartabedian, 2006a: 8-9).

In July the National Research Council of the National Academy of Sciences issued its report, largely supporting the findings of the EPA, though criticizing some technical aspects of their study. They judged that the evidence of health risks had increased since 2001 and recommended that the agencies "finalize their risk assessment now so that risk management decisions can be made expeditiously" (see <www.nap.edu>). Meanwhile, five years had gone by.

COMMERCIALIZATION

Just as prominent as politicization of science in media reports has been its commercialization. In *Universities in the Marketplace: The Commercialization of Higher Education*, Derek Bok, former and now acting president of Harvard University, has given a very accessible overview of the problems commercialization poses for the entire life of the university. I limit myself to the research component. Industrial consulting, patenting, industrially funded research, and spin-off companies are not new at American universities but they have grown dramatically since the 1970s, with the percentage of academic research funding increasing from 2.3 percent to 8 percent by 2000. The turning point in commercialization came in 1980 with the Bayh-Dole Act, which made patenting of federally funded research—by the National Science Foundation (NSF), the National Institutes of Health (NIH), and other agencies—much more attractive than it had previously been by granting the right to exclusive licensing. The basic idea was to promote the public good by decreasing the time elapsed between research findings obtained with public funds and useful products. Profits to universities and researchers would be the motor. And it was a powerful motor. By 2000, university patenting had increased 10 times, earning more than $1 billion per year, and 12,000 academic scientists had established industrial connections (Bok, 2003; see also Mowery, 2004). Since then, according to a recent NSF report, patenting has remained nearly constant, as has industrially funded research, although it has dropped significantly as a percentage, from 7.4 percent in 1999 to 4.9 percent in 2004 (cf. Bok's figures) (Rapoport, 2006).

From the perspective of the traditional values of science, which rest on the free exchange of information, patenting could disrupt scientific progress through refusal to share information, materials, and instruments; monopolistic licensing practices; and the inhibition of downstream research. Anecdotal examples abound and recent statistical studies appear to confirm the inhibition effect (Sampat, forthcoming). If correct, it implies that the university, by expanding its patenting of research results, would be undermining its own mission to promote research and the acquisition of new knowledge, contrary to the intention of Bayh-Dole. Equally worrisome is that research for profit may well not be research in the public interest, so that commercialization of research may skew its direction away from what would most benefit society.

A more direct and pressing worry is that commercialization actively subverts the public interest by distorting research results. Because reports of this kind are so numerous, one example may serve for the genre. Over the last several years a controversy has blossomed over "aspirin resistance," the claim that many who take aspirin as an anti-clotting agent to reduce the risk of existing or potential heart ailments may be resistant to the drug, are at increased risk of heart attacks and strokes, and should perhaps be taking other anti-clotting drugs. An article by David Armstrong in the *Wall Street Journal* in April 2006 brought the issue to widespread public attention under the title, "Aspirin Dispute is Fueled by Funds of Industry Rivals." Researchers raising the aspirin resistance alarm have largely been funded by Accumetrics, who make the most widely used test for resistance, and by Schering-Plough and Bristol-Myers Squibb, which market alternative drugs. Best known is Plavix, sold by Bristol-Myers Squibb and Sanofi-Aventis. With $5.9 billion in sales in 2005, it lags behind only the anti-cholesterol drug Lipitor. On the other side, some of the leading researchers protesting aspirin resistance have been funded by Bayer, the big aspirin maker (Armstrong, 2006).

