SCIENCE BOUGHT AND SOLD

Essays in the Economics of Science

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Introduction

Philip Mirowski and Esther-Mirjam Sent

It is a commonplace observation that economists love the Individual; it is just real people that they cannot be bothered about. A wag once added that economists also profess to love Science; it is just real scientists that make them nervous. Their penchant for stark abstractions, gilded icons, and representative agents in a world of messy particulars and intractable diversity is the hallmark of fin de siècle economists, one they like to think they share with their neighbors, the natural scientists.

We think that many natural scientists, on the contrary, more often than not tend to harbor an aversion for economists, although some of their best friends might just happen to be card-carrying practitioners of the dismal science. If they have ever bothered to look into what economists do, they have often come away with reactions ranging from boredom to distaste for what impresses them as an insufficiently scientific inquiry. For precisely that reason, the very idea of an "economics of science" has frequently evoked vertigo and worse among scientists. Deep down, all scientists understand that at some fundamental level, there is some sort of economic process or processes channeling and fortifying their science; it is indisputable that someone, for some reason, has been picking up the tab. Yet the tendency until recently has been to deny that process has any substantive bearing on the real activity of scientific research. We suspect that this rather pervasive disdain may be eroding under the pressure of recent events, and therefore the random scientist may just pick up this book in an effort to clarify her own thinking on these issues. We aim to please; and nothing would please us more than to have natural scientists venture beyond stereotypic philippics about the utter folly of not funding their own favorite research agenda to the hilt.

Of course, we also have other potential audiences in mind. As will be inferred from the roster of authors in our collection, we are inclined to pay as much attention to the communities of science studies scholars and specialists in the history and philosophy of science as we are to economists and to the scientists themselves. There too, we feel that there is room for improvement. In our experience, there has persisted a strangely antihistorical approach to the study of the economics of science among those interested in science policy. In the immediate postwar period, both science and the economy were treated by commentators as timeless generic entities, with overarching "norms of science" supposed uniformly to exercise their regulative sway over scientists from the seventeenth to the twentieth centuries. Both historians and sociologists of science have protested against such invariant norms from the 1960s onward; nevertheless, whereas the processes of scientific research have been openly acknowledged to undergo temporal change and cultural variation in the interim, somehow the economics behind the science was imagined as mired in stasis. Even in sophisticated work on changing structures of scientific organization, such as that by Joseph Ben-David (1971), a species of timeless market was held to underlie the changing parade of scientific institutions. We believe that one novel contribution of this volume will be to insist upon the fact that the *economics* has been changing in noteworthy ways in tandem with the sciences.[1] Indeed, the reason we have produced an anthology of approaches to the economics of science is to demonstrate that it would be a serious error to appeal to a monolithic dogma called "*the* economics of science," either in history or in modern science policy.

And then there is our conventionally obvious audience. A contemporary economist who might chance upon this volume will undoubtedly be looking for meditations upon one of his favorite themes, namely the significance of Science for the abstract Representative Individual and, conversely, the centrality of this Individual in the progress of Science. Be assured once more, dear reader, that we aim to please: in the following pages one will find many nameless rational actors plying their trade, and a fair amount in the way of statistics of aggregate scientific agents producing and distributing their wares. Nevertheless, one of the primary reasons we undertook to put together this volume is that we believe (and we are not alone) that something rather drastic and profound has been happening to the social organization of science in America and Europe at the end of this century, if not throughout the world; and that an inordinate fascination with a timeless representative scientific agent serves mainly to distract attention from these changes; and therefore a serious reconsideration of the "economics of science" is long overdue. (In saying this, we do *not* think the standard trope of triumphantly announcing the immanent arrival of a "new economics of science" is anywhere near an adequate response.) We will approach this claim from many angles in this volume, but the best way we can think of to embark upon our journey is to render this thesis as palpable and immediate as we know how—that is, to tell a story.

1. We intend this to refer both to the school or theoretical tradition that is accessed in order to explain the provisioning and organization of science, as well as the social structures and institutions within which scientific careers are embedded. In this regard, the ambiguity present in the phrase "economics of science" is appropriate, for when it comes to science, the conditions of its support cannot be readily separated from what we think the set of institutions is *for*. To those aware of our individual work in the history of modern economics (Mirowski 1989, forthcoming; Sent 1998) and the history of science our insistence upon the interplay and joint historicity of economics and the natural sciences should come as no surprise.

1. The Unfortunate Case of Petr Taborsky

The quintessential American success story starts off, just as this one, with an immigrant coming to America, the land of opportunity and freedom, partaking of the benefits of our educational system, and then turning her natural abilities to innovation and hard work in pursuit of a better life. The tale of Petr Taborsky, born in Prague in 1962, begins in just that way.[2] Taborsky's family fled the events of 1968 in Czechoslovakia for the United States. Taborsky was a precocious child and an inveterate inventor, filing for his first patent at age sixteen. In the fall of 1986, Taborsky enrolled as an undergraduate at the University of South Florida (USF), pursuing a double major in biology and chemistry; needing financial aid, he began working as a part-time student lab assistant in a civil engineering laboratory at the USF run by Dr. Robert Carnahan.[3] USF, although a public university, like many others strongly en-

2. The sources for this story are Sanchez 1996; a series of reports in the USF student newspaper *The Oracle* found at http://www.oracle.usf.edu/archive; Jaroff 1997; the AP wire service story dated June 18, 1996, entitled "Ex-Student Sent to Chain Gang"; and the web site of the Student Coalition for Handling Intellectual Property, http://www.ij.net/S-Chip/petr.

3. The shifting importance of foreign-born students in American science at the end of the century is just one of the many aspects of this story that renders it representative. In 1995, 23 percent of American Ph.D.s in the sciences and engineering were foreign born, although the proportion was much higher in certain fields like civil engineering, where it was 50 percent. See National Research Board, *Science and Engineering Indicators 1998*, 3–19. All subsequent citations to this source will assume the format *S&EI [YEAR]*.

courages its faculty to attract and conduct research sponsored by corporations and other external private entities. Not only does this augment basic state funding for university activities, but attracting such grants also enhances the reputation and status of the university in the wider world. USF had found itself impelled over the years to develop various institutional structures in order to negotiate the public/private interface, not to mention the reconciliation of the obligations of the faculty to their university with their obligations to their sponsors and grantors. For instance, USF had established a Division of Sponsored Research that supervised roughly fifty million dollars in grants in the academic year 1988–89 and was charged with overseeing the assignment of property rights arising from such research. However, as at so many other universities in this period, USF had to improvise many procedures and policies toward this innovative sponsored research as it went along, in part due to lack of prior experience with this kind of research funding, partly because of the prior legal and commercial expertise of the corporate funders, and in part simply to keep up with the more irrepressible entrepreneurial innovations of some of its more active faculty members.

At first, Carnahan was impressed with Taborsky's abilities and encouraged him to pursue a master's degree in civil engineering, which Taborsky embarked upon in August 1987. Meanwhile, Carnahan and USF managed to land a small portion of large study of wastewater treatment procedures offered by a 1986 consortium of water and power utilities, the immediately relevant participant being Florida Progress Corporation. Florida Progress awarded a three-month $20,000 contract to Carnahan at USF to determine the capacity of some bacteria to clean calcium deposits from a granular clay called clinoptilolite for reuse in wastewater treatment. Carnahan assigned Taborsky to this research account to perform the laboratory tests. At the end of the three months the testing was completed, and therefore Carnahan removed Taborsky from the account and wrote up a project report. At the termination of the grant, Taborsky embarked upon his master's project on the physical properties of the aluminosilicates (resembling commonplace kitty litter), separate from any question of bacterial action on the clay. In this interval, Florida Progress expressed no interest in funding further research, although Taborsky did perform tasks for Carnahan, primarily laboratory assistance at $8.50 per hour, on other accounts. Taborsky summarized the findings of his own project for Carnahan in a May 5, 1988, report, in preparation for completion of his degree.

In July 1988, while working on his own ideas in the lab, Taborsky

discovered that heating aluminosilicates like clinoptilolite at temperatures of to 850°C would vastly improve the abilities of the clay to be reused in treating wastewater. Since conventional wisdom was that heating this clay above 600 degrees would destroy it, no one had looked into the effect of extreme heat on its ability to reject calcium deposits and absorb ammonium ions. Previous work on the bacteria project had prompted Taborsky to realize that this process would be potentially valuable to Florida Progress and other such water utilities. He discussed this with Carnahan, who informed Taborsky that he could not expect to recoup any benefits from this discovery, which Carnahan suggested would belong to USF. Carnahan also advised that Taborsky could be prosecuted and incarcerated if he attempted to file a patent on the discovery or attempted to publish the result. The blow was softened, however, by the further suggestion that Florida Progress might hire Taborsky as an engineer.

Taborsky met with representatives of Florida Progress in September 1988 and described his thesis work. In December, Florida Progress offered Taborsky a job, which Taborsky declined, believing (rightly or wrongly) that all Florida Progress wanted was legal control over the discovery. However, as part of the job application, Taborsky did sign what he was told was a routine confidentiality agreement. On January 6, 1989, Taborsky filed a patent application on his discovery, after leaving USF and resigning from his lab job, without taking his finals or in any other way wrapping up his obligations there; but he did abscond with the lab notebooks in which he had first described his discovery. Carnahan, finding the notebooks missing, tried to get hold of Taborsky by phone, leaving a message on his answering machine threatening him with jail if he did not return the notebooks. Carnahan then reported the theft to university police, charging that Taborsky had filched at least thirty-two trade secrets from himself, USF, and Florida Progress. On September 27, Carnahan and Florida Progress filed a competing patent on the aluminosilicate process.

Up to this point, the reader might suspect that, however unfortunate the details and ferocious the recriminations, such patent disputes are nothing especially novel or noteworthy in the rough-and-tumble world of corporate research and development and that inventions are especially prone to legal contests over ownership. Combine this with poor interpersonal dynamics and one principal who was dead-set in his fervently held conviction of moral superiority, and regrettable conflicts will inevitably ensue. However, such offhand impressions would not begin to appreciate the special characteristics of this case, nor focus upon attributes that bear important lessons for contemporary economics of science. This incident constitutes a veritable witches' brew of all the ingredients that make up the "new regime" of science funding and organization in America at the beginning of the twenty-first century. Into the pot were tossed vulnerable foreign students, warped career paths, visions of virtual riches (Wilson 2000), ill-prepared university administrators, vague case law, intangible intellectual property rights, reengineered corporations bent on capturing competitive advantage, and self-seeking faculty entrepreneurs in a fragmenting university, not to mention the ersatz kitty litter. The older Mertonian ethos of noble, disinterested "communistic" science had no purchase here; but even simplistic notions of "exploitation" would equally lead us astray.

The most striking aspect of subsequent events was the intransigence with which USF sought to assert what it (or, at minimum, its research administrators) conceived as its prerogatives in this case. Note well Taborsky's particular vulnerabilities: a student with a paucity of standard intellectual credentials, occupying an underlaboring role in the laboratory, and an immigrant whose pending application for citizenship could itself be put at risk. Recall further that Taborsky had never signed any intellectual property agreements with the university, but only with Florida Progress. USF, far from seeing itself as sharing interests with one of its own students, followed the inclination of its vengeful faculty member in prosecuting Taborsky to the utmost. Not only did USF file criminal charges, but it also sought permission from its regents to file a civil suit as well. In a downward spiral of recrimination, Taborsky lodged a countersuit, alleging conspiracy, violation of civil rights, and racketeering. There is some suggestion that USF pursued the criminal case because it might be used to block award of the patent to Taborsky. In January 1990, Taborsky was convicted of grand theft and theft of trade secrets, sentenced to fifteen years' probation, and ordered to turn over all research materials to USF. Former USF president Francis Borkowski wrote a letter to the judge in the case urging a prison sentence for Taborsky, asserting that he was "beyond rehabilitation," and more ominously, that his actions had threatened the good relationship that had been fostered between USF and its corporate sponsors.

From Taborsky's vantage point, USF had gone well beyond the pale in seeking to prosecute him in this matter. For instance, Taborsky alleged that some administrators falsely maintained that he had signed a confidentiality agreement with USF prior to the Florida Progress project, that Carnahan had backdated his own confidentiality agreement with Florida Progress, and that the jury in the criminal trial was denied access to USF's own "Property and Procedures Manual." Whatever the truth of these allegations, on January 24, 1991, the United States Patent Office denied the Carnahan application and awarded patent 5082813 to Taborsky for the aluminosilicate process; he was subsequently awarded two further patents. Regarding this ruling as personal vindication, Taborsky refused to turn over his notebooks to Carnahan and USF, stating that they would deny him access and use of his own notes; he regarded the order to sign his patent over to USF by a district court judge a travesty. The USF responded by taking him to court again for violation of his parole agreement; in 1992 he was sentenced to three and a half years in prison for the infringement. Exhausting all appeal procedures, he was incarcerated in 1993, including an eight-week interlude on a chain gang; it was at this juncture that the press got hold of the story.

Reading about the case in a local newspaper, the Florida corrections secretary got him removed from the chain gang; then the governor got into the act and offered a full pardon. By this time, Taborsky, in full righteous dudgeon, flat out refused the pardon, on the grounds that accepting a pardon would be an admission of guilt. As Taborsky said in his radio interview: "I'm seeking justice and seeking the truth. What actually happened? What were the contracts for? Where did the money go? And those sorts of things. Those things will vindicate me. They will show that I did nothing wrong."[4] Taborsky's own story is not over yet. He was released from prison after serving eighteen months of his sentence and managed to get a judge to terminate his probation. He has filed an appeal to overturn his original conviction, and the civil suit has yet to be settled. Taborsky, on the warpath, says he will settle for nothing less than complete and total vindication.

Was this merely an iconic "failure to communicate" after the manner of Cool Hand Luke, or was it something larger? One fears that the cinematic aspects of "working on the chain gang" account for much of the publicity that brought this case to public consciousness (however fleeting); but far from being an aberration, troublesome cases such as this one have become a fact of life in modern science. While few have gone to the lengths of squandering vast resources on legal fees for a criminal case as did USF, most modern research universities now find they must take a certain level of civil prosecution over intellectual property rights

4. Sanchez 1996.

in stride. For instance, the University of Michigan paid $1.67 million in damages to a scientist who maintained that her work had been stolen by a superior; and that was in the field of psychology, and not a really high-stakes research area like biotechnology or computer technologies.[5] Some faculty have taken to suing their own universities for shortchanging them on royalty agreements, as the case of Jerome Singer and Lawrence Crooks in their dispute with the University of California over magnetic resonance imaging. Most large research universities now find a persistent nagging parade of problems of science regulation, construed broadly to encompass not just property disputes but also "fraud," negotiation of contractual agreements with industry and the federal government, and the monitoring of human subjects and hazardous materials research, not to mention the portmanteau category of "research ethics" sufficient to justify the maintenance of a full-time "Office of Scientific Integrity"—although terminology has not yet been stabilized in this area, so the bureau might also travel under the more colorless but upbeat designation of "Office of Technology Transfer." And as good economists, our thoughts immediately turn to ways to quantify the costs and benefits of this endeavor. Interestingly enough, although individual universities do keep fairly close tabs on the costs of various breakdowns of their own operation of the scientific enterprise, they are rarely (if ever) shared between institutions, much less rendered the subject of academic study. One observer has made a very impressionistic back-of-the-envelope calculation that quotidian regulatory problems of science at the university level absorb something like 2 percent of the total research budget, although worst-case scenarios such as the aforementioned Michigan dispute might cause that figure to go higher.[6] Another source estimates that "dozens of major universities—Brandeis, West Virginia, Tufts, and Miami among them—actually spent more on legal fees in fiscal year 1997 than they earned from all licensing and patent activity that year" (Press and Washburn 2000, 48).