Since the majority of the research involved has been carried out at universities, one may wonder whose interests they and their scientists represent. An instructive example is that of Dr. Daniel Simon, who was associate professor at the Harvard Medical School. Simon published an article in the trade journal *Physician's Weekly* in 2005 reporting that perhaps 30 percent of the 25 million people taking aspirin for heart problems were aspirin resistant. The article did not disclose that Simon had research funding from Accumetrics and Schering-Plough nor that he was a consultant and paid speaker for Schering-Plough. Instead, *Physician's Weekly*, which knew of the connection, said that their policy is not to disclose such potential conflicts of interest but to use the connection for things like securing advertisements to be placed next to the article from the sponsoring companies. There must be some comedy in this circle of interests, from manufacturer to researcher to publisher to manufacturer and back to researcher again, for rather than leading to professional censure it has led Dr. Simon to new studies of aspirin resistance funded by the same companies and to a new position at Case Western Reserve University. As for *Physician's Weekly*, their cynicism seems to be evenhanded. Dr. Charles Hennekins of the University of Miami School of Medicine, who had done basic research in the 1980s on the benefits of taking aspirin daily, objected in the journal in 2004 to the resistance scare, saying that "this undocumented phenomenon may have the negative consequence of reduced aspirin use." His connection to Bayer was not disclosed (Armstrong, 2006).

Such practices are not only a matter of the trade press but also show up in the most prestigious of journals. The *New England Journal of Medicine*, for example, got caught up in the scandal over Vioxx as a result of having published the report in 2000 that exaggerated its safety. The study was sponsored by Merck, the maker of the drug, and suppressed Merck's own evidence that Vioxx was more dangerous than its equally effective competitor, naproxen (Aleve), available over-the-counter at one-tenth the price. The journal rightly blamed Merck, but in 2001 it violated its own conflict of interest rules when it published a review article dismissing the dangers of Vioxx written by two authors with financial ties to Merck. The difficulty may be that academic journals are as dependent as academic researchers on commercial funding (Abramson, 2006).

The potential or actual distortions of research inherent in stories of this kind, which seem to have become epidemic in biomedicine if not yet in other less commercially lucrative areas of science, has sent universities, science publishers and federal agencies like the NIH scrambling for remedies. Before taking up remedies, however, I want to consider the stakes.

PUBLIC TRUST AND THREAT TO DEMOCRACY

The sagas of aspirin resistance and of the Vioxx report are examples of what Sheldon Krimsky has aptly called *Science in the Private Interest*. His worry is not just that this or that researcher produces distorted results, or even fraudulent claims, but that the entire system of biomedical research, especially as carried out at universities, may no longer be serving the public interest (Krimsky, 2003). If that is the case, I argue, or if it is widely perceived to be the case by the concerned public, then research for profit will make universities look increasingly like think tanks funded by private interests. This is precisely the ground on which politicization of science has become such a virulent problem since the early 1990s. If, in the aspirin resistance case, it were just a matter of Bristol-Myers Squibb competing with Bayer to gain market share, we would likely look at it as merely a matter of advertising claims, from which we might hope to extract some humor if not much trustworthy knowledge. But when the research of corporations is backed by the credentials of universities as servants of the public interest, then we have a different situation. Should the public put its trust in what is seen to be the corporate research of the University of Miami, Harvard, McMaster, or Case Western, as compared with laboratories funded by, say, the tobacco industry? Or is university science providing just the latest example of the "sound science" promoted by industry-based think tanks as compared with the "junk science" that used to be carried out at universities?

If the commercialization of academic science comes to have the character of science in the private interest, then it is the status of universities in the polity that we need to be concerned about, not merely the objective validity of some particular research report. Along this route lies politicization. Only let the aspirin question become one of proposed regulation and it will immediately become a candidate for politically motivated attempts to control the regulatory outcome by controlling the content of research results. The only thing that saves academic research from this fate—to the degree that it does escape—is its claim on science in the public interest.

An example of how the process of politicization through commercialization has been working of late can be seen in the role that Willie Soon, a Harvard-Smithsonian astrophysicist, and David Legates, a University of Delaware climate scientist, played in the contest over global warming. Global warming is of course one of the most egregious recent examples of political misrepresentation and distortion. Soon and Legates both did research supported directly or indirectly by the American Petroleum Institute, the George C. Marshall Institute, and/or Exxon-Mobil. They were called to present their findings against global warming to the Environment and Public Works Committee of the US