It is our impression that this novel administrative arm of the university, combined with incidents resembling the Taborsky debacle that have

5. See Philip Hilts, "University Forced to Pay $1.6 Million to Researcher," *New York Times*, August 10, 1997, A-13. For other such cases, see Hilts, "Jury Award Voided in Scientific Research Case," *New York Times*, February 2, 1997, A-17. Indeed, the corporate penetration of university research in molecular biology has proceeded to such an extent that it is getting to the point that such disputes are treated as newsworthy only to the extent that the acrimony spills outside the cloistered halls of the university and into a more public arena: see, for instance, Horton 1999.

6. Wright 1997.

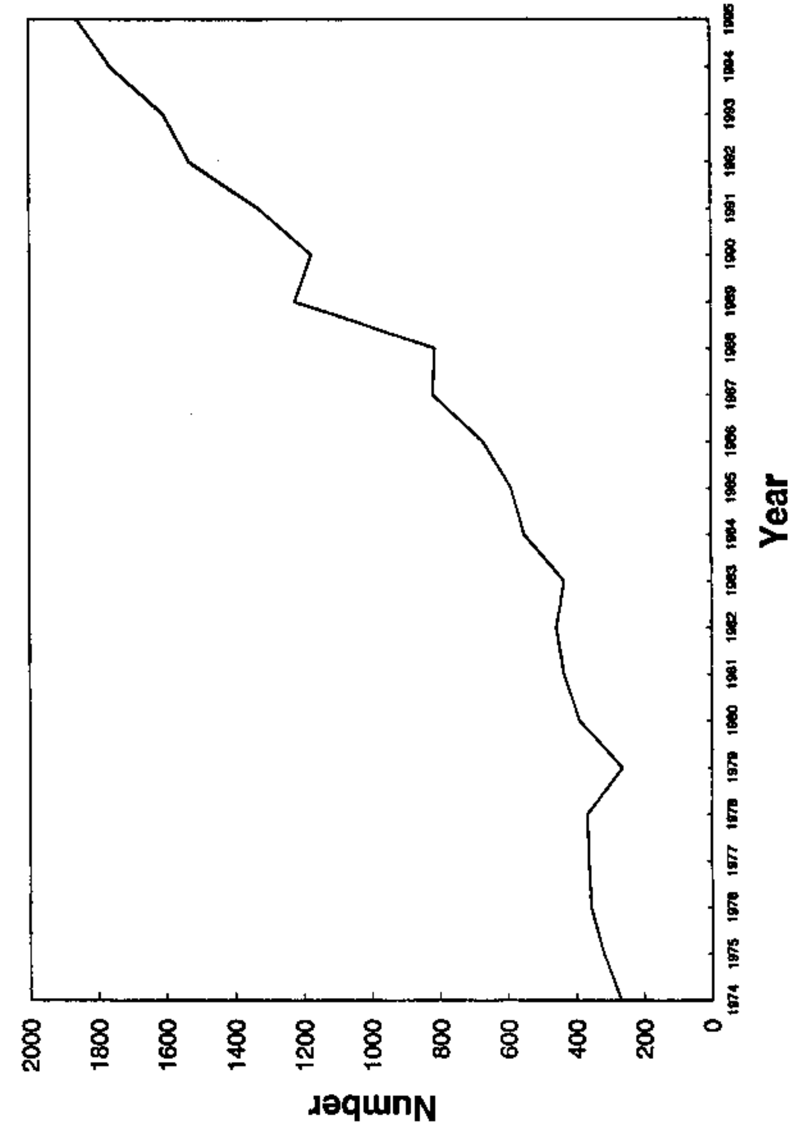

FIGURE I.1. Number of Patents Granted to U.S. Academic Institutions
SOURCE: *S&EI 1996*, appendix pp. 249–51 (table 5-41) for 1974–1991 and *S&EI 1998*, pp. A-337–A-343 (table 5-57) for 1992–1995.

summoned it forth, are themselves symptomatic of much larger changes happening within the structure of the scientific enterprise, and as such, should be of more direct concern to a suitably contemporary economics of science. But that would necessitate viewing the Taborsky incident as something more than an unfortunate fluke or a local oddity, as a bellwether instead of an aberration. It is part and parcel of the phenomenon illustrated in figure I.1, where universities have been reengineered in the recent past to become active producers of patented knowledge.

What is sorely needed for a better understanding of this incident is some historical background in order to gauge the extent to which the relevant economic structures of scientific research have indeed changed dramatically over the last century, which includes the extent to which universities have ceased to serve as repositories for wisdom and have become profit centers for the generation of intellectual property.

2. Scientists Who Came out of the Cold (War): An American Tale

The lesson that things change over time is hardly novel or earthshaking; rather, the proposition that there are some discernible regularities to perceived change is the starting point of all analysis. Science has indeed undergone profound transformations in organization and content over the course of the twentieth century; and yet, the only sort of change that many commentators on the health and well-being of Science deign to recognize is that there has been but more of the same. In other words, in this simple but widespread view, progressively larger phalanxes of scientists produce progressively more "knowledge" that is poured into a vast communal storage tank of information, available to be tapped into by anyone willing to invest in the training required to successfully drink from the wellsprings. Of course, from this perspective, bigness itself can create social problems—problems of coordination, problems of organization, and if science grows "too fast," problems of funding and support. These problems, it has been suggested, exist merely as the unfortunate by-product of the demonstrated success of science and as such have nothing whatsoever to do with the nature of the knowledge produced, the culture that produced it, or the nature of the understanding of science in its multiple manifestations. Partisans of this account favor locutions like "If it ain't broke, don't fix it." One observes these inclinations in what has been called the "public understanding of science" movement, where "the public" is repeatedly quizzed and badgered on its ability to recognize correct scientific propositions and willingness to report favorable attitudes toward science in general.[7] These inclinations were also popularized by one of the pioneers of "scientometrics," Derek de Solla Price, and have been elaborated more recently by John Ziman (1994). In this approach, many of the seemingly dysfunctional attributes of the prosecution of modern scientific research are attributed to the sheer magnitude of the modern scientific enterprise. Big Science, it is claimed, must eventually run into diminishing returns, if only because it has grown so much more rapidly than the underlying population base, economic base, or even the cognitive capacity of any individual to comprehend its achievements. "Nowadays planning a new research program is much more like . . . setting up a factory to manufacture a new product line" (Ziman 1994, 47). But mass production and bureaucratic rationality are said to be the unfortunate price that must be paid for ratcheting up the scale of scientific research; paraphrasing Joseph Schumpeter: it is ham-fisted and soulless, but it delivers the goods.

However commonsensical such quasi-economic language of scale economies might initially appear to those with social science backgrounds, we must beg to differ that such an approach is ahistorical, mechanistic in the extreme, entangled in problems concerning the appropriate specification of production functions for science, and therefore fundamentally misleading. Although it would seem the height of consistency to apply a species of autonomic technological determinism to science itself, we argue that it contributes little to an understanding of the present predicament of science, and much less to specific cases of severe malfunction, such as the Taborsky incident. In its place, we seek to suggest an analytical classification of structures of science organization in the twentieth century that is more intentionally connected to the relevant geographic, historical, and economic contexts; one that does not trade in a passive fatalism and instead serves to direct our attention to more finely detailed issues concerning specifics of social organization in the economics of science. What this implies is that whereas it is undeniable that the content of science aims to transcend all geographical and temporal bounds, the actual prosecution of scientific research cannot seriously aspire to any such transcendence. Accepting that premise for the moment, principles of selection must be invoked in order to restrict analysis to a limited set of structures of social organization of scientific

7. See, for instance, S&EI [1998], chap. 7. There we learn, among other interesting tidbits, that Japanese citizens tend to report very high anxiety over the negative effects of science and technology, at much higher levels than American citizens.

inquiry. In this volume, we are concerned with what have been predominantly "American" theories of the economics of science—that is, postwar neoclassical theories—and that consequently dictates some brief familiarity with the history of American regimes of science funding as a prerequisite. We want to stress that this will not ultimately preclude consideration of science funding and organization in other countries—rather, exploration of the demonstrably parochial character of unfounded assertions concerning the existence of a universal "economics of science" (a characteristic American vice) must take precedence, before one explores alternative national idioms of the organization of research.

In brief, based upon a cross-section of recent work in the history of science and science policy, we would like to propose that there have existed three very distinct regimes of science organization and funding in the United States in the twentieth century, and that each regime has borne a special relationship to the contemporary organization of science in other countries.[8] While they may overlap in certain particulars, we will place the temporal divisions of these regimes at the following boundaries: early twentieth century to 1940; from World War II through the Cold War; and (roughly) 1980 to the present. For convenience, we will call them the (1) protoindustrial regime, (2) the Cold War regime, and (3) the globalized privatization regime. Our object in describing these regimes is not to provide a full-fledged history of science organization in the United States—those can better be found in our references—but rather to set the stage for our larger argument, namely, that each regime comprised a distinct set of structures that have in practice summoned quite differing versions of an "economics of science" to justify and account for their regularities. As promised, we regard funding structures and theoretical accounts of their efficacy as inextricably interlinked. The stubborn quarantine of science policy discussions from the history and sociology of the organization of science must cease if there is to be a sensible economics of science. One of the aims of this volume, and of

8. The description of these regimes is distilled out of a large literature, some highlights of which are Kohler 1991, Reich 1985, Stine 1986, Sarewitz 1996, Hart 1998, Brooks 1996, Kleinman 1995, Gruber 1995, Mowery and Rosenberg 1998, Noll 1998, Guston and Kenniston 1994, Reingold 1995, Gibbons et al. 1994, Ziman 1994, Kline 1995, and Branscomb, Kodama, and Florida 1999. For reasons of space, we must curtail any elaborate historical description of the first two regimes in this volume. The evidence concerning the specific shape of the third of our three regimes is summarized in this volume in the article by Slaughter and Rhoades (chapter 1) and the pieces contained in our section on the "contours of the globalized privatization."

this introduction then is to situate some of what are generally conceded to be the classic texts representing these alternative approaches within the various American regimes of science organization and funding.

2.1 The Protoindustrial Regime

The first, protoindustrial regime of science funding and organization dates from an era when American science was widely regarded as inferior in many respects to European science and when a few American universities were entering their initial phase as incubators of scientific research. In this era, most colleges and universities existed almost exclusively to perform the service of education and the propagation of the liberal arts, based upon Scots and British models of liberal education.[9] As our label intimates, most scientific research and development in this period was to be found in a few large American corporations. The reasons that some large corporations such as General Electric, DuPont, American Telephone and Telegraph, and Eastman Kodak fostered in-house research capacity had much more to do with the need for routine testing capacities and with the fin de siècle merger wave and American antitrust and patent policies than with any belief in the necessity of innovation or the commercial value of science, as is now widely acknowledged in the historical literature.[10] America lagged behind European practice only by a few decades, since the modern literature dates the inception of corporate research labs for the purpose of product innovation in Europe to roughly the 1880s (Fox and Guagnini 1998–99, 215, 251). In the public sphere, the U.S. federal government role in supporting research was comparatively small and consisted primarily of promotion of agricultural research through a network of agricultural extension stations, or of subsidizing specialized research in government-run labs

9. Our characterization of this situation as "protoindustrial" is supported by one of the earliest texts in the economics of science, Thorstein Veblen's *Higher Learning in America* (1918). There Veblen complains that universities are increasingly being run according to business principles; and although he casts this in invidious comparison to the Germanic model, in fact it paralleled the organization of science in American corporate and government laboratories at that time. For an instructive instance of the prehistory of protoindustrial science in the United States before the great wave of consolidation, see Lucier 1995; for an overview of the protoindustrial landscape, see Carlson in Krige and Pestre 1997 and Hounshell in Rosenbloom and Spencer 1996. The European history of protoindustrial laboratories is comprehensively surveyed in Fox and Guagnini 1998–99.

10. "Industrial research laboratories were first established in the U.S. primarily to protect large corporations from competition" (Reich 1985, 239). On the uses of research labs to ward off antitrust prosecution and foreign competition, see Reich 1985, Mowery and Rosenberg 1998, and Dennis 1987.

tied to motives of nation building. Examples of the latter would include the Coast Survey (which employed the earliest author in this anthology, Charles Sanders Peirce), the U.S. Geological Survey, the Bureau of Chemistry of the Agriculture Department, and the National Bureau of Standards. In many respects, American policy reformers sought (with indifferent success) to mimic science policies innovated in Germany, especially in the promotion of state-funded higher education combined with state-run research institutes, and with good reason, since German science was thought to be the best in the world (Lenoir 1998). What the German system had started under the aegis of the Humboldtian reforms was a closer integration of (graduate) teaching and research, which extended to the institution of a laboratory-based pedagogy (Fox and Guagnini, 1998–99). American students seeking advanced academic training were therefore urged to spend time in German universities, in the absence of suitable American infrastructure. The professionalization of academic disciplines was then in its earliest stages; and in most fields, a career consisting primarily of research was simply not a viable option. Unless one worked for a federal or a corporate lab, the life of an American scientist was a hard one. Only toward the very end of the period did a handful of private foundations (such as Rockefeller and Carnegie; see Kohler 1991) begin to innovate new forms of scientific patronage, aimed at building up a few selected universities as research institutions and revising the previous construction of the research grant as a temporary dole parceled out as charity to poverty-stricken academics. Thus both corporate science and the nascent structure of academic careers in America were almost entirely defined by the captains of industry and their managers.

As noted by Reingold (1991), Americans had great trouble coming to terms with the nascent idea of public funding for a scientific elite. By the 1920s, there arose a substantial cultural trend that regarded industrial concentration and technological advancement as two sides of the same coin, a dynamic resulting in technological displacement, and in the 1930s, even widespread unemployment. Moreover, Continental Europeans (with the British emphatically excluded) tended to treat their scientists as having a status patterned upon their previous aristocracies and thus took for granted their role within the state; but this option was foreclosed in the American context. Suspicion of elites tended to shade over into skepticism over the very premise that there should exist a cadre of researchers who would do their thinking for the benefit of the larger

populace. This was captured by some comments in the *New York Times* of 1885:

> Like other men [scientists] are self-seeking, ambitious, and have their personal ends to gain. Can we assume that they are morally any better than their neighbors; or that, if they get possession of place and power, they will not use and pervert them to the promotion of their selfish objects? (quoted in Kevles 1995, 54)

Hence, we should note that there is nothing particularly novel or radical about the American penchant for regarding the scientist as a rational self-interested agent; nor is the notion of a market-driven science especially innovative or pathbreaking.[11] Indeed, it constituted the intellectual underpinnings for the bulk of science support in America at the turn of the last century. The point we wish to stress is that the science supported under this regime was relatively modest and rarely attained world-class status. Innovative American scientists tended to be autodidacts and loners (Peirce himself serving as an extreme example) even if they managed to ascend to a university position, and many did not. It took a dramatic change of regime to propel American science to the front ranks of world science.

Because this spotty situation barely qualified as a "system," it should perhaps come as no surprise that there was very little literature dating from this era that could qualify as propounding a self-conscious "economics of science"; and with the exception of the short article by Peirce, we have found nothing suitably relevant to include in this volume.[12]

2.2 The Cold War Regime

It is now almost universally acknowledged that World War II stands as the watershed of American science and that the system of science funding and management in the United States propelled it to world

11. Here we take issue with a strain within the science policy community that attempts to portray a single Mode 1/Mode 2 watershed in the organization of modern science between communalist and corporate science. The locus classicus of this theme is Gibbons et al. 1994.