Senate. They came at the invitation of the committee chair, Republican Senator James Inhofe of Oklahoma. Inhofe is the man who once called the Environmental Protection Agency a "Gestapo bureaucracy" and global warming a "hoax." He aimed to use Soon and Legates to discredit the "junk science" carried on at universities and government laboratories that made global warming a practical certainty (Mooney, 2005: 86-7). The issue here is not so much whether Soon and Legates did valid research; it is that the commercialization of their university research brought them into the process of politicization just as if it had been the work of an ideological think tank, which it was in part. Commercialization of university research can make the distinction hard to draw. And if Inhofe could have made the claim stick that the weight of other academic climatologists represented special interests and uncertain science then he would have had a much easier time making Soon and Legates politically credible. In this case the tactics did not succeed and it seems that much of the public trust in academic climate research remains more or less intact, though the struggle has been hard fought and might have ended up otherwise.

The problem is a very deep one for our democratic society because universities play a key role in its social and political life. They are our primary institutions of trustworthy knowledge (Bok, 2003: 115-117). By trustworthy knowledge, I do not mean that it will always turn out to be correct, but that it is worthy of our trust because we believe that the people and institutions who produce it have made every attempt to ensure that their interpretations are valid in the current state of things. Such sources of knowledge are crucial to the effective functioning of both legislators and the voting public. Without those sources, decisions can only be made arbitrarily or politically, in the worst sense of the term: purely ideologically or out of self interest, or the interests of power, without any ground for judging what would best serve the public good. Collective, deliberative civic life depends on an informed public and informed legislators, whose knowledge is widely distributed. The historical and theoretical basis for this view of democracy as dependent on widely distributed knowledge is the subject of a new book by Josiah

Ober on classical Athenian democracy, aiming to show that "putting knowledge into action is the original source of democracy's strength . . . [and] remains our best hope for the future." Or as John Adams put it in 1765, "Liberty cannot be preserved without a general knowledge among the people, who have a right, from the frame of their nature, to knowledge. . . . The preservation of the means of knowledge among the lowest ranks, is of more importance to the public than all the property of all the rich men in the country" (Ober, 2005).

Granted, this is rather high-blown rhetoric. But the university as a source of trustworthy knowledge has a status in a democratic society similar to that of a free and independent press. Of course the press is never quite free and never quite independent but the ideal is extraordinarily valuable nevertheless.

PURE-APPLIED AND ACADEMIC-COMMERCIAL DISTINCTIONS

Public trust in the claims of science, and indeed the trust of other scientists, has long rested on the belief that, generally speaking, scientific results are objective. Indeed, nonobjective implies nonscientific. Objectivity in this sense does not refer to ultimate truth but to objective validity: other people doing similar work would get corroborating results. Interpretation of the results, furthermore, is aimed at providing the most plausible account of them in relation to other empirical and theoretical findings. The best guarantee for such trust in the objectivity of science has usually been thought to be the separation of the pursuit of truth from the pursuit of mammon. Like other teachers and scholars, scientists should not be motivated by personal gain or ideological interests. As Jacques Loeb of the Rockefeller Foundation put it early in the twentieth century, "if the institutions of pure science go into the handling of patents I am afraid pure science will be doomed," (quoted in Weiner, 1986: 35; Bok, 2003: 139).

In the United States, our intuitions about this standard view of disinterested science as the guarantee of objective science have been supported by two canonical texts, Henry Rowland's 1895 "Plea for Pure

Science" and Vannevar Bush's *Science—The Endless Frontier*, written in 1945. Writing in the midst of the "golden age" of American industrial growth, Rowland's plea rested on the belief that university research and education should answer to a higher moral purpose, that the search for truth epitomized by the sciences served to produce citizens with integrity and discipline. Implicitly, he was attacking the popular hero Thomas Edison, whose phonographs, telephones, and electric lights epitomized scientific accomplishment to much of the public. Such pursuit of profit, in Rowland's view, compromised the ideals of science (Rowland, 1902).