12. The only candidates would have been the aforementioned *Higher Learning in America* (1918) by Veblen, and perhaps some Marxist writing on the relationship between base and superstructure within the context of some version of historical materialism. Because what little funding there was emanated from the trust-dominated industrial sphere, most of this literature sported a certain muckraking attitude.

dominance for the second half of the century.[13] This is not to say that the feat was accomplished consciously and intentionally; nor is it to ignore the fact that the Depression and Nazi-era disruption conveniently destroyed the hitherto-dominant German university system and drove a generation of stellar scientific talent to seek asylum in the United States. For obvious reasons, this is not the place to rehearse the familiar narrative of how World War II became known as the "physicist's war" and how the postwar politics of the bomb and the Cold War locked America into a military-dominated system of science funding.[14] Rather, we merely wish to indicate the ways in which the various components of the Cold War regime that were forged in the fires of World War II tended to fit together, and the ways in which this system was virtually unprecedented among the other economically developed nations, at least until the 1980s. Our purpose, to reiterate, is to set the stage for the conditions giving rise to a nascent analytical discussion of the "economics of science" in the United States in this period.

The centerpiece of this regime was the massive federal presence in science planning and funding. The dominance can be demonstrated by means of many quantitative measures, showing federal expansion from the immediate postwar period to the mid-1960s, and then subsequent contraction, as illustrated in figure I.2 below.

But it was also a structural dominance, with the wartime practice (innovated at the Office of Scientific Research and Development [OSRD]) of research support in the format of government "contracts" being granted through universities and industrial firms on a "nonprofit" but fully reimbursed basis, strengthening both of these institutions by using them to channel support to individual scientists, rather than simply hiring the scientists as civil service employees of a federal laboratory system. This infusion of cash jump-started the cultivation of Big Science on a scale previously unimaginable, with massive instrumentation and hierarchical teams of interdisciplinary scientists and engineers roughly patterned upon the successes of the MIT Radiation Laboratory and the

13. "The postwar R&D system, with its large well-funded research universities and Federal contracts with industry, had little or no precedent in the pre-1940 era and contrasted with the structure of research systems of other postwar industrial economies. In a very real sense, the US developed a postwar R&D system that was internationally unique" (Mowery and Rosenberg 1998, 12).

14. On this history, see Kevles 1995, Kevles in Galison and Hevly 1992, Rhodes 1986, Reingold 1995, Leslie 1993, Morin 1993, Hacker 1993, Brooks 1996, Lowen 1997, Mowery and Rosenberg 1998, Kleinman 1995, Hart 1998a.

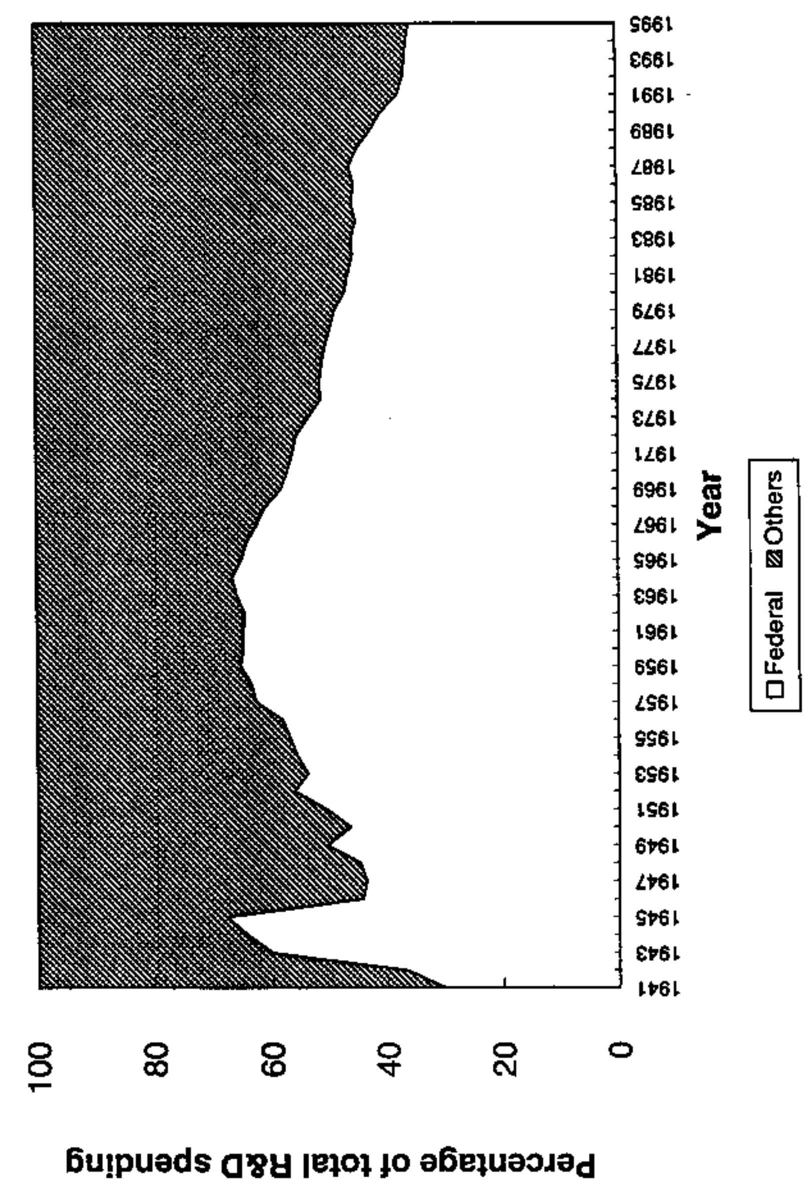

FIGURE I.2. National R&D Expenditures, by Sources of Funds (Federal vs. Others)
SOURCE: *The First Annual Report of the National Science Foundation: 1950–51*, p. 30, appendix 6 [estimated data] for 1941–1952, and *National Patterns of R&D Resources, 1995 Data Update*, table B-3, available at http://www.nsf.gov/sbe/srs/s2195/start.htm for 1953–1995.

Manhattan Project.[15] Curiously enough, this was combined with a strong reinforcement of the centrality of academic discipline as the arbiter of legitimacy of training and career success of the scientist. The vehicle for reconciling these seemingly contrary trends was the reconfigured postwar American university, where federal science policy created a situation in which teaching was now openly avowed to be complementary to research (but not in those massive introductory courses staffed by graduate students); where overhead costs on contracts helped fund the operating expenses and the graduate population; where the GI bill and the creation of the temporary occupation known as the "research assistantship" forged a whole new career trajectory for novice scientists and underwrote a massive expansion in graduate education. If some especially favored scientists still chafed under this shotgun marriage of teaching and research and disciplinary identity, a hybrid largely absent in most other developed economies, then a novel quasi-governmental entity known as the "think tank"—or more colloquially, a university campus without students—was created to cushion the irritation for the favored few.[16]

It would be a mistake, however, to see American science policy as focused solely or even primarily upon universities. Federal and military science policies also fostered what has been called a "stealth industrial policy" in the United States.[17] It was an ideological imperative in America that the government not be seen as favoring certain industries in contradiction to the marketplace, a ruse that Cold War security considerations substantially facilitated. Nevertheless, most federal R&D funding was channeled through private corporations, even at the peak of university support, skewing the direction of technological exploration in selected industries. Private industry (broadly defined) has always been far and away the largest performing sector of R&D in the United States, as revealed in table I.1.

Indeed, these figures understate the magnitude of federal subsidy of

15. See, for definition of "Big Science," Galison and Hevly 1992, Galison 1997, Heilbron and Seidel 1989.

16. The paradigm instance of the postwar think tank was the RAND Corporation, split off from the Douglas Aircraft Corporation and supported initially by Air Force funding. On the phenomenon of think tanks, see Stone, Denham, and Garnett 1998. The early history of RAND is covered in Smith 1966 and Collins 1998.

17. The notion that the United States did have a sub rosa version of industrial policy has gained increasing attention among political scientists and other policy analysts. See, for instance, Etzkowitz 1994; Markuson and Yudken 1992; Teske and Johnson 1994; Hart 1998a, 227–29.

TABLE I.1. National Expenditures for Total R&D, by Performer

Year	Total	Federal Govt.	Industry	U&C	U&C FFRDCs	Nonprofits
1953	5,124	1,010	3,630	255	121	108
1954	5,644	1,020	4,070	290	141	123
1955	6,172	905	4,640	312	180	135
1956	8,364	1,041	6,605	372	194	152
1957	9,775	1,220	7,731	410	240	174
1958	10,711	1,374	8,389	456	293	199
1959	12,357	1,639	9,618	526	338	236
1960	13,520	1,723	10,509	646	360	282
1961	14,320	1,878	10,908	763	410	361
1962	15,392	2,096	11,464	904	470	458
1963	17,059	2,279	12,630	1,081	530	539
1964	18,854	2,838	13,512	1,275	629	600
1965	20,044	3,093	14,185	1,474	629	663
1966	21,846	3,220	15,548	1,715	630	733
1967	23,146	3,396	16,385	1,921	673	771
1968	24,605	3,494	17,429	2,149	719	814
1969	25,629	3,501	18,308	2,225	725	870
1970	26,134	4,079	18,067	2,335	737	916
1971	26,676	4,228	18,320	2,500	716	912
1972	28,476	4,589	19,552	2,630	753	952
1973	30,718	4,762	21,249	2,884	817	1,006
1974	32,863	4,911	22,887	3,022	865	1,178
1975	35,213	5,354	24,187	3,409	987	1,276
1976	39,018	5,769	26,997	3,729	1,147	1,376
1977	42,783	6,012	29,825	4,067	1,384	1,495
1978	48,128	6,810	33,304	4,625	1,717	1,672
1979	54,939	7,418	38,226	5,366	1,935	1,994
1980	62,596	7,632	44,505	6,063	2,246	2,150
1981	71,869	8,426	51,810	6,847	2,486	2,300
1982	80,018	9,141	58,650	7,323	2,479	2,425
1983	89,143	10,582	65,268	7,881	2,737	2,675
1984	101,167	11,572	74,800	8,620	3,150	3,025
1985	113,818	12,945	84,239	9,686	3,523	3,425
1986	119,555	13,535	87,823	10,927	3,895	3,375
1987	125,376	13,413	92,155	12,152	4,206	3,450
1988	132,889	14,281	97,015	13,462	4,531	3,600
1989	140,981	15,121	102,055	14,975	4,730	4,100
1990	151,544	16,002	109,727	16,283	4,832	4,700
1991	160,096	15,238	116,952	17,577	5,079	5,250
1992	164,493	15,690	119,110	19,794	5,249	5,650
1993	165,849	16,556	118,334	19,911	5,298	5,750
1994	169,100	17,200	119,700	20,950	5,250	6,000
1995	171,000	16,700	121,400	21,600	5,300	6,000

NOTE: U&C = universities and colleges; FFRDCs = federally funded research and development centers. All the data are measured in current dollars.

SOURCE: *National Patterns of R&D Resources: 1994,* An SRS Special Report, NSF 95-304, Division of Science Resources Studies, National Science Foundation at http://www.nsf.gov/sbe/srs/s2194/dst1.htm for 1953–1991 and *S&EI 1996,* appendix p. 107, table 4-4 for 1992–1995.

corporate science, since a fair proportion of it occurred through tax rebates and third-party payments. (It is an artifact of the Cold War regime that there exist no dependable consolidated R&D accounts for the entire federal government, even down to the present. Things have, if anything, gotten worse with the decline of the Cold War regime, with the Department of Defense no longer providing detailed breakdowns of its own R&D spending by academic field after 1993.)

This industrial policy extended well beyond monetary grants and subsidies, however. In sharp contrast to the protoindustrial regime, the Cold War regime was characterized by a very weak legal structure of intellectual property protection combined with a very active antitrust posture. The net consequence of this mode of science organization was that many of the scientific and technological breakthroughs achieved by corporate labs such as Bell Labs, Xerox Parc, RCA Sarnoff, Merck Rahway, and IBM Yorktown were not adequately capitalized upon by their huge corporate sponsors, but were instead turned into downstream marketable commodities by small start-up firms, themselves often formed by fugitives from those very same corporate labs.[18] This "communal" approach to appropriation of the fruits of subsidized research was also encapsulated in the Department of Defense policy of a "second-source rule" for suppliers of high-tech weaponry and devices, duly sweetened by cost-plus contracts. This rather cavalier attitude toward technology transfer from the laboratory to the marketplace was one of the prime hallmarks of the Cold War regime, one that could trace its provenance to the looming presence of the military in science funding.

The mutual reinforcement of this stealth industrial policy and the postwar ideology of the "freedom" of the scientist is another phenomenon, like the existence of a stealth industrial policy and the structure of intellectual property, that we believe has not yet been adequately explored in the science studies literature. The scientists most heavily embroiled in military funding had to submit to the classification and clearance procedures of the state; in exchange, the state would promise not to micromanage their research agenda. Control, while not completely internalized, was certainly rendered unobtrusive; and the ability to ap-

18. One such famous instance, that of the transistor, is related by Riordan and Hoddeson 1997. The capture of Xerox Parc technology by Apple Computer is legendary in the business history literature. This aspect of industrial policy was pointed out by Mowery and Rosenberg (1998, 44). The interplay of antitrust and science policy is also stressed in Hart 1998b.

peal to freedom of expression was a critical component of the ideological rivalry of the period.[19] Since a major feature of the Cold War regime was the maintenance of ongoing university ties of scientists doing government-sponsored work, they were exhorted also to publish in the "open" literature to meet their disciplinary obligations and bolster their credentials if they exercised prudent discretion. (The existence of completely classified "scientific journals" stands as one of the more extreme anomalies of that era.) The "uses" of various discoveries therefore became more radically separated from their original elaborations (as well they might, given the rather imprecise concepts of intellectual property), especially in the formats in which they were disseminated.[20] The economics of these disciplinary outlets were themselves often obscured through such indirect devices as page charges, submission fees, and wildly inflated library subscription rates. Crudely, it became possible for nominal academic scientific stature to be denominated in terms of public intangibles like disciplinary "credit" or "eminence," all the while the money was being allocated according to somewhat different criteria. Even though science was being closely managed by research officers, at first in the wartime OSRD and after the war in the Office of Naval Research, DARPA (Defense Advanced Research Projects Agency, and other permutations), the Atomic Energy Commission, and elsewhere, the scientists eventually learned to come to terms with any residual sense that there might fester some conflict between their own freedom of inquiry and larger decisions to channel research in certain directions. If the exigencies of national security did not appear sufficiently compelling, the researcher could always take succor from the ethos of "pure science" within the academy. Indeed, this became the background to the public face of science policy enshrined in Vannevar Bush's famous 1945 mani-

19. Many in policy positions were quite adamant that it was the scientists who had to adapt to political reality. See Bureau of the Budget controller Harold Smith to Admiral Julius Furer, quoted in Owens 1994, 536: "The real difficulty, I think, has been that the physical scientists are worried about governmental controls largely because most of them—as they make clear to me by what they do and do not say—do not know even the first thing about the basic philosophy of democracy. . . . However, most of them have learned to accept government funds with ease, and I think they can adapt themselves to governmental organization with equal ease."

20. One of the major trends in recent science studies has been to seek to recover the military context of postwar scientific discoveries that have been treated as disembodied "pure" science. See, for instance, Forman 1987, Edwards 1996, Mendelsohn in Krige and Pestre 1997, Forman and Sanchez-Ron 1996, Kay 2000, Sent forthcoming, Mirowski forthcoming.

festo, *Science: The Endless Frontier,* namely, the "linear model" of "basic" science → "applied" science → "development" → production. The alert reader will have detected that, until now, we have not acknowledged the existence of any hard and fast distinction between basic vs. applied science and technology in this introduction, for reasons that should now become clear. We regard the endless fascination in science studies (and, as we shall shortly observe, economics) with boundary maintenance between "basic" and "applied" science to be itself an artifact of the Cold War regime (Kline 1995). This doctrine, so taken for granted within modern orthodox economics and much of orthodox science policy, was hardly even present in economic writings prior to World War II.

The Bush report has been analyzed repeatedly, and perhaps to excess, in the literature on the history of science policy. Our only concern here is to suggest that it played an important conceptual and ideological role in the Cold War regime, even if it did not end up serving as a blueprint for the actual structures of science funding and management that were eventually instituted in the United States in this period.[21] The idea that there was some necessary but unproductive form of scientific research that required state funding for its very existence, and that the economic growth of the nation would suffer in its absence, whereas applied R&D could be safely left to the corporate sector to organize, in conjunction with the previously described stealth industrial policy that had precipitated out of the immediate postwar political process, provided the ideal cover for the absence of accountability of military science planning. Although often pitched at a rarefied level of abstraction seemingly free of any parochial considerations, it has only become clearer in retrospect that it was a product of local conditions prevalent in the specific postwar regime in the United States, and in fact bore little relevance for science policy in other countries in the same time frame.[22]

21. This distinction will shortly play a role in our account of the evolution of successive "theories" of the economics of science. For instance, see the comment of Mowery and Rosenberg (1998, 31): "Anticipating subsequent economic analysis, Bush argued that basic research was the ultimate source of economic growth." It is now generally understood that Bush did *not* seek to have his OSRD experience extended into the postwar period because he believed in something much more closely resembling the "first" regime was superior; rather he was outmaneuvered on this issue. See, for instance, Zachary 1997, Reingold 1995, Hart 1998a. The eventual compromise on the shape of the National Science Foundation was itself heavily informed by military imperatives, focused upon the "open" science practiced in universities, and has always stood as a relatively small component in the overall federal approach to science policy. Because economists have neglected these points, their commentaries on the Bush report tend to misconstrue its significance. See, for instance, Holton 1998 and the papers in Barfield 1997.

2.3 The Globalized Privatization Regime

Whether it be the cataclysmic downsizing of physics in the last two decades, or sweeping changes in the rules of the game for academic entrepreneurship, or the radical restructuring of research universities, everyone now realizes to a greater or lesser degree that the Cold War structure of science management is rapidly going the way of the whalebone corset and the phonograph record. Tectonic shifts of science funding between various sciences, as indicated in table I.2, have been accompanied by drastic reorganization in the very structures of scientific funding and conduct. The papers collected in this volume attest to that fact, from varying angles and perspectives.

From many different vantage points, it should now become apparent that the Cold War regime of science policy could not have persisted over the longer term. The convenient fiction of a clear separation between "pure" and "applied" science could not be long maintained. There were simply too many internal contradictions and repressed economic considerations in what had initially seemed a politically viable set of compromises. First, it was inevitable that the heavy subsidies provided by the federal government to the universities and corporations would sooner or later have run into political and economic obstacles in the U.S. political culture. Most Americans had never really relinquished their suspicions concerning coddled intellectual elites (Hollinger 1995), and the trope of the danger of the self-interested and therefore untrustworthy scientific expert could easily be revived as a democratic plea for greater accountability in value for money. Furthermore, because the R&D budget of the federal and state governments was so widely dispersed among numerous agencies and programs, and so devoid of coordination and effective interest-group mobilization (with the possible exception here of biomedical research), they made an inviting target if and when budgetary stringencies would prompt belt-tightening measures. A high-profile example of this growing vulnerability came with the cancellation of the Supercon-

22. One wonders, for instance, how different the economic trajectory of postwar Japan might have been if the Japanese had heeded Bush's assertion that "[a] nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill" (Bush 1945, 19).

TABLE I.2. Federal R&D Obligations by Field, Basic Research

Year	Federal Total	National Total	% Physics	% Life Sciences	% Social Sciences	Federal/ National Total
1963	1,152	1,965	19.7	32.2	2.1	0.586
1970	1,926	3,567	17.6	36.19	3.32	0.54
1971	1,980	3,698	17.73	37.73	3.54	0.535
1972	2,187	3,829	16.55	39.69	3.66	0.571
1973	2,232	4,051	15.73	39.78	3.58	0.551
1974	2,388	4,439	15.08	43.22	3.14	0.538
1975	2,588	4,827	14.65	43.12	2.86	0.536
1976	2,767	5,291	14.02	44.16	3.14	0.523
1977	3,259	5,925	14.33	42.44	2.95	0.55
1978	3,699	6,841	14.03	42.93	3.35	0.541
1979	4,193	7,736	12.78	45.12	3.1	0.542
1980	4,674	8,651	14.29	43.95	3.15	0.54
1981	5,041	9,741	14.58	44.12	2.72	0.518
1982	5,482	10,658	14.43	46.08	2.19	0.514
1983	6,260	11,859	13.66	46.18	2.2	0.528
1984	7,067	13,176	13.03	46.52	1.88	0.536
1985	7,819	14,510	12.28	48.43	1.8	0.539
1986	8,153	16,885	12.3	47.33	1.39	0.483
1987	8,942	18,213	11.99	48.78	1.45	0.491
1988	9,474	19,381	12.73	47.52	1.55	0.489
1989	10,602	21,477	13.16	46.37	1.46	0.494
1990	11,286	22,556	13.06	45.88	1.28	0.5
1991	12,171	26,629	13.52	44.65	1.32	0.457
1992	12,490	27,044	12.87	46.77	1.12	0.462
1993	13,400	28,125	11.95	46.93	1.45	0.476
1994	13,523	28,934	11.11	47.86	1.36	0.467
1995	13,877	28,642	10.86	47.57	1.49	0.484
1996	14,464	29.574	10.69	47.56	1.47	0.489
1997	14,942	31,212	10.45	48.21	1.48	0.479
1998	15,862	NA	NA	48.47	1.51	NA
1999	16,914	NA	NA	49.2	1.57	NA

SOURCE: Table 35 in *Federal Funds Survey, Detailed Historical Tables, Fiscal Years 1951–99* at http://www.nsf.gov/sbe/srs/nsf99347/htmstart.htm and *S&EI 1998*, appendix p. A-125, table 4-7.

ducting Super Collider in October 1993, an event that marks the dethronement of physics as the unchallenged champion of the Cold War regime (Sarewitz 1996), even though, as shown in table I.2, physics as a funding priority had eroded even earlier.

The supposed immunity of the quotidian prosecution of science from economic considerations was further compromised when universities themselves stopped being perceived as otherworldly ivory towers re-

moved from politics and were caught in the unseemly act of trampling all over each other in competition to get federal research funds specifically earmarked to their campuses, lobbying representatives not on grounds of some abstract scientific peer review but rather for geographic or other political justifications (Brainard and Cordes 1999). Another doomed holdover from the Cold War regime was the extreme concentration of research funds in a small number of elite institutions—Bush's own MIT being the most favored recipient—a skewed distribution that could not persist once excluded universities realized that they could actively enter the political sweepstakes for federal funds and effectively challenge previously hidden old-boy networks. Indeed, peer review was one of the early casualties of the breakdown of the Cold War regime, since the insistence upon functioning internal standards of quality control could not begin to assuage external demands for responsibility, relevance, and accountability.[23]

Third, the Cold War premise of "science policy in one nation" could not continue to be maintained in a world that was growing less bipolar and more economically developed. Contacts between scientists across the Iron Curtain could be monitored in the name of military security; but as European and Asian firms regrouped and became economically significant, they conceived a desire to tap into the scientific and technological developments that had become such a prominent feature of postwar American prosperity. Since their own indigenous research infrastructures were so divergent from that found in the United States—frequently with one set of institutions dedicated purely to instruction, another different set for state-sponsored research, and a third set charged with state planning of industrial research—they initially had to send their most promising students to the United States to partake of the novel developments within the framework of the unfamiliar pedagogy. American universities were initially inclined to welcome the newcomers, but this had perverse unintended effects on American science policy. As the American research infrastructure grew so prodigiously up through the 1960s, it became apparent that the pool of indigenous candidates for scientific careers would not keep pace; and so foreign students were

23. Paul Forman's paper in this volume (chapter 2) takes up the discussion of the shift from self-generated and self-referential norms of excellence to appeals to social responsibility in modern science and relates it to larger cultural issues of postmodernism and the demise of the national security mindset. Shaun Hargreaves Heap discusses how the English version of accountability for state-funded universities assumed the format of "Research Assessment Exercises."

increasingly recruited into American academe and industrial research. Although wonderfully beneficial for American culture, this did tend to create problems for the prevalent political rationale for the government funding of higher education and its integration within the research system, which tended to be phrased in terms of investment in national human capital. It became increasingly difficult to justify the subsidized training of foreign students, many of whom would return to their home countries in order to staff the major economic and political competitors to the American system; and this does not even take into account the undercurrent of xenophobia undermining political support for government-subsidized education. As figure I.3 reveals, some disciplines began to be dominated by foreign nationals by the 1990s.

The net result was that nationalist pride and xenophobia were effectively undercut as rallying cries for American science policy from many different directions, and the emotional center of the national discourse about science congealed into a fear that convergence of other economies to U.S. standards and practices (inevitable to some extent in any case) was undermining American economic "competitiveness." Hence, Slaughter and Rhoades in this volume (chapter 1) refer to the complex of events characterizing the third regime as the rise of the "competitiveness R&D coalition."

The fourth countervailing tendency, materially the least gradual and therefore the most obvious in retrospect, was the utter collapse of the Soviet Union as the premier rival in the Cold War system. Because of its fragmented and partially classified nature, it had been previously impossible to gauge the extent of the importance of the national security imperative for the framework of science organization in America, at least until the fall of the Berlin Wall. Subsequently, many analysts have come to an increasingly nuanced appreciation of just how much the unquestioned assumption of an implacable technologically advanced enemy ramified throughout the practices and presuppositions of American science. The immediate fallout was the contraction of direct defense-related R&D after 1988 while nondefense expenditures failed to take up the slack, as shown in figure I.4.

However, shrinking federal research budgets were just the tip of the iceberg that had punctured the supposedly unsinkable vessel of Cold War scientific research.

The collapse of the national security imperative heralded an array of incursions upon universities; it has by now become clear that it had stood as their line of defense against many forms of encroachment originating

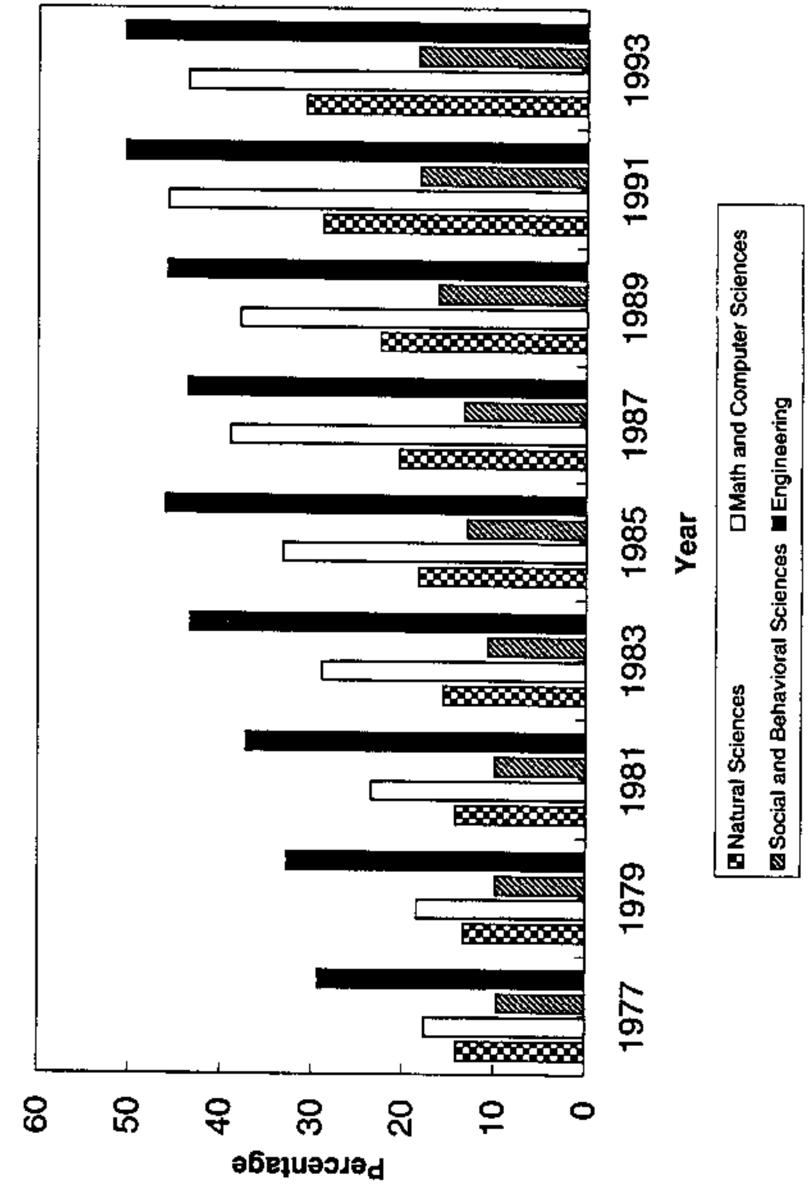

FIGURE I.3. Percentage of Science and Engineering Doctoral Degrees Awarded to Foreign Students
SOURCE: *S&EI 1996*, appendix pp. 58–59, table 2-29.

from disgruntled elements of their clientele. Soon after the fall of the Berlin Wall, universities came under attack for their lax accounting standards and supposed abuse of indirect cost provisions that had been forged back during World War II.[24] Various high-profile fraud accusations were lent some credence by hastily constituted government inquisitions; and periodic eruptions of litigation, such as that described in section 1 of this chapter, made it seem as though universities were not quite altogether capable of keeping their own houses in order. After years of quiescence, the quality and amount of teaching at major research universities came in for scathing criticism, as did the professorate in general.[25] Admittedly, there was some basis in fact for the upsurge of dissatisfaction, since the universities had reacted to the decline in federal subsidies as a signal that the combined teaching-and-research model was no longer being supported by the government and responded to their own fiscal crisis by resorting to "gypsies," part-time adjuncts and other improvised nontenured job categories of "unfaculty" as a means of maintaining their position in the research sweepstakes while simultaneously offering the accustomed broad array of coursework. But the irony of the attack on the professoriat was that it came just as the job description for that profession was being reengineered within the new regime.

As Paula Stephan and Sharon Levin argue in this volume (chapter 14), reconfigured career paths of laboratory scientists in particular have induced a more collaborative and hierarchical social structure of research. Growth areas of the university are no longer found in the traditional departmental structures, but rather in interdisciplinary research units, often constituted around a particular applied research topic and outside corporate funding. The phenomenon of "research parks" is merely the geographical manifestation of this disestablishmentarian movement. But as research increasingly assumes a privatized character, so too do the teaching functions of the university. A very important de-

24. This refers to the Dingell committee hearings in the House, beginning in 1991. The high-profile Stanford "scandal" is discussed in Guston and Keniston (1994, 177–93). The early history of indirect and overhead cost provisions can be found in Gruber 1995.