Vannevar Bush, writing at the end of World War II at the request of President Franklin D. Roosevelt, agreed about the values of pure science but articulated a way around worries like those of Rowland and Loeb. He represented the great contributions of science during the war as products flowing precisely from the distinction between basic research and applied research. The flow was a one-way stream, from the basic (pure) to the applied. Thus the question of whether profits would infect the source never arose. Bush could depict science as an endless frontier of progress and prosperity at the applied end without compromising the ideal of knowledge for its own sake from which all progress arose. Critical to this image was the institutional distinction between universities, on the one hand, as the location of basic research, and industry and the military, on the other hand, as the location of applications (Bush, 1990: 6-7, 1219-22). This understanding of the pure-applied and university-industrial distinctions has continued to supply the basic terms of discussion throughout the growth of federally funded research administered through the NSF, the NIH, and other agencies, until recently.

It may very well be that the canonical distinctions have helped to insulate academic research from the threat of compromise by material interests, thereby maintaining objectivity and the public trust. The question remains, however, whether the separation has been, or is, necessary for this purpose. Does pursuit of profit necessarily undermine pursuit of truth? And even more fundamentally, are the distinctions historically valid?

Historians of science have by now shown repeatedly that far from being the derivative products of research for its own sake, technological developments have just as often been the source of basic experimental and theoretical pursuits. A paradigmatic case is that of William Thomson (Lord Kelvin), the very image of science for the British professional and popular audience in the late nineteenth century and a founding theorist of modern energy physics. In each of the areas of his foundational work—electromagnetism, thermodynamics, mechanics, and the sought-after vortex atom—his theoretical perceptions depended critically on his deep engagement with concrete technologies, most notably the submarine telegraph, the steam engine, and the vortex turbine. His patenting and marketing of telegraphic instruments made him a wealthy man, as symbolized by his ocean-going, 126-ton schooner-yacht, the *Lalla Rookh* (Smith and Wise, 1989).

Another example is the great chemist Justus Liebig, whose pioneering work in rationalized agriculture and chemical fertilizers, as well as the production of meat extract and baking powder for the kitchen, accompanied his laboratory analysis of substances like superphosphate, his theoretical discoveries of radicals and isomers, and his monographs on organic chemistry (Schwedt, 2002). It is illuminating to recognize that even some of the most esoteric conceptual developments of modern theoretical physics have been rooted in part in quite practical concerns. Einstein's theory of special relativity, with its elegant analysis of the problem of simultaneity using moving railway cars and the exchange of light signals, was grounded in the problem of synchronizing clocks for railway networks and of the practice of exchange of telegraph signals (Galison, 2003).

These few examples serve to make two obvious but oft-forgotten points: the pursuit of truth and the pursuit of profit have often stood in a complementary relationship; and one of the most fertile sources of scientific creativity has always been engagement with technological practices. As a historical matter, then, the pure-applied and university-industry distinctions, especially on the Bush model of a one-way flow of knowledge from pure to applied and from university to industry, have never been valid (although the ideals embodied in the distinctions probably served an important purpose in elevating truth above profit in the scale of both academic and public values and certainly guided science policy for decades).[4] Recent developments within the sciences, furthermore, have made the distinctions increasingly untenable, even as ideals. Three interrelated developments are readily identifiable. First, the pure-applied distinction rested in part on maintaining the status of general and abstract theoretical physics, particularly elementary particle physics in the twentieth century, as the ideal of "fundamental" science. That ideal focused on finding high-level general laws—covering laws—that would explain (in the sense of derivation) all lower-lying and more specific phenomena. Second, this model of what science should be has lost much of its sway in the last 30 years, in part because it was artificially maintained by the prestige that it acquired from the atomic bomb project, which was subsequently maintained during the Cold War but rapidly declined after the collapse of the Soviet threat in 1989. Third, it has became increasingly apparent, even in physics, at least from the 1960s, that high-level theories of quantum mechanics, general relativity, and elementary particle physics offer little in the way of explanation in the world of everyday materials and processes that populate other areas of physics.