25. One can regularly find the following sorts of outbursts in mainstream outlets, such as the *New York Times Magazine*: "Nevertheless, says Charles Clotfelter, an economist at Duke University, 'higher education is the biggest service industry that hasn't gone through a substantial, gut-wrenching restructuring.' What might that mean? Start with research. Once you get past the top 20% of research being done, the rest is—how shall I put it?—idle, tenure-earning junk with little or no social value. Is the *Journal of Plankton Research* really indispensable? Cracking down on dubious research would go a long way toward making college affordable for the masses" (Miller 1999, 49).

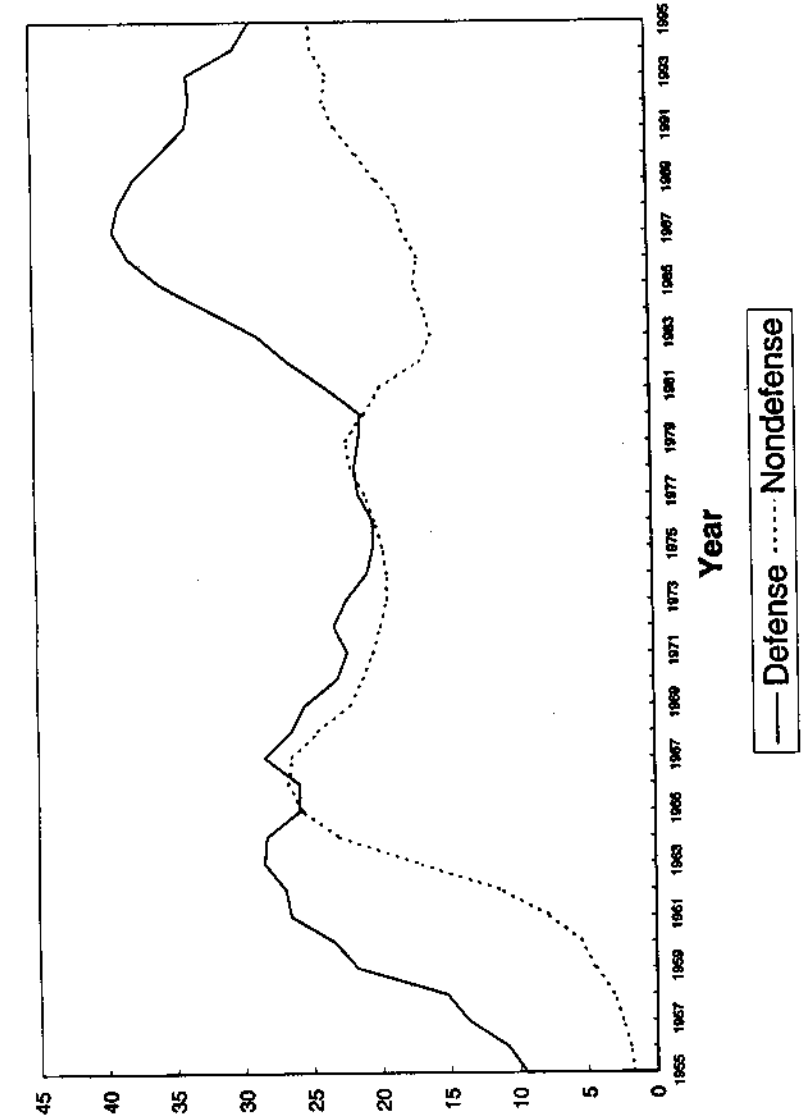

FIGURE I.4. Federal R&D Funding, Defense vs. Nondefense
SOURCE: Tables 25a–25g in *Statistical Tables on Federal Research and Development (R&D) Funding by Budget Function: Fiscal Years 1995–97 [Early Release Tables]* available at http://www.nsf.gov/sbe/srs/fbf95-97/budget.htm and GDP implicit price deflators: 1953–95 available at http://www.nsf.gov/sbe/srs/s2194/dst1.htm.

velopment is the increasing use of computers and the Internet to pursue the thoroughgoing automation of university teaching, under the colorless rubric of "distance learning" (Schiller 1999, chap. 4). Controversial issues of intellectual property, once only the concern of the research scientist, have thus now invaded the classroom, as argued here in the article by David Noble. The commoditization of the act of teaching has proceeded apace since his article, with firms such as Blackboard, IntraLearn, and WebCT supplying software to facilitate distribution of the courses outside of their universities of origin, and the Department of Defense chiming in with its own plan for standardization of Internet instruction, the "Advanced Distributed Learning Initiative" (Carr 2000). The alliance of new technologies and a new entrepreneurial culture has given rise to all sorts of automated teaching curricula subject to the privatization frenzy of IPOs and commercial takeover bids, which themselves serve to further fragment the university into profit centers and loss leaders.[26]

Academic career paths have themselves been downgraded, if not deskilled (as argued herein by Fuller), with the standard career in the laboratory sciences encompassing two or even three "postdoctoral" positions prior to attaining the assistant professorship that aspirants used to count on right out of graduate school under the previous regime. Since the very construct of "academic freedom" had itself been a key structural aspect of the Cold War regime, it was only a matter of time before the practice of granting academic tenure itself came under attack, and the academic freedom defense of self-directed inquiry grew to sound increasingly like an exorbitant luxury, if not a hollow catechism. Some universities sought to close down entire academic departments, and the sciences were not always exempted. Centrifugal forces began to separate out what federal largesse had held together for four decades.

Moreover, it was not only the universities that were experiencing an unanticipated gale of creative destruction. Many sectors of corporate America also initially felt blindsided by the collapse of the Cold War system. The industries that had most benefited from the stealth industrial policy were the first to feel the chill winds of restructuring. Close on the heels of the fall of the Berlin Wall, many of the corporations with the most illustrious in-house R&D units decided upon the withdrawal of military subsidy they could no longer afford such forward-looking subsidiaries and sought to either downsize the units or spin them off as free-

26. Updates on these issues can be found in Young 2000 as well as on the web in regular updates posted by *The Chronicle of Higher Education*.

standing firms.[27] The alternative model for R&D has increasingly been to seek collaborations where directed research can be outsourced from the corporation. It has been estimated that in 1997 roughly 10 percent of all U.S. companies outsource some portion of their R&D, and in research-intensive industries like pharmaceuticals, the proportion is more like 37 percent (Borchardt 2000). Some of these contract research organizations (CROs) benefiting from the R&D spin-off can be found in those research parks abutting American universities; but increasingly, CROs are to be found in areas like the former Soviet Union or Ireland, where labor costs are significantly lower and the temporary character of employment for research staff is much more accepted.

Not unexpectedly, the other salient aspects of the stealth industrial policy were also summarily reversed (Branscomb, Kodama, and Florida 1999). Antitrust policy veered in a much more lenient direction, while legal strictures on intellectual property rights grew much more stringent. Corporate legal staffs thrived while research units were downsized. In the new regime, the watchword was "privatization" of functions and entities that had previously enjoyed governmental subsidy; what this meant for corporate R&D is that it could be separated out as a modular profit center subject to thoroughgoing restructuring as it was subordinated to the competitive strategic position of the new multinational firm. The mutual interaction of the induced economic vulnerability of the universities with the novel corporate drive to reinvent contract research, itself the outcome of a change in federal government policy, has resulted in a new science policy for a global privatized economy of information.

As in all economic formations, there are golden opportunities and there are raging disasters. One opportunity has been for academic entrepreneurs to pursue these franchised research projects and to use them as leverage to change the governance structures of the structurally weakened university. Academic disciplinary boundaries, and even the budgeting and planning functions of deans and administrators, are eroded by what are essentially commercial deals being negotiated by laboratory heads and directors of research institutes for long-term funding. University-industry research parks are merely the latest topological manifestation, and part-time faculty CEOs a social innovation, of the structural makeover of the university as a site of privatized research ca-

27. Bell Labs was spun off from AT&T as Lucent Technologies in 1996. Other firms simply chose to severely curtail their in-house research units. See Uchitelle 1996 and Sweet 1993. Some corporate research directors discuss recent events in (Rosenbloom and Spencer 1996).

pacity.[28] But these novel sources of support of science are not an unmixed blessing. Not only do tangled commercial/academic contretemps like the Taborsky affair surface with increasing frequency; once the research function has been uniformly privatized throughout academia, it will become increasingly impossible to insist upon any residual academic control over the conduct of the research, as illustrated by the 1998 agreement between the University of California-Berkeley and Novartis, giving the latter an active veto in the research committee of the university's department of plant and microbial biology (Press and Washburn 2000). This loss of independence is already endemic in the lucrative area of clinical drug trials for pharmaceutical concerns. Drug companies, finding that clinical trials of new drugs both take too long and are too expensive when conducted at university hospitals, have turned to entrepreneurs who recruit general-practice physicians to themselves recruit their patients as clinical subjects and to conduct basic experimental protocols (Eichenwald and Kolata 1999). Beyond the thorny ethical issues raised by the perverted incentive structures in this system, it should stand as testimony to the fact that, if universities privatize research, then there is no guarantee they will long remain the low-cost provider of corporate research services. In an increasingly globalized setting, short-term research contracts can be as volatile as short-term capital flows.

3. Tracking the Economics of Science with Gun and Camera

Economists have frequently been caught in a bind when confronting the social phenomenon of scientific research. Their initial temptation has been to treat science as just another commodity, on a social and epistemic par with poetry and pushpin. This is the first reaction of anyone who asserts that science is just a special case of the greater "marketplace of ideas"; since the market is thought to allocate resources in an optimal manner, there is no need for anything as pretentious as an "economics of science." The free operation of "Open Science" and individual competition for the applause of peers is all that is required, and it follows that the idea of science policy is utterly otiose. This conviction, practically second nature for a neoclassical economist, has been a snare

28. For a survey of the development of university-corporate alliances, see Cohen et al. in Noll 1998 and Branscomb, Kodama, and Florida 1999. The prospects for further industry alliances are regarded from different perspectives in Rosenbloom and Spencer 1996. A critical approach restricted to the field of biotechnology is provided by Krimsky 1991, Blumenthal et al. 1996, and Thackray 1998.

and a pitfall for those who have turned their attentions to science.[29] Quite baldly, economists are not free to treat science like putty clay or pancakes. The reasons are fourfold: to begin, neoclassical economists have looked upon the natural sciences, and physics in particular, with something akin to envy and admiration, relative to their own debased status (Mirowski 1989), giving the lie to any such epistemic leveling at the level of practical discourse. Second, natural scientists rarely see their own world the same way, and bear such scorn for economists' aspirations to scientific status that they could never wholeheartedly acquiesce in an unvarnished "invisible hand" account of intellectual endeavor or indeed of the present predicament of the academy. Third, as the contribution by Wade Hands to this volume (chapter 19) explains so pithily, there lurks the problem of paradoxical self-reference whenever an economist evokes the marketplace of ideas, leading to interminable discussions of whether the "market for economics" is flawed or not; and finally, while neoclassical theory aspires to have much to say about desire, it really has no special resources to pronounce upon truth and cognition.

One consequence of this intolerable bind was that, up until World War II, economists had next to nothing to say about the social structure of science per se. True enough, they may have touched upon something called disembodied "knowledge" or market innovation here and there; but the vicissitudes of research organization and funding were deemed beyond their purview.[30] Interestingly enough, prior to World War II it was frequently outsiders to the field, such as Charles Babbage and Charles Sanders Peirce, who proffered essentially economic models of the social organization of science. The physical chemist and philosopher

29. Some examples: Kealey 1996; Wible 1998; Radnitzky 1987. A telling example of how this simplistic approach is rapidly brought up short in the real world can be found in Ehrenberg 1999, 104:

> One of the first things that economists teach undergraduate students is that relative prices matter and that as the relative price of something increases, one should substitute away from that commodity. Hence, it appeared quite obvious to me that, to the extent that the increased relative cost of the physical sciences and engineering is permanent . . . we should seriously consider reducing our investments in engineering and the physical sciences and redirect these saved resources to other areas. To even suggest this in the presence of faculty from those fields would have marked me as a very dangerous person in the administrative hierarchy.

30. The history of the profound shift from the definition of neoclassical economics as the allocation of scarce means to given ends to the organizing principle that economics should be concerned with the agent as information processor is covered in Mirowski forthcoming.

Michael Polanyi was one of the first to make a classical liberal case for the "Republic of Science," although his quest to link this to a market metaphor was a failure on his own account (see Mirowski 1997). These attempts to broach the possibility of an economics of science were scattered and ill focused and had little or no impact upon the actual practices and prognostications of economists, much less scientists themselves. We might point out that, at least in the United States, there was little if any demand for such an analysis, since during the protoindustrial era science funding was primarily subordinate to other corporate goals and the prospect of large-scale public support of science simply was not even on anyone's agenda. Scientists themselves were only embarking upon their process of academic professionalization, and their place in the academy was not yet secure. Economic growth was not linked to the provision of epistemic novelty by the organized concerted efforts of researchers in most schools of economics, with the exceptions of the work of Joseph Schumpeter and a few others in the 1930s; instead, growth tended to be attributed to surpluses arising from various "natural" endowments. Any way one looked at it, an economics of science was a commodity without a consumer, a manifesto without a constituency—a superfluous entity.

All that changed drastically with the advent of World War II and what we have called the Cold War regime. Not only were the funding structures and organization of the physical sciences utterly revamped; the very composition of the economics profession also was irreversibly transformed. One of us has argued in a number of articles that the defining moment of the American economic orthodoxy was World War II, from which point one can date the definitive supercession of the Institutionalist school of economics by the neoclassical school.[31] For the purposes of the present volume, the salient aspects of this history are these: that physicists and mathematicians themselves had innovated a novel set of techniques and tools for modeling issues of command, control, communications, and information transmission during the war under the general rubric of operations research (OR); that OR was in part an attempt to address issues of the funding and integration of scientific research and consultation into the military command structure, which constituted one of the defining features of World War II in America; that

31. This account starts with the narrative of the 1930s found in Hands and Mirowski 1998 and continues with the relationship of American neoclassical economics to war work and operations research in Mirowski 1999; the postwar period is covered in detail in Mirowski forthcoming. The watershed of the war was the topic of a number of papers collected in Morgan and Rutherford 1998.

various neoclassical economists were inducted into these techniques and tools in the course of their war work; that through their sustained OR ties in the postwar period, they were brought face to face with problems of technological change, military funding of R&D, and the construction of plans and strategies in the face of inadequate knowledge and uncertainty; and that the military alliance became a resource in the defeat of the Institutionalist school in the American university. This encounter of economists with military research regime would have far-reaching implications for the postwar profession, and also for the existence of an economics of science.

It may be one of the ironies of the story that we relate here that neoclassical economists, science policy scholars, and the sociologists of science may indeed have had more in common (at least in this regard) than they have been previously willing to admit. In stark outline, the distinctive formats of OR found in various cultural settings in World War II had quite a differential impact upon the way in which the study of science organization would be approached in the postwar period. In Britain, "operational research" was initially inseparable from the "Social Relations of Science" movement of J. D. Bernal, Patrick Blackett, and Solly Zuckerman (McGuckin 1984). There, due to their political location, science policy and planning were loosely wedded to a Marxian approach; thus it provoked the formation of an opposition movement called the Society for Freedom in Science spearheaded by Michael Polanyi, John Baker, and Friedrich von Hayek. This reaction growing out of the "socialist calculation controversy" provided the context for the early development of notions of markets as conveyors of information (rather than simpler allocation devices) associated with those latter figures. Some have asserted that the overt left-wing bias of operational research in Britain spelled its doom for science policy (Mendelsohn in Krige and Pestre 1997), but perhaps it is more correct to say that the contretemps over planning served merely to remove the subject from the explicit realm of orthodox academic economics, with it ending up instead split between a narrow technical specialization in OR as management consulting and a different branch of OR assuming the professional identity of a "sociology of science."

The situation in America was altogether different. There, largely under the tutelage of some key figures such as John von Neumann, Vannevar Bush, James Conant, and Warren Weaver, the military planning of science managed to evade any socialist connotations and therefore avoid political donnybrook; it proceeded to recast OR itself to encompass a number of analytical technologies never favored in Britain, such as game theory, computer simulation, linear and dynamic programming, and systems analysis. The last consisted primarily of projecting the future shape of various nonexistent weapons systems (hydrogen bombs, ICBMs, nuclear airplanes, military satellites), including their technological capabilities and strategic implications. Neoclassical economists, far from being revulsed by such procedures, were induced to become pivotal players in the nascent professionalization of OR in postwar America. It was out of the insistent controversies dogging postwar Pentagon procurement practices, and therefore OR and systems analysis, particularly at the RAND Corporation, that the first self-conscious theoretical papers in America on the "economics of science" were conceived, as has been deftly documented by the historian David Hounshell (2000). Thus, the Cold War regime of science funding not only bequeathed Americans a distinct and novel set of economic structures for the support of science, but it also identifiably nurtured a certain specific variant of economic doctrine to buttress and support the kinds of science policy it had produced. The Cold War left Britons with an altogether different sort of system of military funding of research, essentially decoupled from the universities (Edgerton 1996), and as a consequence of the original Bernalist social relations of science movement, it was blessed (or cursed?) with an academic sociology of scientific knowledge that kept relatively aloof from the military system of patronage. In Britain, the sociology of science therefore became the locus of science scholarship in the academic community, especially beginning with the Edinburgh school's refusal to abide a strict content/organization dichotomy; whereas in America, a chastened Mertonian sociology provided a species of non- or antieconomics of science, which refused to become embroiled in issues of scientific content and management, and thus discreetly declined to actively engage with issues of military planning in science policy. To this day, vibrant sociology of science speaks with a British (and even Continental) accent, whereas America became the preeminent bastion of an economics of science.

Still, once the gravamen of a serious economics of science was raised within American neoclassical economics, it had trouble becoming fully integrated into the core of the subject, and thus it suffered further trials and tribulations. Although the economics of technical change grew to be a quasi-respectable subfield of either growth theory or the theory of production functions, the "economics of science" tended to be relegated to an almost subterranean existence within the profession, at least until

the 1990s. One reason for this delayed development is that the sharp distinction between "basic" and "applied" science, which had become an unquestioned dogma in science policy and an indispensable component of the neoclassical approach in the immediate postwar period, essentially sanctioned the quarantine (and neglect) of the economic considerations impinging on basic science in deference to those economic forces supposedly governing the diffusion and rate of growth of a distinct technological change. Although we shall further trace the genesis of the basic/applied divide to the Cold War regime below, it should not pass unnoticed that the suppression of economic consideration of "science" as functionally subordinate to that of "technology" conveniently short-circuited all manner of thorny conceptual paradoxes inhibiting the development of a professionalized economics of science, as discussed by Hands and noted at the beginning of this section. In many ways, this rather convenient quarantine paralleled the similar artificial distinction forged within the philosophy of science at roughly the same juncture between the "context of discovery" and the "context of justification."[32] If the genesis of new ideas was treated as fundamentally ineffable, the stuff of genius and serendipity, then it would follow that the only phenomenon that could be subject to rational analysis was verification, quality control, and the diffusion of those ideas. "Knowledge" had to be both decontextualized and rationalized to be confined to the realm of the economic (and, we would insist, the philosophic) in the immediate postwar period, so as not to be mired in what was then deemed the "psychologistic fallacy" in the 1950s (Kusch 1995); and the way this was done was to insist upon its "thinglike" qualities, as distinct from issues of personal and social cognitive processes.

Yet even this evanescent debut of a neoclassical economics of science in America should not be regarded as a fait accompli in any sense. The very insubstantiality of its marginal existence within economics rendered it vulnerable to any number of sweeping revisions in microeconomic (or, to a lesser degree, macroeconomic) orthodoxy in the American economics profession. Since it had never really attained the status of an academic specialty, it was constantly thrust into the uneasy position of having to prove its bona fides by allying itself with various subsets of neoclassical theory, as well as maintaining some coherence with neoclassical conceptions of technological change. The profound rupture between the Cold

32. This comment is based upon some recent unpublished work on the history of the philosophy of science by Don Howard.

War and global privatization regimes of actual science organization since the 1980s has only further undermined any pretensions to strong continuity of analytical tradition. We contend that the shape of "the economics of science" was both at the mercy of specific science management structures prevalent in the American context *and* subordinate to the changing conceptions of the nature of theoretical orthodoxy within the American economics profession. Hence, our second task in this introduction is to try to make some sense of the postwar sequence of landmark papers in the economics of science anthologized herein by the analytical technique of relating them to (a) the evolving content of microeconomic orthodoxy and (b) the problems thrown up by the sequential appearance of two rather distinctive regimes of science organization prevalent in the postwar American context. Again, we need to stress that it is necessarily the American context that is most relevant to attaining an understanding these papers, since until very recently these papers were produced solely within the American profession.

Because there is no canonical history of postwar American economic orthodoxy to which we can direct the curious reader, we opt to divide the papers up according to what we view as three phases of "high theory" microeconomic concerns within the economics profession: (1) welfare economics and production theory as organized within Walrasian general equilibrium; (2) game theory and the cognitive turn in microeconomics; and (3) theories of computational complexity, networks, and bounded rationality. These have tended to recast the dominant image of science prevalent in the neoclassical community in each era as, respectively: (1*) the provision of a public good; (2*) presenting a problem of agency and incomplete contracts in a world of strategic uncertainty; and (3*) giving rise to an unintentional emergent order out of stochastic processes plus local interactions of cognitively flawed agents.

3.1 Science as a Problem of Production and Welfare Economics

The two papers in part II on science conceived as a production process are representative of the initial configuration of the economics of science during the Cold War regime in the postwar United States. Although they represent different idioms coexisting within neoclassical theory—Richard Nelson reflecting Samuelsonian welfare economics and Kenneth Arrow representing Cowles Commission Walrasian theory—both share the characteristic insistence upon science as producing a "thing" called knowledge, and all promote the central question of a formal economics of science as whether or not the "thing" produced by scientists qualifies as a public good and therefore as economically deserving of public subsidy. Because the entry point from which these theorists approached science was through the question of commodification, the analysis that grew out of these concerns tended to treat science itself as just another generic production process: Kenneth Arrow's favorite metaphor for research was, notably, that of mineral extraction or mining. Moreover, this tradition sought to erect a sharp distinction between science and technology, or pure and applied science, or basic and applied research. "Basic science" produced knowledge that was considered to be conveyed unidirectionally to "applied" or technological contexts, where it was portrayed as subject to further processing into conventional economic goods: this became known (perhaps with derogatory inflection) as the "linear model" of the relationship of science to the economy, as discussed above. By insisting upon the generic market as the general paradigm for all modern social organization, it sought to subsume science as just one special case of this generic social structure. Neoclassicism codified rational action; science was just thought to be the highest form and best instantiation of human rationality. As Nelson put it, with the usual 1950s gender oblivion, "men have always been, at least in a limited way, scientists."

It behooves us to resist this siren song of atemporal generality and instead be reminded of the ways in which this construct resonated with the initially precarious postwar situation of American Walrasian economics, as well as the Cold War funding regime. The Cowles Commission and its patron, the RAND Corporation, were at the time exploring the ways in which Walrasian theory and "decision theory" could be brought into line with OR and systems analysis as part of the military regime of science management. Both Arrow and Nelson were consultants at RAND when they wrote the papers included in this volume. The Cowles objective in the 1950s was to promote general equilibrium as an "institution-free" theory of social organization and furthermore to portray the rational agent as an "intuitive statistician," combining Neyman-Pearson statistical inference procedures with the maximization of utility (Mirowski forthcoming, chap. 5). Arrow, in particular, sought to combine decision making under uncertainty, information as a commodity, and the underfunding of science as a special case of market failure in the presence of uncertainty into one tidy package. Briefly, in an uncertain world, Arrow maintained that knowledge is effectively transformed into a commodity.[33] According to Arrow, various special characteristics of this commodity, such as uncertainty, indivisibilities, nonexcludability, and nonappropriability, dictated that it be recast as a particularly troublesome thing, a "public good."[34] The linear model implicitly came into play at this juncture, since the possibility of defining a metric of distance away from the more conventional production process (basic knowledge being used to "produce" applied knowledge being used to produce physical commodities) implied that basic knowledge would be "especially unlikely to be rewarded."[35] Next, conventional neoclassical welfare economics of the 1950s suggested that such basic knowledge would be underproduced relative to some virtual welfare maximum in the absence of public subsidy. This was a reprise of Arrow's favored argument that divergences from full Walrasian general equilibrium, dubbed (somewhat misleadingly) "market failures," were the true and only legitimate justifications for government interventions in the marketplace. The quest, then, was to achieve certain welfare goals by judicious government intervention in the research production process.

By this rather circuitous route, various neoclassical economists convinced themselves of what must have seemed a bald oxymoron to outsiders: "science" was really little different from any other human activity and thus best conceptualized as yet another market process, virtual or otherwise; and yet, simultaneously, science was also a problematic special case, a locus of endemic "market failures" that required lavish and persistent shoring up if it were to function on a credible scale. In a curious way, this paradoxical doctrine dovetailed with the other main development of the era, namely the rise of Keynesian macroeconomics. There, attempts to account for growth of national economies by quantification of conventional productive inputs had come a-cropper; one solution, proposed by Robert Solow, was to associate the "residual" with a species

33. Because this was a world purely abstract and institution free, with markets suspended in a void, no serious attention was given to property rights or cognitive structures, much less actual scientific practices.

34. Ontological freedom in defining the commodity to be anything one pleased, entirely disengaged from any empirical or logical considerations, was a Cowles practice codified in another key text, Gerard Debreu's *Theory of Value*.

35. The idea that there could be any sort of sequential ranking of the inputs of production in what was confessedly a thoroughgoing static model was just one of the ways in which the shotgun marriage of the process of research and Walras was unconsummated. One might indeed detect the faint echoes here of the Austrian tradition, which had been concerned with uncertainty and production in a manner antithetical to the Cowles approach.

of "technological progress" that shifted the production function upward relative to the same set of inputs. While we need not rehearse all the various controversies from the 1960s onward over growth accounting, the effect of this literature was to equate "technological change" with "spending on research and development" as a prerequisite for prognostications about measurement of the ways in which aggregate dollar amounts of spending on science and R&D generally would translate into macroeconomic improvement. This was the genesis of that all-time favorite statistic of the science policy wonk, the ratio of R&D spending to GNP or some other derivative macroeconomic aggregate. While not strictly conformable in all technical aspects, the public good/market failure microeconomic story resonated quite nicely with the Keynesian growth theory story, since both treated knowledge as a commodity whose economic impact could be conflated with the dollar amounts paid to vaguely demarcated activities dubbed "R&D" that supposedly eventually resulted in technological change on the shop floor. American Keynesians thought macroeconomic stabilization required increased government expenditure; American Walrasians thought scientific research required government subsidy; and both were united under the banner of correcting "market failures." As for those who thought this all a contemptible slide down the road to serfdom, such as various partisans of the Chicago school, they could still participate in the discussion by asserting the contrary proposition that the conventional market did adequately allocate knowledge as a commodity, or else that scientific knowledge did not actually exhibit the relevant attributes of a public good.

To understand the attractions of this position, we shall insist that one must venture beyond these relatively arid discussions between a few select economists and indicate how the broad policy prescriptions of "science as a production process" served to buttress the Cold War science regime. First and foremost, brief comments by both Nelson and Arrow reveal that the looming subtext of these debates was indeed the military reorganization of scientific research in the Cold War era.[36] But we need

36. "Much, though not all, of the government contribution to basic research is national defense–oriented." (Footnote in original: "This paper will not consider the vital question of whether the Department of Defense is spending enough on defense-oriented basic research.") Nelson 1959, 298, citation omitted; p. 153 herein. "The rapid growth of military research and development has led to a large-scale development of contractual relations between producers and a buyer of invention and research. The problems encountered in assuring efficiency here are the same as those that would be met if the government were to enter upon the financing of invention and research in civilian fields." Arrow 1962, 624; p. 179 herein.

not accept their protestations that there was no difference between civilian and military support of research; indeed, one way to comprehend their approach is to see it as a *justification* for the special arrangements that had grown up around the Cold War regime of science planning. The thrust of this analysis was to portray "basic" research as irreducibly opaque and uncertain and therefore requiring lavish government subsidy on welfare grounds, but simultaneously not being subjected to more conventional democratic structures of accountability that one might find in the more market-oriented "downstream" applications. In a sense, it ended up being the preferred economic counterpart to Vannevar Bush's manifesto *Science: The Endless Frontier* and simultaneously serving as an apologetics for the reigning division of labor between universities and industrial labs in the midst of the stealth industrial policy. There is no better brief summary of the postwar military's own view of its conduct of science management for defense purposes than this first incarnation of an economics of science in the papers of Arrow and Nelson.

The treatment of science as a commodity bore other indirect Cold War benefits. The analytical stress of this school upon the problematic aspects of knowledge as a commodity had a tenuous but nonetheless real relationship to the fact that intellectual property rights were *not* well defined or closely policed during the Cold War regime; this was not simply an inadvertent oversight, as we have discussed in the previous section. The characteristic trope of these postwar economists was to envision science as "nothing but" a market, and then sequester those problematic aspects of markets that did not seem to fit the characterization—here, the definition of the commodity—securely couched in some putative timeless predicament of human knowledge, rather than in some adventitious and transient social institutions linked to actual structures of scientific management and funding. This problem of the conceptualization of "ownership" in science as a function of historically identifiable institutions is the subject of the paper by Biagioli herein. Furthermore, the shift to macroeconomic aggregates of R&D spending relative to GNP as the central quantitative tools of the science policy analyst were skewed to make the U.S. economy look especially progressive given the gargantuan levels of American defense budgets, rather conveniently diverting attention from very real concern over whether all that weapons research really would eventually bear beneficial spillover effects for the civilian standard of living. The question of the magnitude of spillovers from military research projects festered as a low-grade controversy in science policy throughout the entire Cold War period. And finally, by

diverting all attention away from the actual social structures of contemporary scientific research (and especially the security clearances that many of our protagonists themselves had to undergo), the economists could readily chime in with the general populace in praising the freedom of Open Science in America, as a prelude to invidious comparison with regimented science behind the Iron Curtain. Treating science as just another production process tended to reinforce a simplistic "marketplace of ideas" gloss on what was effectively a disdain for scrutiny of the actual process of scientific research. Hence, the papers on science as a production process are Cold War artifacts through and through.

3.2 Science as a Matter of Implicit Contracts and Cognitive Information Processing

The intellectual image of science as the production of public goods for the common weal never has entirely faded away from economic discourse—indeed, one might venture that no concept in economics ever becomes so obsolete that it cannot with a little effort be recycled for another generation. However, it seems to us indispensable for science studies scholars to realize that, when they set out to attack the public good scenario in the economics of science, they are thrashing a horse on its last legs, if not already in the knacker's yard.[37] It might redound to the credit of American economists were they to admit to outsiders that their later models of an economics of science are not at all conformable to their earlier welfare economics–inspired images of knowledge as a commodity. It would take a book in itself to do simple justice to the transition from the "production" scenario of science prevalent through the 1970s to the "cognitive/contracts" scenario embodied in the papers on science conceived as a problem of information processing herein. The situation might seem further complicated by the fact that one of our exemplars—that of Charles Sanders Peirce, the great American outlier—dates from more than a century earlier. In the interests of brevity, we merely sketch some causal factors, which await the critical scrutiny of future historians.