The argument has been made in an accessible manner by condensed matter physicist and Nobelist Robert Laughlin in *A Different Universe: Reinventing Physics from the Bottom Down*. In many other areas new ideals of scientific explanation have emerged: chemistry, geology, climate studies, genetics, and others (Laughlin, 2005; Laughlin and Pines, 2000; Wise, 2004). These are the areas where the sciences of complexity have grown up. Fourth, biology has replaced physics as the leading science of today. In none of these new areas does the ideal of covering laws have much purchase; on the contrary, they are highly dependent on technological mastery: computer simulations, model systems, polymerase chain reaction (PCR), microchip arrays, imaging technologies, and nano-engineering. The technologies provide tools to think with, tools that are all the more important in the absence of organizing laws (Creager et al., 2007). Here the distinction of pure and applied is very hard to make. Finally, it has become evident that what used to be called applied research is carried out also at universities and that formerly pure research is carried out also in industrial laboratories. The research as well as its locations has taken on a hybrid character.

In these circumstances, where it is widely recognized that "applications" are actually one of the most fruitful resources for creative science, there can be no question of preserving the purity of science by dreaming of the "endless frontier" in the form that Vannevar Bush projected. The frontier may be endless but if so it depends on cross-cultivation of academic and industrial science. One can only conclude that commercialization of university research is inevitable in a healthy scientific environment.

THE NEW LANDSCAPE OF SCIENCE

In his 1973 book on the best conditions for *Invention*, the mathematician Norbert Wiener, who had himself done exceedingly important work on feedback control of guns and other weapons during World War II and pioneered cybernetics, argued that treating ideas as property and introducing the profit motive of patent royalties into science instead of the pure love of discovery would "render sterile the soil of human intellect," (quoted in Bok, 2003: 140). I have argued that such predictions of doom are both historically and currently untenable. On the other hand, research for profit does threaten to undermine such traditional academic values as free exchange of information and materials. More fundamentally, it threatens to politicize academic research by putting it on a footing similar to that of the "sound science" of corporate-sponsored think tanks. If it spreads much more broadly than it has so far, it will severely erode public confidence in the trustworthiness of academic research. And with that it will compromise one of the pillars of our knowledge-based democratic society. Nothing in this scenario allows us the luxury of hoping that the problem of politicization will go away with the current administration, since its preconditions are in commercialization. So we need to reevaluate the landscape of current science and to develop better ideals for its organization.

To put a point on the problem, return once again to the aspirin controversy, in which the studies are nearly all funded by corporations that manufacture the tests and the drugs and by researchers with material interests in the outcomes of the studies. Thus John Eikelboom of McMaster University acknowledged that "there is a real issue of who you can get unbiased opinion from in medicine." He was an author of a 2002 study that first raised the specter of aspirin resistance and has consulted for companies on both sides. "It is a terrible problem. . . . I try to be honest with myself, but I can't pretend I will always be as honest as necessary." One might imagine that this problem could be avoided by attempting to exclude conflict of interest from academic research or by requiring disclosure in publications. But Daniel Simon put a sharper point on the issue, remarking that it would be a mistake to dismiss the views of researchers with conflicts of interest because industry is one of the main sources of medical progress, so that those without conflicts "are not truly expert" (Armstrong, 2006). Precisely so, if the current landscape is one populated by university/industry hybrids. It would seem that remedies for the problem of trustworthy knowledge have to come from within that landscape itself.

One attractive illustration for university/industry relations can be seen in Princeton University's award of its 2006 James Madison Medal, its highest honor for a graduate alumnus, to Arthur D. Levinson for his "success in bridging the worlds of science and business." Levinson earned his Ph.D. in biochemical science in 1977 and has gone on to become the CEO of Genentech, where he has developed collaborations with universities and cancer research institutions. Studies by the company are regularly cited in peer-reviewed journals at the same time as Genentech is included on *Fortune* magazine's list of the best companies to work for. Levinson has served on the editorial boards of several journals of molecular biology and virology, has been a leader in directing rigorous clinical trials, and is an author on over 80 scientific articles (see <www.gene.com>).