Those familiar with postwar neoclassicism are well aware that the center of gravity of orthodoxy has shifted from the early touchstone of Walrasian general equilibrium to game theory, and in particular, Nash equilibria for noncooperative games (Rizvi 1994). Starting in the 1980s, strategic reasoning began to be deployed in applied areas of economics such as industrial organization and law and economics. We shall simply

37. We have in mind recent articles, such as Callon 1994 and Fuller 1991 and 2000a.

take this fact as an unexamined primitive in our subsequent arguments. A second premise is the utter demise of the Keynesian/neoclassical synthesis in macroeconomics. A third premise, perhaps less familiar than the first two, is a progressive shift of theory away from the static allocation of objects and toward increasingly complicated specifications of the cognitive capacities of the rational economic agent (Mirowski forthcoming; Sent 1998 and forthcoming). Under the aegis of the computer, a cognitive revolution has been sweeping many of the sciences; this has shown up in economics in the 1980s as a fascination with rationality as a strategic and epistemic problem for the utility model. Again, we shall merely take this observation as a given. What is interesting for our present purposes is that, under the banner of these three trends, it became less and less acceptable to treat knowledge as a simple thing[38] and thus as an unquestioned commodity; indeed, pursuant to the influence of the computer, one begins to notice a subtle distinction materializing in economics, where information could still be treated as a thing that is conveyed between parties, but knowledge was regarded as more akin to a state of epistemic virtue, which could be characterized only as the outcome of a process. This subtle transformation bears immediate relevance for the scaffolding of any neoclassical "economics of science," as the reader of the previous section will appreciate. If "science" no longer is regarded as straightforwardly producing a thing, but rather fostering the existence of a complex of cognitive states, then the onus of the economics of science is displaced from a fascination with technology and the "public good" welfare characteristics of the output and redirected toward questions of the optimal organization of the actual process of inquiry in the face of strategic uncertainty, with preference given to approaches constructing the analysis as a search for optimal contractual relationships. A fourth causal factor, to which we will return at the end of this section, is the structural transition out of the Cold War regime and into a global privatization regime, something that altered the original analytical imperative for justifications of the public subsidy of science. These four factors are the building blocks of our interpretation of the papers in our information processing section, and in particular the work of Paul David and Partha Dasgupta as representative of this "new" economics of science, and the work of Philip Kitcher as its rational choice analogue within the philosophy of science literature.

38. "Knowledge is not a homogeneous commodity. There are different kinds of knowledge and no obvious natural units in which they can be measured" (Dasgupta 1988, 2).

The economic approach that grew out of these developments, then, tended to treat science itself as a matter of individual information processing. As in the previous phase, markets continued to be seen as the general paradigm for all modern social organization, with science remaining as just one special case of this generic social structure; but now, in a twist upon the old chestnut of a marketplace of ideas, that subsumption was founded upon the idea that the neoclassical market model provided a general paradigm for information dissemination and computation in science. However, with this shift the implicit distinction between information and knowledge cited above then began to come into play. If science resulted in knowledge, then perhaps what really mattered was how research conjured up something less thinglike than "information"; various computer metaphors for cognitive processes were then brought into the modeling mix. Researchers developing this version of an economics of science were more than willing to acknowledge the functional importance of tacit knowledge, traditional practices, and less-than-explicit or -codified norms in the realm of scientific research, something that had also been stressed earlier in a different context by Michael Polanyi and Friedrich von Hayek in their denial of the very possibility of science planning, as well as in some precincts of the more recent sociology of science (Collins 1985). Indeed, we find in the excerpts of the paper by Paul David and Partha Dasgupta included in this volume (chapter 7) an identification of the distinction between tacit and codified knowledge with that between "pure" and "applied" science, and furthermore, a suggestion that the mix of the two in any particular circumstance is a function of such economic variables as transmission costs, differential reward structures, and the costs of codification.[39]

A signal characteristic of this version of an economics of science became the repudiation of the linear model of basic → applied science,[40] which often stands in as a surrogate for the repudiation of the entire previous public goods framework. The reason often given for this about-face is that the sequence of links between the original basic discovery and the final market commodity is too serpentine and indirect to under-

39. It may be important to note that in David and Foray 1995, Paul David appeared to retreat from the rather simplistic conflation of tacit/codified with pure/applied science. We tend to read this as a further retreat from the notion that it is the characteristics of the commodity/output that somehow determine the optimal funding structures of science.

40. "Everyone knows the linear model of innovation is dead" (Rosenberg 1994, 139). "The linear model has been repudiated by scholars of innovation" (Sarewitz 1996, 97). See also Brown 1998, 39–40.

write any backward imputation of valuation. What this admission does tend to accomplish is to open the door to the proposition that there exist other, more immediate goals or objectives of the individual scientist, objectives that should instead be inserted into the utilitarian cost-benefit calculation. Charles Sanders Peirce, that visionary eccentric, essentially pioneered this style of analysis in 1879, when he suggested modeling a major objective of scientists as the minimization of quantitative probable error. Although this sounds eerily similar to many versions of modern decision theory, those familiar with Peirce's elaborate philosophical architectonic will readily appreciate that he himself could never be confused with any species of neoclassical utilitarian. Indeed, the paper reprinted here was one of his numerous attempts to argue that his pragmatism suggested that science was a process of communal reduction of error, resulting in a convergence to truth in the long run. A modern reader will not be impressed with his rather arbitrary manipulation of utility and cost functions to arrive at his reassuring results; nevertheless, Peirce displayed canny insight at the end of this paper, when he admitted that the aggregate of scientists may have as their shared communal goal the ascertainment of truth, but that individual scientists may be motivated to seek personal distinction, and that "the economics of the problem are entirely different."

The modern school of the cognitive/contracts approach to the economics of science—the self-proclaimed "new economics of science" that adopts the language of the agent as information processor—takes this possible divergence between individual and social goals as its major point of departure. Scientific goals, once treated as unproblematic, now become the crux of the debate. One consequence of the spread of game theory in economics has been to elevate the hermeneutics of suspicion to an automatic analytical principle; its influence upon the economics of science has been to embolden economists (and, indeed, some philosophers) to entertain the notion that individual scientists may not all be the epistemic angels and selfless lovers of truth so glorified in earlier hagiographies of science; nevertheless, the tendency of this school has been to assert that *something* about the behavior of scientists or the social organization of science still manages to promote the goal of truth and predominantly preserve the time-honored virtues. It may be vaguely unsettling to watch economists preach that private vices produce public virtues in science, as Hands himself indicates, but we would be remiss if we did not mention that this particular trope has proved immensely

attractive to all manner of other intellectuals, especially in the context of the fin de siècle Science Wars.[41] Most immediately relevant for this volume, philosophers such as Philip Kitcher sought to reprimand their rivals in the science studies community who upheld the view that the nonepistemic goals of individual scientists, such as professional success or political aspirations, could divert the epistemic aims of science away from time-honored virtues such as the acquisition of truth. In response, philosophers of science started launching ripostes against perceived postmodernist extremists in the Science Wars by appropriating models from neoclassical economics, in order to assert their own versions of a "naturalized epistemology" and to account for the social structures of science.[42] More than once it was asserted that these game-theoretic models revealed how social structures did *not* matter ultimately for the successful attainment of the true or ultimate goals of science, in obvious parallel to some strains of economics that argued that alternative attributions of property rights (in the absence of pesky transactions costs) would not change efficient market outcomes. Individual rationality was deemed to triumph inevitably over myriad social obstacles. Thus it came to pass that one version of the economics of science was recruited as the first line of defense against the depredations of postmodern skepticism in the rather dreary reprise of the older "two cultures" controversy. What seemed to have been overlooked by both sides is the thesis suggested here by Paul Forman, that some aspects of postmodern skepticism could themselves have been regarded a function of changing structures of science organization and funding.

While neoclassical models may always be dragooned into performing ideological boundary work in many cultural controversies, our present concern is rather to understand the central tendencies of a particular set of theoretical developments, and then to relate them to contemporary changes in the structures of science policy and provisioning. To that end, we opt here to summarize the extensive writings of Paul David, undoubtedly the premier proponent of what has come to be called the Stanford

41. The so-called Science Wars, precipitated by a "hoax" by Alan Sokal that gained notoriety out of all proportion to its importance, has been dominated by those seeking to banish all manner of activities that they deem detrimental to the health of science. Representative works are Gross and Levitt 1994 and Koertge 1998. A somewhat more temperate account can be found in Hacking 1999.

42. On various versions of neoclassically socialized epistemology, see Bartley 1990; Goldman and Shaked 1991; Goldman 1999; Sent 1996; Kitcher 1990 and chapter 8, this volume; Mirowski 1996; Radnitzky 1987; Rescher 1989.

school of the economics of science.[43] David, following the footsteps of earlier economists, aspires to be regarded as a defender of "pure science" versus market-oriented research, but his initial quandary is that "the linear model is dead," or, in the early words of David and Dasgupta (1987, 542), "it seems less and less promising to separate research in science from that in technology on the basis of the characteristics of the knowledge generated by these activities." The response of David and Dasgupta is not to totally abandon the basic/applied distinction, but rather assert that it is socially created in pursuit of an optimal solution to the strategic problem of a divergence between private incentives and social welfare in the consequences of research. By contrast to the previous "production" approach, David and Dasgupta assert that knowledge can be differentially appropriable, but there persist principal-agent problems in controlling researchers.[44] Bluntly, incentives have to be restructured to force scientists both to disclose their discoveries with alacrity—that is, to turn private tacit "knowledge" into interpersonal fungible "information"—and to induce their peers to perform the vetting and validation functions that "scientific outsiders" would themselves find too time and resource intensive to conduct on a need-to-know basis. The purported solution to this problem is to foster something called "open science" according to a priority/credit system, and to sequester other scientific research subordinated to "technology" to be conducted along more secretive and proprietary pecuniary valuations. After the manner of the "new industrial organization" literature, David and Dasgupta proffer a prisoner's dilemma game setup and suggest the Pareto optimal solution constitutes the basis for a "new institutional economics": that is, somehow cultures in "the West" stumbled upon the one best way to structure science *for all time* so that it mutually reinforces the expansion of the market. Note well there is no facile reference to an efficient marketplace for ideas ab initio in David and Dasgupta: the optimal organization by their lights implies a putative *nonmarket* incentive system for

43. The Stanford school, loosely construed, encompasses the work of economic historians Paul David, Gavin Wright, Nathan Rosenberg, and their students such as David Mowery. Our summary of David's thematic is based upon David and Dasgupta (1987; chapter 7, this volume) as well as his papers (1993b, 1994, 1998a, 1998b).

44. There is a tendency of some scholars in science studies to attempt to appropriate this principal-agent literature and bend it to the purposes of a sociology of science. See, for instance, Guston 1999 and Turner (chapter 12, this volume). It might clarify matters if the sociologists became more acquainted with the sorts of presumptions built into these game-theoretic models so that they might better assess the extent to which they clash with conventional sociological preconceptions.

science, abutting a market system for more conventional commodities. The incongruous aspect of this characterization is that they cast the mix of market and nonmarket systems as a choice over a continuum: that is, the proportion of market to nonmarket institutions is somehow itself subject to the selfsame optimization calculation conventionally used in neoclassical economics to describe market operation. Hence, what is initially cast as a nonmarket process is actually surreptitiously given a market interpretation.

Although their recourse to noncooperative games is clearly a function of the shift of orthodox microtheory away from Walrasian general equilibrium in the profession at large, there remain many logical elisions that hobble the use of game theory in this particular context, both in the description and motivation of the existence of certain cognitive practices and social institutions, not the least of which are the standard presumptions of hyperrationality and common knowledge as the basis for the Nash equilibrium.[45] If scientists really ever did fit that characterization, it would be hard to understand most of their daytime behavior spent uncovering the secrets of the universe and attempting to persuade their colleagues that they had indeed found some of them out. There is also the nagging matter of what manner of superplayer is "choosing" the optimal mix of tacit and privatized science for the scientists themselves. (We have already mentioned the concession in David and Foray 1995 that the tacit/codified continuum cannot be readily mapped onto the public/privatized distinction, nor indeed that of disclosure/secrecy.)

While these critiques are undeniably salient for someone concerned with modeling strategies, what we would instead prefer to consider in this introduction is the manner in which David's other papers reveal the extent of his ambitions for the proposed "new economics of science." David is well aware that there are many other types of social analysis afoot in science studies and the history of science, and that these scholars would not placidly entertain simple stories of universal optimal science organization with equanimity.[46] Indeed, because he is nearly unique among economists in his appreciation for trends in the analysis of science

45. Further critiques of the deployment of noncooperative game theory to discuss cognitive and social formations can be found in Hargreaves Heap and Varoufakis 1995; Rizvi 1997; and Mirowski forthcoming.

46. "Subscribers to the theory of rational utility-maximizing behavior will want to know what induces researchers to seek admission to these 'clubs'; and conform by and large to club rules, by sharing knowledge they have not yet divulged" (David 1998b, 128). This argument was formalized in Mirowski and Sklivas 1991. One of the better critiques of David's approach located within science studies can be found in MacKenzie 1996, chap. 3.

outside of economics proper, David's later papers often provide the best critique of his earlier work. Furthermore, in his role as an economic historian, David also appreciates that the model inscribed in the David and Dasgupta paper is excessively static and abstracted from any historical specificity, tending to belie any claims that it is capable of explaining real institutions and social structures of science. To ameliorate those perceived drawbacks, David has proposed a "stage theory" interpretation of the rise of modern science (1991; 1998a). There he argues that there was a premodern stage in which secrecy and suspicion rendered almost all knowledge "tacit" and precluded anything like a cumulative process of scientific inquiry. He then posits the existence of a transitional stage, which he associates with the historiography of the Scientific Revolution, where aristocrats bestowed patronage on savants as another form of conspicuous consumption. The problem confronting patrons with the rise of the new learning, and especially the importance of mathematical expression, was that they themselves were incapable of judging the excellence of their house savants. David regards this as an instance of the problems of information asymmetries and principal-agent problems found in the modern industrial organization literature. The solution, presaged in the David and Dasgupta paper, was to allow the savants to judge each other according to reputation and socially attributed priority claims thrashed out in an open forum, with the patrons essentially excluded from evaluation but persisting in their willingness to pay the bills for the pleasure of their company.[47]

In our view, one way to understand the perspective from Stanford on the economics of science is to realize that they envision two essentially incommensurate systems of organization and valuation coexisting side-by-side in the modern world of scientific research. The first posited social structure they envision is a highly idealized invisible college of scholars who operate purely according to their own whims and inclinations, whose stature rests entirely upon disciplinary reputation and intellectual credibility, and whose evaluations of the quality of research are so tacit and maintained in such multilateral conformity by the relevant reference

47. David admits (1991) the inspiration for this story derives from Michael Spence's model of signaling and screening. In that paper, he also rejects the thesis found in Biagioli 1993 that patronage *produced* credibility in a savant as much as it was conditional upon prior credibility. David's screening model would indeed fail if the act of funding polluted the supposedly separate and independent value nexus of professionally awarded credit. This illustrates just one way in which David's work is frequently pitted in unacknowledged opposition against the themes found in modern science studies.

group that the actual process of producing warranted knowledge can largely be left out of the picture. As David has written (1998b, 120), it was

> not surprising that the "new economics of science" found it most natural to start by reworking the area of organizational analysis originally ploughed by Mertonian sociology of science, looking at the implications of certain institutional arrangements for allocative efficiency in the production of generic information . . . but not troubling itself over issues of socio-cognitive interaction that have occupied the sociology of scientific knowledge.

The second posited formation envisioned by the Stanford school is the everyday corporate reality of proprietary information and market-driven research, where the coin of the realm is not scientific fame but cold hard cash and success is denominated in tangible products and patents. Paul David and the Stanford school insist that we all need both, although their justification for this insistence is largely external to their models themselves. A world consisting solely of corporate science would be one where novel results were diffused much more slowly, they insist; because David denies that it would affect the *content* of science, speed of conveyance of discovery is the only operant variable. Since this seems a rather trivial hook upon which to hang their entire argument for the necessity of the persistence of academic science, David and Dasgupta also raise the possibility that a world of uniform corporate science would forgo the training and screening externalities that are generated from within the academic sector: in other words, corporate patrons face the same difficulties of asymmetric information and selection as their Enlightenment aristocratic predecessors, and elite universities (like Stanford) are better at recruiting and choosing and socializing candidates into the norms of science than would be corporate research managers. Hence, the bottom line of their prognostications is that (at least some) university research should be subsidized with no strings attached.[48] It is

48. David and Dasgupta themselves signal in a few places that this argument might be countered by the suggestion that tyro scientists not be subsidized in the academic sector, but rather borrow against their anticipated future earnings in their corporate careers to fund their education, and then let the corporate sector "cherry-pick" the more successful academic scientists in midcareer. This would recapitulate the usual Chicago deconstruction of liberal public goods arguments by simply redistributing the property rights and appealing to perfect capital markets. While they evince some discomfort with this option, they realize it clearly has some relevance as a description of what we earlier called the globalized privatization regime.

not science *as such* that requires public subsidy in their model, but rather certain elite universities in their role of providing scientific screening and education services to the larger society.

There can be no doubt that the doctrines of the Stanford school, as well as many other game-theoretic analyses of science, have been provoked by experience with the end of the Cold War regime and the onset of global privatization. These authors tend to position themselves as defenders of the university from various commercial and political onslaughts, such as those we have described above, while trying to use modern economics to locate the correct or efficient boundaries between public and privatized science. Yet, because of their desire to maintain conceptual continuities with the previous neoclassical tradition, the thrust of their program is backward looking, seeking to preserve a relationship between the postwar university and the corporate sector that is rapidly disappearing. But this strategy is fundamentally unavailing: by ignoring almost everything that was characteristic of science funding and management in the Cold War regime, in the final analysis it bears little relevance to the ultimate dissolution of that regime. Attenuated intellectual property, the stealth industrial policy, the military imperative (incidentally unmistakably present at Stanford: see Lowen 1997), the academic separation from commercial funding, the fragile construct of the teaching-and-research career path: all this and more is entirely absent from their model. Indeed, one might aver that their game-theoretic models could equally stand as a brief for the utter liquidation of the American mass-education university system, one where most scientific research is spun off into corporate or quasi-corporate settings, and most teaching outside of a few high-priced ivies became externally privatized as computerized distance education. Not only is there no attempt to analyze the profound changes taking place in intellectual property, industrial policy, university governance, the relative standing of the individual sciences, and the utter devaluation of the teaching-and-research career path; rather awkwardly, there is no serious consideration of any conditions that might demarcate science from any sort of generic "learning" by a generic rational actor. It is not an exaggeration to suggest that the only thing that renders someone a scientist in their model is possession of a certain configuration of risk and time preferences.

Furthermore, whereas both Paul David and Philip Kitcher acknowledge the existence of rival analyses of the operation of modern science, especially those emanating out of the Social Studies of Knowledge movement (SSK), science policy centers and science studies units, it seems

apparent to us that they assert their equilibrium characterizations of their abstract university ideal of circa 1960 as prophylactics against those fine-grained studies of the impact of funding and organization upon scientific process and content, such as those that we summarized in the previous section of this introduction. "Credit" is the gaping black hole at the heart of their models, a cognitive imponderable that supposedly sports many of the interpersonal attributes of money without actually capitulating to Mammon. It seems here that, for all the mathematical sophistication, we have not progressed all that much from the original quasi-market account found in Polanyi's paper "The Republic of Science." David and Kitcher, it seems, are resistant to acknowledging, or perhaps oblivious to, the raft of research in the sociology of science that demonstrates that the categories of "priority" and "discovery" are frequently agonistic with outcomes that bear a tenuous relationship to true responsibility and are rarely independent of the economic and social status of the claimant (Brannigan 1981); they have missed the commonplace notion in SSK that complete disclosure has *never* been the rule in "open science" throughout its history;[49] and it goes without saying that no agent's full panoply of rationality in their models is ever transformed by their experience as a scientist. Because they start with a neoclassical agent purportedly capable of solving any problem, their appeals to "tacit" or local knowledge represent little more than artificially induced frictions superimposed upon a global optimization carried out by hyperrational individuals: it has no recognizable connection with any modern developments in cognitive science or social epistemology. Michael Polanyi, who worried much more about the significance of tacit knowledge in science, concluded that serious consideration of the tacit dimension would conflict with such an imperious utilitarianism.

From our perspective, this "implicit contracts" version of an economics of science has all the drawbacks of inertia often attributed to old generals: far too absorbed with fighting the last war, to the neglect and detriment of the present war. While it is undoubtedly driven by the dual motivations of defending the Cold War university structure and appearing to keep up to date with modern changes in microeconomics, so far it has had very little of substance to contribute to the pressing problems of understanding the current regime of globalized privatization of science.

49. The classic text in this literature is Collins 1985; for a game-theoretic model that states this in terms economists might appreciate, see Mirowski and Sklivas 1991.

3.3 Science as the Outcome of an Interactive Network of Cognitively Challenged Agents

We would be less than candid if we concluded our survey of the economics of science on such an unremittingly downbeat note. There are daunting challenges facing scientists in the twenty-first century; but fortunately there are more than a few scholars who realize this, and the papers in the part of this volume on science conceived as a network represent the efforts of those who seek to rethink the entire "marketplace of ideas" approach. Of course, these forays are not so closely tied to the fortunes of the microeconomic orthodoxy in America as were the previous two versions of an economics of science, and therefore it is not so easy to lump them all together under some shared rubric. However, it does seem that science studies scholars located outside of the economics profession proper are especially concerned nowadays to interrogate the market metaphor and seek to uncover whatever may indeed be apposite and illuminating about the ideas contained in the contemporary economists' rucksack. Each representative of this modern trend, in his own fashion, subjects different aspects of the orthodoxy to an external audit: Michel Callon the industrial organization aspects of university-firm alliances; John Ziman the metaphor of scientific credit as market exchange; and Steve Turner the principal-agent relationship between the patron and the scientist. It will become apparent to the reader that each of these contributors approaches the favored tropes of the neoclassical economist with some measure of hesitation and distrust, but nevertheless ends up extracting some positive lessons for the globalized privatization regime.

However, it appears to us that there are also indications of a larger conceptual drift in the ways in which an economics of science will be conceived of in the near future, a shift that began to surface at the New Economics of Science conference at Notre Dame in March 1997, the conference at which the papers in this section were first presented. What these writers all seemed to share was a conviction that science should not be approached as some species of completed timeless entity, like some triumphant realization of an apotheosis of self-sufficient rationality and Pareto optimality. Rather, many writers are presently engaged in a quest for a description of something a little more nearly in the process of becoming; something comprised of individual scientists equipped with cognitive capacities falling well short of the superhuman neoclassical agents engaged in solipsistic activities; something whose telos is not given in advance, but itself coevolving with circumambient economic and political structures. These writers are often attracted to notions such as path dependence, spontaneous emergence of order, and evolutionary epistemology.

The mode of expression of this incipient novel trend among both the economists and the science studies community has been to cast their models in terms of networks of fallible, even cognitively limited agents engaged in some communal endeavor or quest. One observes this tendency in Bruno Latour and Michel Callon's advocacy of what they call "actor-network" theory (Latour 1987), as well as in the paper by Steven Durlauf and William Brock reprinted herein. (See also David 1998b.) What is heartening about this seeming convergence of approaches advocated within science studies and those in economics is that the turn toward the treatment of substantive rival scientific theories in all their diversity as a fitting topic for the social characterization of science, something notably absent in the prior two traditions of an economics of science. Plainly, if there were no rivalry and dissension in science, and there persisted no multiple paths to exploring nature and society, then there would be no problem of knowing what sorts of research to support and prioritize. For this reason alone, network theories are a welcome departure for an economics of science. Furthermore, the explicit recognition that scientists are frequently no better at prodigious baroque calculations of self-interest than are the superhuman caricatures of rational expectations theory could only encourage recourse to serious study of how scientists actually make decisions concerning both their careers and their research methodologies (Shadish and Fuller 1994). Perhaps this development might even betoken an appreciation for the fact that solutions to the problems of knowledge and discovery are often first posed as innovations in structures of social order. However salutary these consequences, we believe that this nascent modeling trend still has some distance to go before it has demonstrated the capacity to illuminate the problems and challenges of the third regime of globalized privatized science.

There is a large literature of critique of the actor-network framework; we cannot deal here with its possible ties to science policy.[50] Here we opt to concentrate instead on the Brock and Durlauf model as the network approach cast in a recognizably economic idiom. In this paper the primary concern seems to be the issue of whether "social" considerations, here a simple metric of conformity to the opinion of peers, can somehow

50. See McClellan 1996; Fuller 2000b, 365–72; Lee and Brown 1994; Law and Hassard 1999.

come to dominate the search for "truth," also modeled as a unique scalar metric. Clearly the "individuals" in this model do not have the capacity to mimic the full-blown rationality of the neoclassical model, and therefore simplistic welfare notions are much attenuated. Yet the idea enshrined in the mathematics that there exists some unique ranking of the validity of scientific theories and, more implausibly, that this ranking is freely available to all participants displays an unwillingness to seriously entertain the most significant work in the history and philosophy of science of the last four decades. Nevertheless, the overriding intention of Brock and Durlauf is to develop a simple model by which scientists at specific locations sample the opinions of their nearby neighbors and then examine the resulting statistical dynamics of the population in its acceptance or rejection of theories.

It should be noted that there is one curious aspect of the model that itself raises larger philosophical issues about the aims and procedures of future economics of science. Although it is not admitted in this paper, the model proposed therein is a slightly modified version of a model found in statistical mechanics, that of Ising spins and phase transitions using mean field techniques (see Weisbuch 1991, chap. 8). Without going into details, brief familiarity with the physics will reveal that most of the specific assumptions of the model, ranging from the binary character of the "theory choice" to the probability measures attributed to the utility functions are artifacts of the fine points of a minimum energy condition of a standard ferromagnet. As in so many other cases in the history of neoclassical economics (Mirowski 1989), the issue will not be so much to challenge the exact isomorphism between the physical model and the social phenomenon (do you sometimes feel you are trapped in a magnet?), but rather to ask, Why is this metaphor deemed compelling or meaningful? For instance: Is it more likely that your median modern solid-state physicist will embrace a formal model in an economics of science if he is being compared to a molecule in a magnet? Do natural scientists prefer to apply their own parochial disciplinary models to their own personal experience to assist in their understanding of how science works? Will physicists tend to gravitate toward physical models of the scientific enterprise, geologists to metaphors of continental drift, biologists to more concertedly evolutionary models (Hull 1988)? And if that is the case, then isn't the role of the economist rather an attenuated one of merely cheering on the project of metaphorical transfer but ultimately desisting from proposing anything that is really more intrinsically "economic" in scope? Are economists to be confined to providing a thin "economic" veneer for what is more correctly regarded as small communities projecting their parochial understandings of Nature onto Society?

It would seem that in the case of Brock and Durlauf, that is essentially what has happened. For where, in the final analysis, is the "economics" content in their economics of science? They begin by suggesting that they will evaluate the role of the "social" in actual theory choice in science; but by the end of the paper they admit, "[W]e are skeptical that there is any generic empirical regularity to be found in the role of social factors in the evolution of scientific theories, rather careful case-by-case historical studies need to be conducted." But then, what functions does this model perform for us? Indeed, there is nothing even remotely "economic" about their portrayal of their limited scientific agents: nothing about funding, nor intellectual property, nor the structure of pedagogy, nor the relationship of ideas to fungible technologies or devices. In the article by Turner, we at least confront directly the problems faced by those outside the narrow specialist research community in trying to form an arm's-length opinion about the validity and worth of a research program, which includes practices akin to "bonding" that facilitate such decisions; whereas in Brock and Durlauf, scientific opinions are formed by a simple weighted sampling procedure, already familiar from an earlier vintage of mechanistic decision theory.

From a more historically sensitive vantage point, it would seem that the problem of acceptance or dissent facing the scientists cannot be so readily expressed in a decision-theoretic framework, even one where "rationality" appears much less hubristic. Don Lavoie has sagely observed that

> market participants are not and should not be price takers any more than scientists should be theory takers. In both cases a background of unquestioned prices or theories is relied upon by the entrepreneur or scientist, but the focus of the activity is on disagreeing with certain market prices or scientific theories. (Lavoie 1985, 83)

This warning should serve as a prophylactic against confusing theory acceptance with either market purchases or statistical sampling procedures. Unlike the Brock and Durlauf model, this mode of inquiry would not aim to explain how agents all individually came to coordinate and agree on the content of "good science"; rather, it would explore how social organizations (such as the NSF, or the university department, or the professional academic society) channel irreducible miscommunication, noise, and dissent into manageable error and (sometimes) spontaneous order. Network metaphors often conjure up visions of computer hookups; but perhaps a better heuristic would be provided by systems configured to reconcile discordant signals in a system of accounts.[51]

Whatever one thinks of the specific network models of science, we would be hard-pressed to point to ways in which they have yet to be deployed to address the shattering implications of the current regime of globalized privatized science. Indeed, one of the most repeated complaints about Latourian actor-network analysis is that it leaves everything pretty much just the way it found it, *pace* the protests of the actual author Bruno Latour (1999). While some might welcome an economics of low ambitions and even lesser achievements, we believe the future lies instead in an economics of science that takes as its mandate dogged confrontation of the big questions facing science in the new century. These would include: What will happen to the university once research and teaching are spun off as separate privatized self-contained endeavors? When most research is being carried out under the auspices of some corporate agency or funding agreement, how will people come to factor that into their own assessment of the validity or dubiousness of the published account? Once scientific journals all get transformed into electronic archives, what will that do to the structure of the professional organizations whose raisons d'être were the care and maintenance of said erstwhile journals? When certain scientific specialties such as subatomic particle physics or algebraic topology (or the history of economic thought!) fail to find their corporate patrons, what will happen to them? Can universities continue to support these sad homeless creatures just off the revenues from their past patents, or will the successful sciences demand these funds as their rightful tribute? Indeed, can reengineered universities survive as the repositories for everything that corporations have considered and found useless? What is the role of various governmental funding agencies such as the National Science Foundation or the National Institutes of Health in the chastened circumstances of the new globalized privatized regime?

While the history we have proffered in this introduction does not give grounds for boundless confidence, we do feel justified in closing with two observations. First, economists will eventually come round to the idea that they will have to discuss the social structures of science as they

51. For an example of a formal network model of reconciliation of measurement of interdependent physical constants with error that illustrates this approach, see Mirowski 1996.

really exist and not how they would like to think of them in some misty monastic academic ideal. And second, the models that will shape the discourse will necessarily have to be cast into the idioms prevalent within the avant garde of the theoretical wing of the economics profession if they are to attract widespread comment. The days of science portrayed as a black-boxed "marketplace of ideas" are over.

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PART I

Science at the Turn of the Millennium