Levinson surely belongs on the dream team for university/industry collaborators. His activities also epitomize, however, the inbreeding of research, journal publication, clinical trials, patenting, and boardroom decision-making. We have to be asking if such inbreeding, in general, serves the public good, since these are precisely the sorts of relationships that have sometimes led to what Krimsky calls "science in the private interest": skewed research, monopolistic patenting and licensing practices, suppressed test results, and nondisclosure of conflicts of interest. They also yield fertile ground for politicization. Can the public trust survive the inbreeding? What controls ought to be in place to see that it does survive?

A second example of university/industry interrelationships may help to address these questions. It concerns the "Bio-Fab Group." This group consists of nine colleagues and friends who are contributing to biological engineering, or more specifically to the fabrication of biological systems from component parts—BioBricks—by analogy with the fabrication of semiconductor chips. They aim for a vertically integrated production system, building up from the specification of DNA sequences with particular characteristics at the bottom; to the manufacture of biological parts that realize the desired characteristics; to the assembly of these parts into devices (like inverters and switches); and up to more complex systems (like transistors and circuits). Circuitry is only one example. They are also working on the manufacture of compounds that would be effective in preventing the spread of malaria and HIV and they envisage novel proteins for gene therapy and energy production (Baker et al., 2006).

In terms of new constellations of science, the fab group is interesting in several respects. First, members of the group come from universities throughout the country—the University of Washington, Harvard Medical School, Boston University, MIT, Berkeley, Duke, Cal Tech, and Princeton—suggesting the distributed character of collaborative research in the age of the Internet. Second, as a highly multidisciplinary group, representing a diverse range of expertise in molecular biology, computational biology, and biological engineering—required for the vertical integration of the fabrication process that they envisage—they completely scramble the pure-applied distinction. Third, they are all directly involved as founders or scientific advisers with commercial companies aiming to market fabricated biological systems, vitiating the university/industry dichotomy. Fourth, they (and the larger synthetic biology community) are deeply concerned with developing ethical codes and regulatory agreements to address ecological and criminal risks associated with devices that could replicate and evolve and with ensuring biological justice (see "Public Draft, n.d.). And fifth, one of them has spearheaded the organization of a nonprofit foundation, the BioBricks Foundation, that seeks to maintain open public access to a library of BioBricks to encourage codes of standard practice, and to provide professional and public education.

These last two elements of the synthetic biology enterprise begin to look like the institutional forms springing up from within the marriage of university and industry that could seriously cope with the problem of commercialized science undermining public trust in academic research. It seems crucial that their ethical and regulatory concerns have emerged in a self-reflexive way from their own attempts to preserve the best interests of research, industrial production, and the public interest simultaneously. It is also crucial that, in its attempt to exercise an oversight role, the BioBricks Foundation has remained a nonprofit organization, although founded by engineers and scientists who are directly involved in commercial biotechnology research. Just how and whether such an oversight organization will be able to maintain an independent critical judgment remains a large question.

Finally, and turning to a more traditional form of oversight in the public interest, it should be perfectly clear by now that dismantling the Office of Technology Assessment was a very bad idea indeed. It should be rejuvenated with a director named in as apolitical a manner as possible (perhaps by the National Academy of Sciences) and generously funded to carry on the public's work.

NOTES

1. For their very helpful suggestions I thank organizer Tiago Saraiva and commentators Manuel Villaverde Cabral and João Caraça at a HoST

Lecture at the Institute of Social Sciences, University of Lisbon; the participants in a colloquium of the Institut für Wissenschafts und Technikforschung, University of Bielefeld; and Elaine Wise, Lorraine Daston, Sally Gibbons, Theodore Porter, Suman Seth, and Otto Sibum.

2. The web version is available at <http://webexhibits.org/bush/index.html>.
3. The Social Science Initiative can be found at <http://www.ucsusa.org/global_warming/sound-science-initiative.html>.
4. The papers collected in Grandin et al. (2004) provide a thorough critique of the pure-to-applied and university-to-industry model, which they call the "linear model." See also Johnson (2004).

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